



The 6th International Conference on the History of Chemistry

# NEIGHBOURS AND TERRITORIES THE EVOLVING IDENTITY OF CHEMISTRY



## PROCEEDINGS

José Ramón Bertomeu-Sánchez, Duncan Thorburn Burns,  
Brigitte Van Tiggelen (Editors)

THE 6TH INTERNATIONAL CONFERENCE ON THE HISTORY OF CHEMISTRY

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*Neighbours and Territories: The Evolving Identity of Chemistry*



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## INTRODUCTION

Throughout its history, chemistry has been shifting ground between different territories. From its roots in artisan technologies, pharmaceutical workshops and alchemistic philosophy, it has developed into an archetypical laboratory science of the eighteenth and nineteenth century, ultimately claiming a full academic status. Chemists have invaded many new fields, from agriculture and industry, to medicine, public hygiene and pharmacology. In the twentieth century, chemistry has contributed to the major scientific developments in molecular biology, quantum mechanics, environmental science and nanotechnology. Chemists also gained key positions in the oil, plastics and pharmaceutical industries. This broad and continuous adaptation of the discipline to various fields of endeavour has brought chemistry into close contact with neighbouring disciplines and to social pressures. Time and again, chemists have needed to carve out their own territory, to negotiate with other specialists, and to gain particular expertise in widely divergent fields.

The disciplinary changes of chemistry had been remarked upon by historians since the nineteenth-century. One of the most famous historians of chemistry, Hermann Kopp (1817-1892), regarded change in purpose, methods and tasks as a distinctive feature of chemistry and he wondered how was possible to write a disciplinary history of such a changing territory.<sup>1</sup> Would he have been surprised by the dramatic twentieth-century metamorphosis, nobody can say for sure, but the issue of disciplinary changes has been in the mind of many succeeding historians of chemistry.<sup>2</sup> More recently, it has become the leading thread in a very popular history of chemistry written by Bernadette Bensaude-Vincent and Isabelle Stengers. The authors pointed out that chemistry “has always been heir to a heterogeneous territory, one that defied all *a priori* definition and therefore challenged chemists to construct an identity for it”. Chemical concepts, objects and methods form “nodes or crossroads among heterogeneous areas on the map of knowledge”, thus blurring the boundaries between chemistry and other scientific disciplines.<sup>3</sup>

The meeting was intended to explore the changing territory of chemistry and the relationships with its neighbouring disciplines. 112 participants gathered in Erasmushuis at the University of Leuven, Belgium, at the end of August (28 August-1 September, 2007) for the 6<sup>th</sup> International Conference on the History of

Chemistry (6ICHC) organized by the Working Party (WP) on History of Chemistry of the European Association for Chemical and Molecular Sciences (EuCheMS). The first such conference was organized in Hungary in 1991, since then the WP has fostered the creation of what is now a well connected community that meets every two years. Previous conferences organized by the Working Party were in Budapest, September 2003, “Communication in chemistry in Europe” and in Lisbon, September 2005, “Chemistry, Technology and Society”. The 2007 meeting was entitled “Neighbours and Territories: the Evolving Identity of Chemistry”.

This conference lived well up to expectations based on experience of earlier ICHCs, in content, ambience, mix of participant’s backgrounds, warmth of welcome and in the ensuing social programme and interactions. This was made possible thanks to the active involvement of many people and institutions, each at their own level.

The meeting was organized by the Belgian and Dutch Chemical Societies: Koninklijke Vlaamse Chemische Vereniging (KVCV), Société Royale de Chimie (SRC) and Koninklijke Nederlandse Chemische Vereeniging (KNCV). This joint collaboration was manifested through the members of the local committee, “local” meaning a Belgian-Dutch team superseding national and linguistic boundaries and led by Dr. Brigitte Van Tiggelen (University of Louvain, Louvain-la-neuve and Mémosciences).<sup>4</sup> The LC was helped by a staff of students and volunteers : Mathilde Urbain and Benjamin Palmaerts serving at the registration desk, and Tom Mortier (Katholieke Hogeschool Leuven) and Pieter Thyssen (Katholieke Universiteit te Leuven), operating the computers to ensure smooth Powerpoint presentations.

But organizing a meeting requires more than just a couple of well intended LC members and staff. A programme was needed, and this was done by a very efficient Scientific Committee, who delineated the general theme and supervised the “quality control” of the contributions under the lead of José Ramón Bertomeu-Sánchez.<sup>5</sup> Though gathering to discuss content, material needs had nonetheless to be attended to: the Katholieke Universiteit te Leuven has provided conference rooms and logistical support, fundings and support has been given by the Fonds Wetenschappelijk Onderzoek – Vlaanderen, the Chemical Heritage Foundation, the Commission for the History of Modern Chemistry (DHS), Mémosciences and the Société Française de Chimie.

We were lucky enough to attract the interest of corporate sponsors who have been attracted by our programme and its scope. Evonik- Degussa even though they were undergoing internal changes accepted to be our Platinum sponsor and

Janssen Pharmaceutica, a Belgian pharmaceutical company, and a division of Johnson and Johnson joined as silver sponsor. Thanks also to DSM who joined us afterwards as a Bronze sponsor this edition of the proceedings was secured. It is an immense pleasure to thank these sponsors who have also shown their dedication to their own corporate histories.

We would like to thank our industrious Working Party chairman Ernst Homburg, and also our Portuguese colleagues, especially Maria Elvira Callapez and Isabel Malaquias who have generously shared their experience gained during the 5<sup>th</sup> ICHC. Their readiness to help allowed us to maintain the very high standards they had set for the Estoril-Lisbon meeting in 2005.

As usual, the conference outing was private visits to museums of interest, this time in Ghent. The first visit deserves a special mention in this report was to the Museum for the History of Sciences of the University of Ghent which has an excellent collection of instruments used in teaching and research since its foundation in 1817. The Director, Dr. Kristel Wautier expertly introduced the main collections and the temporary exhibition of particular chemical interest she had prepared about Leo Baekeland. Leo Hendrik Baekeland (1863-1944), the inventor of Bakelite, studied chemistry in Ghent under Théodore Swarts (1839-1911). On show were the Bakelite volumetric apparatus, resistant to hydrofluoric acid, which Baekeland made for his step-brother, Frédéric Jean Edmond Swarts (1866-1940). This was a most significant and useful gift to Swarts, a pioneer in the organic chemistry of fluorine. The contemplation of such chemical heritage was at least as significant and meaningful to those who devote themselves to the current practice or to the history of chemistry. Another impressive item was Kekule's blackboard, surrounded by many instruments and a working bench from that period, including a models of molecules devised by him. It is indeed during his professorship in Ghent (1858-1867) that August Kekulé (1929-1896) published his famous hypothesis on the structure of benzene.

The plenary lectures reflected the many facets of the meetings main theme. Prof. Bernadette Bensaude-Vincent (Université Paris X) gave the opening lecture about "The New Identity of Chemistry as Biomimetic and Nanoscience", in which she analysed the recent trends of biomimetic chemistry within the long tradition of chemistry challenging nature through the artificial creation of life. Through a survey of various strategies for mimicking biological materials and biological processes, she argued that nanotechnology is revitalising the chemists' ambitions to answer the big questions about the origin of life and the universe. Prof. Ana Simões (University of Lisbon) investigated the emergence and identity of quantum chemistry in her talk "Dangerous Liaisons or Unavoidable Associations:

Quantum Chemistry at the Crossroads of Chemistry, Physics and Mathematics”. She showed how the history of quantum chemistry illustrates one of main characteristics features of twentieth-century science, namely the exploration of frontiers and the crossing of disciplinary boundaries, reinforced by the mediation of many new instruments and tools. Moving some centuries back, Prof. Lawrence Principe (The Johns Hopkins University) showed through his lecture “Transmuting Chymistry into Chemistry: Eighteenth-Century loss of Chrysopoeia and its Repudiation” how the disappearance of alchemical pursuits at the Paris Academy of Science were triggered by the local French context with the suspicions of poisoning at the court and not so much by a shift in the aims of the exact sciences. With his presentation “Close Neighbours, but Different Chemistries: Chemistry in the Low Countries 1600-1900”, Prof. Ernst Homburg (University of Maastricht) demonstrated clearly the influence of local political, social or economical context by contrasting the development of the discipline in two very different settings, the southern and northern Low Countries, now Belgium and the Netherlands.

The conference was attended by 112 participants from over 26 nations. Europe was of course well represented but what was more striking was the growing presence of overseas historians of chemistry or historically-minded chemists: some came from the fringes of Europe, Israel or Russia, others from much further away, Canada, United States, Mexico, Brazil, Taiwan and Japan. Four workshops were organised by groups of historians and chemists on specific topics related to the main theme: “Early Modern Chemistry and Mechanical Philosophy”, “Chemistry Courses and the Construction of Chemistry”, “Chemistry in Relation to Physics in the xx<sup>th</sup> Century” and “Foundation and Development of Macromolecular Chemistry”. The other submitted papers were organized in fourteen sessions dealing with a broad range of topics. Two sessions included papers dealing with the identity and boundaries of chemistry from xvii<sup>th</sup> to xx<sup>th</sup> century. Other sessions analysed the processes that make possible a scientific discipline: research institutions and societies, teaching, scientific communication, specific literature, chemical careers and professional life. Another group of sessions analysed the changing relationship between chemistry and other areas such as physics, medicine, pharmacy, engineering and industry.

One of major aims of the Working Party on History of Chemistry of EuCheMS is to facilitate communication between historically interested chemists and historians of chemistry from all over Europe. This conference lived well up these expectations. The broad range of topics, problems and methods of the papers attests to the different background and interests of the participants in the meeting. The meeting proved to be useful in bridging the gap and securing a space for dialogue

between historians and chemists, which seems to be crucial for enlarging the numbers of those studying the history of chemistry.

Readers of these proceedings will however perceive the “two-cultures” gap –the chemical and the humanities– in the different narratives, rhetorical styles, main focus and, despite editors’ efforts, in the references and notes. The editors have chosen to leave the final decision to the contributors. The key-note lectures will be found in the first part whereas the second part is devoted to symposia. In the call for papers, this edition of ICHC had indeed introduced a new kind of session, organised by one or more colleagues on a specific theme. These symposia are preceded by a short introduction. In the third part, papers and posters have been organised in a thematic order, inspired by the programme<sup>vi</sup>, slightly improved by merging posters and papers. This option has allowed the editors to do full justice to the quality of contributions and the intensity of scientific discussions that took place during the meeting, standing in front of posters as well as during sessions or coffee breaks. Indeed the Scientific Committee had a hard time in selecting from a large number of good abstracts, and only logistic restrictions forced to make a distinction between papers and posters. The presentation herein thus reflects better the actual richness and diversity of this fruitful meeting.

This huge amount of work to produce this volume would not have been possible to achieve in a short time without the active participation of all contributors, many of whom have squeezed an already more than full teaching and research schedule time to provide their papers. We are also grateful to the Chemical Heritage Foundation for providing good quality illustrations, out of their splendid collections, for a few papers. But the final and crucial work was achieved by Vicente Zorrilla-Palau (Instituto de Historia de la Medicina y la Ciencia “López Piñero”, Valencia, Spain), who has done a great job in producing the final copy for the printer, using his expertise to solve the many problems associated with the layout of around eighty papers coming from computers from all over the world.

Merging the diversity and managing to achieve some kind of coherence, technically and most importantly also intellectually was a challenging task. These features have been at the heart of the editors’ work, who have enjoyed greatly their collaboration and share of different cultural and educational settings. The team hopes that the present book will not only reflect the pluralism and the vitality of meeting, but also provide a snapshot of the present state of the art among the growing community with interests in the history of chemistry.

JOSÉ RAMÓN BERTOMEU-SÁNCHEZ  
DUNCAN THORBURN BURNS  
BRIGITTE VAN TIGGELEN

## Notes

<sup>1</sup> “Die Chemie unterscheidet sich von den meisten anderen Wissenschaften in Bezug auf ihre Entwicklung wesentlich dadurch, dass ihr Zweck zu verschiedenen Zeiten ganz verschieden aufgefasst wird [...] Bei dieser Wissenschaft [Chemie] wechselt nicht allein die Wahl der Hilfsmittel und die Anwendung, sondern auch die ganze Aufgabe, die Bedingung der Existenz der Wissenschaft. Kann dann aber von der Geschichte der Chemie, als der Geschichte einer Wissenschaft, die Rede sein?”. Cf. Hermann Kopp, *Geschichte der Chemie* (Brunswick: F. Vieweg und Sohn, 1843-47), vol. I, p. 4-5.

<sup>2</sup> “La chimie a toujours été héritière d’un territoire dont la multiplicité déborde toute définition *a priori* et impose le défi d’une identité à construire. Parce que leurs concepts et leurs méthodes formaient des noeuds ou des carrefours entre des espaces hétérogènes, parce qu’ils tenaient des lieux stratégiques mais disputés, les chimistes n’ont cessé de défendre l’autonomie et la rationalité spécifique de leur science”. Bernadette Bensaude-Vincent, Isabelle Stengers, *Histoire de la chimie* (Paris : La Découverte, 1992), p. 9. English translation taken from Bernadette Bensaude-Vincent, Isabelle Stengers, *A History of Chemistry* (Cambridge, MA: Harvard University Press, 1996), p. 5.

<sup>3</sup> Some examples of this feature of chemistry are investigated in Mary J. Nye, *From Chemical Philosophy to Theoretical Chemistry* (Berkeley: University of California Press, 1993). Jonathan Simon, *Chemistry, Pharmacy and Revolution in France, 1777-1809* (Aldershot and Burlington: Ashgate, 2005); Carsten Reinhardt (ed.), *Chemical Sciences in the 20th Century. Bridging Boundaries* (Weinheim: Wiley-VCH, 2001).

<sup>4</sup> Members of the local committee were Kenneth Bertrams (Université Libre de Bruxelles), Hendrik Deelstra (Universiteit Antwerpen, honorary chairman), Ernst Homburg, (Universiteit Maastricht), Bernard Mahieu, Université catholique de Louvain, Belgium), Adriaan Minderhoud, (Chemiehistorische Groep KNCV), Geert Vanpaemel (Katholieke Universiteit Leuven), and Brigitte Van Tiggelen, (Université catholique de Louvain, Mémosciences asbl. (chairwoman)).

<sup>5</sup> Members of the scientific committee were Marco Beretta (Università di Bologna, Italy), José Ramón Bertomeu-Sánchez, (Universitat de València, Spain (chair)), Ana Carneiro, (Universidade Nova de Lisboa, Portugal), Ursula Klein (Max Planck Institut für Wissenschaftsgeschichte, Berlin, Germany), Laurence Lestel (Conservatoire National des Arts et Métiers, Paris, France), Anders Lundgren (Uppsala Universitet, Sweden), Peter Morris (Science Museum, London), Carsten Reinhardt (Universität Regensburg, Germany), Soňa Štrbáňová, (Ústav pro soudobé dějiny, Akademie věd České republiky, Prague, Czech Republic) and Brigitte Van Tiggelen (Université catholique de Louvain, Mémosciences asbl.).

<sup>6</sup> Information pertaining to the meeting can be found on the webpage <<http://www.6ichc.be>>, as for the programme <<http://www.6ichc.be/6ICHC-Programme.pdf>>.

# I

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## Plenary Lectures



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## Transmuting Chymistry into Chemistry: Eighteenth-Century Chrysopoeia and Its Repudiation<sup>1</sup>

Lawrence M. Principe\*

In the early decades of the eighteenth century, chemistry suddenly lost a large and central piece of its traditional domain. The sudden and definitive loss of *chrysopoeia*, that is, the search for the transmutation of base metals into gold, from chemistry in the 1720s was a crucial moment for the developing identity of chemistry. Yet despite the significance of this event, relatively little is known about it, **how** it happened, **why** it happened, and **why** it happened **when and where** it did.

The reasons for our relative lack of knowledge regarding the demise of *chrysopoeia* are not hard to locate. For a long time the history of science was dominated by triumphalist narratives of progress. In the context of these narratives, alchemy was seen as non-scientific, a misguided delusion, a blatant error. Under such circumstances, the loss of *chrysopoeia* did not seem to need any special historical explanation.

But today we know better. Transmutational alchemy was not irrational. It was studied and practiced by important figures down very nearly to the point when it vanished suddenly from the scene. It contributed important concepts and practices to the development of modern science, and was not seen as separate from what we now call chemistry. *Chrysopoeia* was a central, an essential part of chymistry.<sup>2</sup> Thus, its disappearance around 1720 *does* need to be explained historically.

The easiest explanation is that new theoretical developments ruled out the possibility of *chrysopoeia*. This hypothesis seems reasonable. However, there is no evidence to support it, and considerable evidence to refute it. Vigorous debates over the reality of transmutation and the reality of the Philosophers' Stone did *not* appear suddenly in the eighteenth or even the seventeenth century. They were the constant companion of *chrysopoeia* from the Middle Ages. What is striking is that the theory-based arguments against transmutation changed very little during all that time, some of the same arguments against transmutation cited by Geber in the thirteenth century were voiced by Thomas Erastus in the sixteenth,

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and by various other anti-*chrysopoeians* in the seventeenth. Seventeenth century mechanical views of matter and its transformations, far from weakening belief in transmutation actually strengthened it. For if all substances are composed of the same universal matter then it would be possible to transform anything into anything, indeed, to accomplish chymical changes beyond what *chrysopoeians* considered possible. That famous champion of corpuscularianism and mechanical chymistry, Robert Boyle was himself absolutely convinced of the reality of the Philosophers' Stone and its transmutational abilities.<sup>3</sup> There simply is no evidence that new theoretical structures or experiments dealt a death-blow to *chrysopoeia*.

Thus the loss of *chrysopoeia* is not to be found, I think, in purely theoretical, scientific, or experimental developments. In fact, if we examine carefully the attacks against *chrysopoeia* in the period leading up to the 1720s, we find that they actually move *away* from purely scientific issues, and towards ridicule and moral opprobrium. The most famous example occurs in 1679 edition of Nicolas Lemery's popular *Cours de chymie* where Lemery launches a full-scale attack on transmutation.<sup>4</sup> He cites the least solid of *chrysopoetic* theories and ridicules them as foolish and primitive. He then moves on to fraud, extending the well-known and long-term connection of cheating practices to transmutation to claim that **all** *chrysopoeia* is simply fraud. The reasons for Lemery's sudden attack remain unclear, but his views are nothing new. Such accusations of fraud were common back to the Middle Ages.<sup>5</sup> Active *chrysopoeians* themselves catalogued such frauds. Thus while accusations of fraud and immoralities were the primary weapon used against alchemy at the end of the seventeenth century, they were not by themselves enough to explain *chrysopoeia's* demise. Something else had to use this weapon consistently and effectively.

I suggest that *chrysopoeia* fell at the hands of a movement intended to domesticate chymistry into a respectable professionalised discipline. The professionalisation of chemistry, as opposed to that of chemical pharmacy, took place most of all, it can be argued, at the Académie Royale des Sciences in Paris. It was here that chemistry received its first stable institutional home, a greatly enhanced public profile, and clear links of duty and responsibility to the State. The very first topics of study chosen by the infant Académie in late 1666 were chymical. Throughout the seventeenth century, chemistry had a presence at the Parisian Académie stronger and more prominent than anywhere else. Chemistry's position was made more visible and official during the *Rénouveau* of 1699 when five positions out of the thirty earmarked for specific scientific disciplines were given to chemistry, placing it for virtually the first time on an equal footing with astronomy, physics, mathematics, botany, and anatomy.<sup>6</sup> The Académie Royale, with its state-financed members and its Academicians' status as the official natural philo-

sophical thinkers of France, guaranteed a high and public profile for chemistry that was not equalled anywhere else, and so it is here that I shall look first for answers.<sup>7</sup>

But this enhanced, and more public, status for chemistry provoked problems. Chemistry suffered an ambiguous reputation in the seventeenth century. Its close link to productive, artisanal, commercial and simply dirty, smelly, and messy processes tainted it with low status relative to more exalted topics such as astronomy, physics, and even mechanics. Metallic transmutation in particular was prone to accusations of fraud and futility. Contemporaneous fine art, theatre, and literature repeatedly (though not exclusively) represented chymists to the public as thieves, counterfeiters, fools, or charlatans. The records of the Bastille recount many chemists, including even the lecturer at the Jardin du Roi, Christophe Glaser, arrested on charges of poisoning, and subsequently ruined.<sup>8</sup> Contrary to what we might like to believe, the seventeenth-century public ridicule of chymistry did not necessarily distinguish between the *chimistes* of the Académie and the *chimistes* ridiculed on the stage, declaimed in the popular press and gossip, and arrested for poisoning or fraud. Thus, Robert Boyle apologises explicitly in his “Essay on Nitre” for the time he spent in “such an empty and deceitful study” as chymistry.<sup>9</sup> And even within the early Académie, for example, Edme Mariotte in writing his 1678 textbook of logic declared simply “How many times has one seen the curious tricked by the impostures of astrologers and chemists?”<sup>10</sup> Thus for some people at least, admitting chemistry to the Academy was a nervous situation rather like inviting a provincial cousin to dine with the King. One is always worried about what embarrassing thing he might say, what other people might think, and whether he’ll wipe his mouth on the tablecloth.

In short, at the end of the seventeenth century chemistry needed a new identity or at least a complete make-over. The easiest solution, it seems to me, as it appears to have seemed to spokesmen of the Académie, was to make a fresh start for chemistry: to create chemistry afresh as if it had never really existed before. This included breaking visibly with the previous chemical tradition, and specifically avoiding and declaiming against the subset of chymistry most easily subject to ill repute, namely, *chrysopoeia*. Thus the Académie could protect the chemistry it was newly professionalising from the ambiguous status that had followed *chimia* since the Middle Ages. It also thus became possible to quarantine all of the questionable activities relating to chemistry under a completely different rubric, namely, that of “alchemy.”<sup>11</sup>

Indeed, there is clear evidence of longstanding tension and uneasiness at the Académie in this regard. What is of particular interest is that it was predominantly the non-chemist administrators of the Académie who tried to suppress trans-

mutation while several of the chemists themselves continued to support it. This tension appears at the very founding of the institution. When Jean-Baptiste Colbert founded the Académie in 1666, he forbade only two topics: astrological prognostication and the Philosophers' Stone.<sup>12</sup> Today we might too easily interpret this regulation as a forward-looking rejection of so-called pseudo-sciences. But it is worth pointing out that these two topics were also those that were potentially most politically subversive and open to controversy; just the sort of a thing with which an agent of the Crown like the Académie should not be involved.

Yet despite Colbert's prohibition, the Académie's foremost chymist, Samuel Cottureau Duclos busied himself with traditional chymical pursuits including the Philosophers' Stone, and did so in the Académie's laboratory that he designed.<sup>13</sup> But shortly after Duclos' death in 1685, Pierre Bayle's *Nouvelles de la Republique des Lettres* published a death-bed repudiation of the Philosophers' Stone supposedly given to Clément, the keeper of the King's Library, by Duclos.<sup>14</sup> One should always be suspicious of death-bed conversions. If it was a true repudiation, it does nothing to diminish the fact that Duclos spent so much time on transmutation. But one cannot discount the possibility that this repudiation was, at least in part, a show for public consumption, akin to Duclos' publicised conversion to Catholicism that took place at the same time.<sup>15</sup> Indeed, Clément's account tells of how he asked Duclos to make an avowal "for the public and the service of the King" in order to "restrain those who would too easily engage themselves with the unhappy passion of idle puffery [*la malheureuse passion de soufflerie*]."<sup>16</sup>

Indeed, it is conceivable that the statement was not even Duclos', but an independent public relations move by the Académie itself. The account does seem exaggerated in some details. For example, it states that Duclos burned all his papers on alchemy; but this was not true, many volumes survive scattered in several archives in France and many more existed at least until the late 1750s. And we know that the Académie was already uncomfortable with Duclos' work, or more accurately, with public knowledge of it. During Duclos' life the Académie refused him permission to publish a major work on chemistry, and his massive work on salts, ready for the press, and including large sections on such things as the Helmontian alkahest, remains unpublished to this day.<sup>17</sup> Moreover, just three months after Duclos' death, Louvois, Colbert's successor as the minister overseeing the Académie, sent a memo to the assembly ordering them to avoid any work dealing with "the extraction of the mercuries of the metals, the transmutation of metals, and their multiplication, about which Mr. de Louvois does not want to hear anything spoken."<sup>18</sup> Surely this ministerial intervention was a response to Duclos' former activities.

Yet this second ministerial warning had no more lasting effect than the first. Five years later, Wilhelm Homberg was elected to the Académie, bringing with himself a considerable range of experiences and interests in metallic transmutation. Homberg, whom I have been studying closely for the past several years, was the chief chemist at the Academy from 1691 until 1715. He, more than any other chemist of the early eighteenth century, laboured to produce a coherent theory of chemistry based upon and illustrated by experiments. Indeed, his system was widely read, adopted, amended, and extended by others throughout the first half of the eighteenth century.<sup>19</sup>

Homberg was a crucial figure in the end of *chrysopoeia*. Homberg was perhaps the only person ever to be both a professional chemist in the modern sense of that word and an unapologetic *chrysopoeian*. Rhetoric carefully crafted by others to provide new boundaries for chemistry would render it impossible for anyone else ever to hold both of those positions again.

There is no question that Homberg was deeply involved in studies of metallic transmutation throughout his career. In 1684 he worked on a process to transmute mercury into silver. Soon after his installation at the Académie, he worked on the extraction of metallic mercuries, one of the very things forbidden by Louvois. Throughout the 1690s he worked extensively with a specially prepared mercury, known in *chrysopoetic* circles as Philosophical Mercury, and believed to be the crucial ingredient for the Philosophers' Stone. The process links Homberg inseparably with other *chrysopoeians* such as Alexander von Suchten, Basilius Valentinus, Johann Joachim Becher, and George Starkey, *alias* Eirenaeus Philaethes.<sup>20</sup> Indeed, the unpublished manuscript of Homberg's first attempt to write a textbook of chemistry, dating from the 1690s affirms explicitly that he carefully pursued "the entire work of Philaethes" in regard to Philosophical Mercury and the Stone.<sup>21</sup> In the same text, Homberg's second chapter is a lengthy and sensitive overview of the theory of transmutation and the Stone, and states unambiguously that transmutation is an important and integral part of chemistry.

In 1702, Homberg became the chemistry tutor to Philippe II, Duc d'Orléans, nephew of Louis XIV. Philippe built a magnificent laboratory at the Palais Royal where he and Homberg worked together on chemical experiments. When Philippe bought the enormous burning lens made by Graf Ehrenfried Walther von Tschirnhaus, the first experiments they performed with it were attempts to use the sun's light to transmute silver into gold.<sup>22</sup> Finally, Homberg's most important publication, his *Essais de chimie*, a kind of a serial textbook published in the Académie's *Mémoires* from 1702 to 1710, is built to a large extent around illustrative experiments with Philosophical Mercury. Homberg also claims to have converted a portion of the mercury into gold, using a traditional *chrysopoetic* method.<sup>23</sup>

But Homberg's continued activity in *chrysopoeia* was in direct conflict with the new image of chemistry that the institution's public spokesman, the perpetual secretary Bernard de Fontenelle, was trying to project. Fontenelle is the antagonist of this story. He held a low opinion of chemistry in general, seemingly because it could not be reduced to deductive axioms, to "l'esprit géométrique" of Descartes, like mathematics and physics. Indeed in his lengthy essay on the utility of the sciences, he mentions chemistry in only one sentence, and then only as an adjunct to medicine.<sup>24</sup> Fontenelle's prejudice against chemistry appears again and again. For example, in 1700 Homberg published a sophisticated paper that literally sets the foundations for the standardisation of chemical reagents for analysis, yet Fontenelle misses, or ignores, the paper's whole point for chemistry, and instead picks out a trivial comment made by Homberg about using ground oyster shells as an antacid, and so concludes pompously in his summary of the paper that "it is principally to these sorts of [medical] uses that all chemical discoveries ought to be turned."<sup>25</sup> For Fontenelle, chemistry was not even a science in itself. The search for hidden arcana, like transmutation and the alkahest, only made things worse for chemistry. Indeed, one of Fontenelle's popular *Dialogues of the Dead* summons up the ghost of Ramon Lull, supposed author of numerous *chrysopoetic* works, who admits that after his death he finally realised (too late!) that the Philosophers' Stone was a lie, but Lull concludes happily that "though I was not able to make the Stone, at least I was able to fool other people into believing I had."<sup>26</sup>

Homberg and Fontenelle were opposed regarding nearly everything dealing with chemistry. While Fontenelle praised physics and Descartes above all else, Homberg rejected Descartes and his methods, and praised chemistry specifically as the science of "infinite extent" that gives us true knowledge, whereas the physicists could not tell us anything certain about the material world.<sup>27</sup> As a colleague of Homberg's wrote shortly after his death "For Homberg, all philosophy came through the manipulation of the fire-tongs."<sup>28</sup> But worst of all, for Fontenelle that is, Homberg's *chrysopoetic* activities became well known outside the Académie. As such they were constantly working against Fontenelle's propaganda for the Académie and for the much diminished and highly domesticated role he was marking out for chemistry. For example, in 1711 Leibniz wrote to Homberg asking him to reveal more of his experiences with transmutation, arguing that such experiments would be useful to refute the matter theories of physicists like Nicolaas Hartsoeker and others.<sup>29</sup> A remarkable book-length manuscript written about 1720 went much further. Extant in multiple copies in France and England, this work, entitled "Essay to Uncover the Knowledge and Practice of the Work of the Chemical Philosophers," fully embraces Homberg within the alchemical tradition. When the anonymous author lists several methods of making the

Philosophical Mercury, he cites in order the methods of Pantaleon and Philalethes, followed directly by “the method of Mr. Homberg.” Even more strikingly, he adopts Homberg’s chymical theory, replete with explicit citations to papers in the Académie’s *Mémoires*, and grafts this theory seamlessly onto a theory of the Philosophers’ Stone and metallic transmutation. Perhaps yet more surprisingly, the third part of the book, which supplies recipes for various preparations relating to *chrysopoeia*, actually imitates the style of Lemery’s famous *Cours de chymie*, with preparative processes followed by sections headed “Remarques” that provided the theoretical background for the procedures. This manuscript argued clearly that developments in chemistry, whether in terms of theory, practice, or style of exposition, did not defeat *chrysopoeia*, instead, here a *chrysopoeian* eagerly adopts the most modern chemical theory in order to *help* in his search.<sup>30</sup>

Amid Homberg’s continuing publication of papers on the analysis of metals and the production of gold and silver, his student Etienne-François Geoffroy claimed success in synthesising iron from non-metallic starting materials.<sup>31</sup> Not *chrysopoeia*, surely, but evidence of the producibleness of the metals, and thus support for more traditional transmutation and the theories that undergirded it. Geoffroy’s claim was attacked by Louis Lemery, son of Nicolas, and their debate lasted from 1704 to 1708.<sup>32</sup> During this time, Homberg used Geoffroy’s results to lend support to traditional *chrysopoeia*, while Lemery sarcastically criticised Geoffroy’s work by tying it to that of Johann Joachim Becher. Fontenelle used the opportunity to reflect negatively upon the artificial production of metals in an unusually lengthy commentary on the paper.<sup>33</sup>

But while Lemery and Fontenelle could criticise Geoffroy, there was little they could say openly about Homberg, given his close relationship to the Duc d’Orléans. Yet while this relationship undoubtedly protected Homberg to some extent, it did nothing to help the public status of chemistry. For knowledge of Philippe’s passion for chemistry co-existed with rumors (and perhaps more than just rumors) that the Duc’s interest in chemistry was accompanied by one in magic, necromancy, water-gazing, and demonic invocations, and it was easy to imply that Homberg was involved as well. This situation explains the strange disclaimer made by the Duc de Saint-Simon when describing Philippe’s interest in chemistry, for he is eager to point out that “it was all done very publically” as if to counter unspoken assumptions that something secret and disreputable was going on. Saint-Simon also claims that Philippe scoffed at transmutation, but Philippe’s mother, Liselotte von der Pfalz, wrote in her letters about how her son and Homberg could make gold in the laboratory, thus indicating a more positive view of *chrysopoeia* on the part of future Regent of France.<sup>34</sup> (It should also be pointed out that another of Philippe’s physicians was Adrien Helvetius, son of the famous Johann Friedrich Helvetius who witnessed transmutation at his house in



The Hague at the hands of an anonymous travelling adept in 1666, and described in *Vitulus aureus*.<sup>35</sup>) More embarrassment came in 1712, when following the deaths of the Dauphin and Dauphine, Homberg was, like Glaser before him, accused of being a poisoner. Unlike Glaser, however, Homberg was saved from the Bastille, but only barely and only by the direct action of Louis XIV.<sup>36</sup> All of this must have looked very bad indeed from the point of view of Fontenelle and his program for the public face of the Académie and of chemistry.

Upon Homberg's death in 1715, Fontenelle immediately began to sanitise Homberg's legacy. His eloge of Homberg is full of revisionist statements intended to force Homberg into the identity Fontenelle had cast for respectable chemists and a respectable chemistry.<sup>37</sup> According to Fontenelle, both Homberg and Lemery (who died the same year) *literally fled* from practitioners of the "old disreputable" chymistry. For Lemery it was from Glaser, whom Fontenelle describes as "a true chemist, full of obscure ideas, greedy of such ideas, and unsociable."<sup>38</sup> For Homberg, his alleged fear over association with a *chrysopoeian* compelled him leave Paris and flee to Italy. At this point, Fontenelle declares loudly that "Homberg was too capable to aspire to the Philosophers' Stone and too sincere to put such a vain idea into anyone's head." But Fontenelle protests too much, for Homberg himself described in print how at just this time he was trying to transmute mercury into silver using an oil distilled from human faeces.<sup>39</sup>

But Fontenelle's attempt to enhance the status of chemists and chemistry involved not only denying relationships with the disreputable but also creating relationships with the reputable. Thus Fontenelle's eloge of Homberg also provides him apprenticeships with more than a dozen notables of the late seventeenth century, even when it means that Fontenelle's chronology apprentices Homberg to people who would have been dead when Homberg met them.<sup>40</sup> Fontenelle is correct to say that Homberg met Boyle, although my research shows that it is impossible that he stayed with him for a year to study, as Fontenelle claimed, in "one of the most learned schools of *physics*." Furthermore, I note with delicious irony, that the only thing that I can confidently assert that Homberg did learn from Boyle was the secret preparation the Philosophical Mercury.<sup>41</sup>

Fontenelle, or perhaps the Académie in a more corporate sense, may even have played a role in preventing Homberg's life-work from being published. Upon his death, Homberg left behind a completed version of his *Essais de chimie*, on which he had been working for over a decade. The manuscript was entrusted to his student Geoffroy, with the request to publish it as soon as possible.<sup>42</sup> But nothing ever appeared. Given the "alchemical" origins of the experiments upon which so much the text was based, and its claims successfully to have produced gold from mercury, the publication of this work, bearing Homberg's name and his title as



Academician, may well have been unwelcome for the image of the Académie and of chemistry being constructed in 1716. If some kind of suppression did take place, it reminds one of the suppression of Duclos' treatises a generation earlier.<sup>43</sup>

The topic of the loss of *chrysopoeia* brings us finally to an event that is often cited in the literature as signaling the last nail in the coffin of *chrysopoeia*, namely the publication of a paper by Etienne-François Geoffroy entitled "Some cheats concerning the Philosophers' Stone."<sup>44</sup> Presented in 1722 and published in 1724, it relates methods used by fraudulent would-be transmuters of metals to trick people into believing that they have witnessed a transmutation; for example, using crucibles that contain gold hidden under a false bottom, or stirring a molten mixture with a hollow rod that contains gold hidden inside. While this paper is often cited, it needs to be better contextualised, for the paper presents a number of historical problems. To what extent does it represent Geoffroy's views? More importantly, why did Geoffroy present it at all, and why in 1722?

It has been shown that the majority of Geoffroy's paper is copied from the *Examen fucorum pseudochymicorum*, a well-known work published in 1617 by Michael Maier and intended to help his fellow *chrysopoeians* to distinguish true from false transmutations. And much of Maier's work is in turn borrowed from Heinrich Khunrath's *Trewhertzige Warnungs-Vermahnung* of 1597.<sup>45</sup> Thus Geoffroy's paper provided nothing new, merely a restatement of material over a century old. Moreover, it would be incorrect to conclude that Geoffroy was necessarily himself utterly opposed to transmutation, for while he describes the cheating practices he nowhere claims that all *chrysopoeia* is fraudulent. Indeed, the catalogue of Geoffroy's library shows that he owned more than seventy books on transmutation, including classic works by Philalethes, Valentine, and others, as well as Manget's huge 1702 compendium *Biblioteca chemica curiosa*.<sup>46</sup>

One important, but hitherto overlooked, feature of Geoffroy's paper is that it was presented not at a private *séance*, but rather at one of the Académie's semi-annual public assemblies (on 15 April 1722); thus, we must consider that it was designed for a wider audience than just the Académie. Papers given at these special assemblies were carefully chosen by committee, and Geoffroy's was virtually unique in that it did not present any research results. It seems instead designed as a public statement intended to reinforce the new boundaries of chemistry, and as will be suggested in a moment, perhaps to deflect contemporaneous rumors about the Academy in regard to transmutation.

Fontenelle used Geoffroy's paper as an opportunity to write a lengthy commentary containing his most vitriolic and sarcastic condemnations of "les Alchimistes."<sup>47</sup> But Fontenelle also used this opportunity explicitly to distinguish "alchemical" claims from the work done by Homberg twenty years earlier. He also

asserts that alchemists have never made a single grain even of an imperfect metal, perhaps a reference to Geoffroy's earlier claims to have produced iron. Geoffroy's reading of this paper might thus be seen as a public act, a kind of a renunciation of the work both he and his master had performed previously, and a statement of the Académie's official views. The paper's public presentation also meant that the popular press covered the event, but while the *Mercure Galant* routinely mentioned the Académie's public meetings, in this case, almost uniquely, the following month's issue carried an additional seven-page reprise of Geoffroy's paper, and no mention of the other papers presented that same day.<sup>48</sup> It might not be out of place to suggest that Fontenelle may have orchestrated this broader coverage in the popular monthly.

But were there special incentives for this paper in 1722? Two events that reinforce the idea that Geoffroy's paper was primarily a public relations event will now be pointed out. Consider the financial state of France in 1720 and 1721. The banking scheme organised by the Scot John Law, with the backing of the Regent, had begun a spectacular collapse in 1720. Too many bank-shares had been sold and there was simply not enough gold in France to back up the banknotes. Not having enough gold was the traditional problem for alchemists! Thus a rumor began to circulate that the Regent of France had ordered the chemists of the Académie to apply themselves precisely to the problem of *chrysopoeia*.<sup>49</sup> Apparently the rumor gained sufficient currency that the agent of the English ambassador, who was sending weekly reports to London on the developing bank crisis, felt obliged to send home a special account of the Regent's abilities in chemistry, and his work with Homberg.<sup>50</sup> If the rumor was true, then what a change from the orders of Colbert and Louvois, and whether or not it was true, then what a disaster for the image of the Académie and of its professionalised, domesticated, and respectable chemistry that Fontenelle was struggling to craft! The involvement of the Regent may also explain the rather late date at which with paper was published. Geoffroy's rather mild paper was given in 1722, and published along with Fontenelle's vitriolic condemnation only in 1724, by which time the young Louis XV had been crowned and the Regent had died.

Another contributing event may have been the publication, just before Geoffroy's presentation, of *Les secrets les plus cachés des Philosophes anciens* by Francesco Maria Pompeo Colonna. The book recounted successful transmutations and other outstanding chymical feats. Colonna's book was reviewed in the *Journal des Sçavans*, which, although it complained that it was written in very bad French, still maintained that it contained important scientific material.<sup>51</sup> While a single publication might not ordinarily provoke a response from the Académie, in this case Colonna had several links to the Académie. He had collaborated with Geoffroy's father, was a friend of the famous astronomer Gian Domenico Cassini

and of the brother of Jean-Paul Bignon, President of the Académie.<sup>52</sup> Thus Geoffroy's publically delivered paper could serve as a countermeasure to possible rumors about the Académie's links to *chrysopoeia*.

It is also crucial to note that the new boundaries of chemistry were reified at this same time, at least in French, by the definitive separation of the words alchemy and chemistry. Geoffroy's paper never uses the word *alchimiste*. He writes instead *chimiste philosophe*, the same term employed by Colonna and nearly all other advocates of transmutation writing in France in the early eighteenth century. But Fontenelle employs the word *alchimiste* consistently and as a term of ridicule, to mark out, to segregate, a group now to be separated entirely from the *chimistes*. Before the end of the next decade this division was complete. For example, the abbé Pluche's 1739 *Histoire du ciel*, describes *la chimie* as a useful and admirable science, while *l'alchimie* was a discredited superstition of former ages.<sup>53</sup>

What is portrayed in this paper is the long-term tension within the Académie Royale over the status and boundaries of chemistry, particularly regarding *chrysopoeia*. The non-chemist administrators—Colbert, Louvois, and most of all Fontenelle, joined occasionally by the Lemerys, took a strongly negative view of *chrysopoeia*, and simultaneously tried to push chemistry towards a servant role to pharmacy. At the same time, the chief chemists, that is to say, the most prominent and most innovative chemists of the Académie, who had an expansive view of the explanatory and philosophical status of chemistry, namely Duclos, Homberg, and Geoffroy, continued to explore transmutational experiments. Geoffroy's final views on the subject will be treated elsewhere, but herein it is proposed that his famous (or infamous) paper of 1722 was largely an act of public relations, triggered by events and associations that could have reflected badly on the Académie.

Given the continued interest in *chrysopoeia* by prominent chemists, we cannot consider the loss of *chrysopoeia* to be simply the result of scientific developments. I have instead pointed to the desire to domesticate and redefine the identity and scope of chymistry into a professionalised and respectable public discipline. Of course I do not claim that the end of *chrysopoeia* came about from a single cause, I am not that reductionist or that brash. One must consider both active and passive factors at work in several contexts. Herein the focus is on the important active repression of *chrysopoeia* at the Paris Academy, and much further work must be done for other locales. At this point, I will mention only the curious fact that Georg Ernst Stahl turned from being a supporter to a critic of *chrysopoeia* at very nearly the same time, the late 1710s and early 1720s, which also correlates with a change in his social status as he moved from university to court.<sup>54</sup> Nonetheless, publications on *chrysopoeia* continued to be produced in Germany into the 1760s long after

they had ceased in France and England. In Sweden, even some official and high-ranking chemists continued to hope and to work for transmutation in the 1750s.<sup>55</sup> The greater longevity of alchemy in Sweden and Germany might be attributable to the absence of high-profile, centralised scientific authorities, or perhaps to the greater economic importance of metals and mining in those countries. In France on the other hand, industrial chemistry of all sorts began to prosper in the early eighteenth century. Thus those skilled in or attracted to chemistry might be drawn away into glass or porcelain works or various other industrial processes, rather than working primarily with metals (in which resources France is poor), and certainly with greater prospects of monetary success than working on transmutation. Yet the necessary work of expanding our view of the demise of *chrysopoeia* beyond what I have been able to present herein must remain a task for the future.

## Notes

<sup>1</sup> A fuller treatment of this topic appears in my book, *Wilhelm Homberg and the Transmutations of Chymistry at the Académie Royale des Sciences*, forthcoming 2009.

<sup>2</sup> On the use of the word *chymistry*, see William R. Newman and Lawrence M. Principe, "Alchemy vs. Chemistry: The Etymological Origins of a Historiographic Mistake," *Early Science and Medicine* 3, (1998):32-65.

<sup>3</sup> Lawrence M. Principe, *The Aspiring Adept: Robert Boyle and his Alchemical Quest* (Princeton: Princeton University Press, 1998).

<sup>4</sup> Nicolas Lemery, *Cours de chymie*, 3<sup>rd</sup> edition, (Paris, 1679), pp. 57-60; on Lemery, see Michel Bougard, *La chimie de Nicolas Lemery*, (Turnhout: Brepols, 1999).

<sup>5</sup> Tara Nummedal, *Alchemy and Authority in the Holy Roman Empire*, (Chicago: University of Chicago Press, 2007), pp. 48-72.

<sup>6</sup> *Histoire de l'Académie Royale des Sciences* (hereinafter *HARS*) 1, (1699):4.

<sup>7</sup> For relevant scholarship on the early Académie, see, for example, Roger Hahn, *The Anatomy of a Scientific Institution: The Paris Academy of Sciences, 1666-1803* (Berkeley: University of California Press, 1971); Alice Stroup, *A Company of Scientists: Botany, Patronage, and Community at the Seventeenth Century Parisian Academy of Sciences* (Berkeley: University of California Press, 1990); David Sturdy, *Science and Social Status: The Members of the Académie des Sciences, 1666-1750* (Woodbridge: Boydell, 1995); for chemistry at the early Académie, see Frederic L. Holmes, *Eighteenth-Century Chemistry as an Investigative Enterprise* (Berkeley: Office for History of Science & Technology, University of California at Berkeley, 1989).

<sup>8</sup> *Archives de la Bastille*, 19 vols, (Paris, 1866-1904), esp. vol. 12 (1881): *Règnes de Louis XIV et de Louis XV* (1709 à 1772), pp. 1-5, 52-4; Clara de Milt, "Christophle Glaser," *Journal of Chemical Education* 19, (1942): 53-60; Arlette Lebigre, *1679-1682, L'Affaire des poisons* (Brussels: Complexe, 2001).

<sup>9</sup> Robert Boyle, "Essay on Nitre," in *Certain Physiological Essays* (1661) in *The Works of Robert Boyle*, eds. Michael Hunter and Edward B. Davis, 14 vols. (London: Pickering and Chatto, 1999-2000), 2:85.

<sup>10</sup> Edme Mariotte, *Essai de logique* (1678), preface, in *Oeuvres*, 2 vols. (The Hague, 1740), 2:611.

<sup>11</sup> On the words "alchemy" and "chemistry" and their changing meanings, see Newman and Principe, "Alchemy vs. Chemistry."

- <sup>12</sup> *Lettres, instructions, et mémoires de Colbert*, ed. Pierre Clément, 8 vols. (Paris: Imprimerie Impériale, 1861-70), 5:515 (cited from "Notes et desseins de Claude Perrault, August 1667).
- <sup>13</sup> On Duclos see Alice Stroup, "Censure ou querelles savantes: L'Affaire Duclos (1666-1685)," pp. 435-52 in *Règlement, usages et science dans la France de l'absolutisme*, eds. Christiane Demeulenaere-Douyère and Éric Brian (Paris: Lavoisier Tec et Doc, 2002), and Doru Todériciu, "Sur la vraie biographie de Samuel Duclos (Du Clos) Cotreau," *Reveu d'histoire des sciences* 27, (1974):64-67.
- <sup>14</sup> *Nouvelles de la République des Lettres*, October 1685, pp. 1139-43.
- <sup>15</sup> *Mercure Galant*, August 1685, pp. 136-37.
- <sup>16</sup> *Nouvelles*, October 1685, pp. 1141-2.
- <sup>17</sup> See Stroup, "Censure ou Querelles", where the work on salts (Bibliothèque Nationale, MS fr. 12309) is identified as Duclos' for the first time, p. 439.
- <sup>18</sup> Archives de l'Académie des Sciences, Procès-verbaux (30 January 1686), vol. 11, fols. 157r-158r (on fol. 157r).
- <sup>19</sup> On Homberg, see my forthcoming *Wilhelm Homberg and the Transmutations of Chymistry*, which contains a completely revised biography for Homberg, and an analysis of his chymical studies and influence; on his *chrysopoeia*, see Lawrence M. Principe, "Wilhelm Homberg: Chymical Corpuscularianism and Chrysopoeia in the Early Eighteenth Century," 535-56 in *Late Medieval and Early Modern Corpuscular Matter Theories*, eds. C. Lüthy, J. E. Murdoch, and W. R. Newman (Leiden: Brill, 2001); on his chymical theory of light, see Lawrence M. Principe, "Wilhelm Homberg et la chimie de la lumière," *Methodos: Savoirs et textes* 8, (2008) at <http://methodos.revues.org/>
- <sup>20</sup> Principe, "Chymical Corpuscularianism," p. 555.
- <sup>21</sup> Wilhelm Homberg, Voennomeditsinskoi Akademii, Boerhaave Archive, MS 130, fols. 233r-v. This newly discovered MS is fully treated in my forthcoming book on Homberg.
- <sup>22</sup> Principe, "Chimie de la lumière," sect. 19-22.
- <sup>23</sup> Principe, "Chymical Corpuscularianism," pp. 546-53.
- <sup>24</sup> *Oeuvres diverses de M. de Fontenelle*, 3 vols. (Paris, 1724), 1:1-35 (not paginated), "Sur l'utilité des mathématiques et de la physique," on sig. Aiiiiv.
- <sup>25</sup> Wilhelm Homberg, "Observations sur la quantité d'acides absorbés par les alcalis terreux," *Mémoires de l'Académie Royale des Sciences* (hereinafter *MARS*) 2, (1700):64-71; Bernard de Fontenelle, *HARS* 2, (1700):50.
- <sup>26</sup> *Oeuvres de Fontenelle*, 1:117-20.
- <sup>27</sup> Homberg, MS 130, fol. 112v; on Homberg's view of the status of chemistry, see Rémi Franckowiak et Luc Peterschmitt, "La chimie de Homberg: Une vérité certaine dans une physique contestable," *Early Science and Medicine* 10, (2005):65-90.
- <sup>28</sup> Niedersächsische Landesbibliothek Hannover, Leibniz Briefe 768, fols. 53r-54v; Remond to Leibniz, 23 December 1715; on fol. 54r: "toute la philosophie selon lui étoit dans l'usage de la pincette et ainsi il faisoit peu de cas des anciens et des modernes."
- <sup>29</sup> NLM, Leibniz Briefe 420, fols. 3r-v; Leibniz to Homberg, 10 March 1711.
- <sup>30</sup> Wellcome Institute Library, MS 2298, "Essai pour développer la science & la pratique de l'Oeuvre des Philosophes chimiques"; another copy is Université de Bordeaux, MS 23.
- <sup>31</sup> Etienne-François Geoffroy, "Maniere de recomposer le soufre commun," *MARS*, 5, (1704):278-286, on pp. 284-86.
- <sup>32</sup> Bernard Joly, "Quarrels between Etienne-François Geoffroy and Louis Lémery at the Académie Royale des Sciences in the Early Eighteenth Century: Mechanism and Alchemy," pp. 203-14 in *Chymists and Chymistry: Studies in the History of Alchemy and Early Modern Chemistry*, ed. Lawrence M. Principe, (Sagamore Beach, MA: Science History Publications, 2007); "Chimie et mécanisme dans la nouvelle Académie royale des sciences: les débats entre Louis

Lémery et Etienne-François Geoffroy," *Methodos: Savoirs et textes* 8, (2008) at <http://methodos.revues.org/>

<sup>33</sup> Fontenelle, "Sur la nature du fer," *HARS* 9, (1708):61-65.

<sup>34</sup> Louis de Rouvroy, duc de Saint-Simon, *Mémoires*, ed. Yves Coirault, 8 vols. (Paris: Gallimard, 1983-88), 4:456.

<sup>35</sup> J. F. Helvetius, *Vitulus aureus, quem mundus adorat et orat* (Amsterdam, 1667).

<sup>36</sup> Saint-Simon, *Mémoires*, 4:459-66; *Aus der Briefe der Herzogin Elisabeth Charlotte von Orléans an die Kurfürstin Sophie von Hannover*, ed. Eduard Bodemann, 2 vols., (Hannover, 1891), 2:302-303, 307.

<sup>37</sup> Fontenelle, "Éloge de M. Homberg," *HARS* 17, (1715): 82-93 on pp. 87-88.

<sup>38</sup> Fontenelle, "Éloge de M. Lemery," *HARS* 17, (1715): 73-82 on p. 73.

<sup>39</sup> Wilhelm Homberg, "Observations sur la matiere fecale," *MARS* 13, (1711) pp. 39-47.

<sup>40</sup> Alice Stroup, "Wilhelm Homberg and the Search for the Constituents of Plants at the Seventeenth-Century Académie Royale des Sciences," *Ambix* 26, (1979): 184-202, on pp. 185-86.

<sup>41</sup> Principe, "Wilhelm Homberg: Chymical Corpuscularianism and Chrysopoeia"; my forthcoming *Transmutations of Chymistry*, ch. 1, completely rewrites Fontenelle's account of Homberg.

<sup>42</sup> NLB, Leibniz Briefe 768, fols. 53r-54v, on fol. 54r; Remond to Leibniz, 23 December 1715.

<sup>43</sup> Stroup, "Censure ou querelles."

<sup>44</sup> Étienne-François Geoffroy, "Des supercheres concernant la pierre philosophale," *MARS* 24, (1722): 61-70.

<sup>45</sup> Wolfgang Beck, *Michael Maiers Examen Fucorum Pseudo-chymicorum: eine Schrift wider die falschen Alchemisten*, Ph.D. 1992, Technische Universität München; Robert Halleux, "L'alchimiste et l'essayeur," in *Die Alchemie in der europaeischen Kultur- und Wissenschaftsgeschichte*, ed. Christoph Meinel (Wiesbaden: Otto Harrassowitz, 1986); Michael Maier, *Examen fucorum pseudo-chymicorum detectorum et in gratiam veritatis amantium succincte refutatorum* (Frankfurt, 1617); Heinrich Khunrath, *Treuhertzige Warnungs-Vermahnung* (Magdeburg, 1597).

<sup>46</sup> *Catalogus librorum Stephani-Francisci Geoffroy*, (Paris, 1731); I warmly thank Dr. Brigitte Van Tiggelen for kindly bringing the existence of this source to my attention.

<sup>47</sup> Fontenelle, *HARS* 24, (1722): 37-39.

<sup>48</sup> *Mercure Galant* (April 1722), pp. 96-97; (May 1722), pp. 122-25.

<sup>49</sup> Reported by Johann Thomas Hensing (1683-1726) in his "Dissertation sur la pierre philosophale," pp. 121-54 in *Mémoires littéraires*, ed. Marc Antoine Eidous (Paris, 1750), pp. 122-23: "M. le Régent a voulu que les Membres de l'Académie qui s'appliquent à la Chymie, travaillent de tout leur pouvoir à découvrir la Pierre Philosophale."

<sup>50</sup> National Archives, State Papers 78/166, fols 339-41; the text is excerpted from Fontenelle's eulogy of Homberg.

<sup>51</sup> *Journal des Sçavans* 1723 (8 March), pp. 147-51.

<sup>52</sup> Gustavo Costa, "Un Collaboratore italiano del Conte di Boulainviller: Francesco Maria Pompeo Colonna (1644-1726)," *Atti e Memorie dell'Accademia Toscana di Scienze e Lettere* 29, (1964):207-95, on pp. 218-19.

<sup>53</sup> Noël Antoine Pluche, *Histoire du ciel*, 2 vols. (Paris, 1757), 2:9-10.

<sup>54</sup> Kevin Chang, "Georg Ernst Stahl's Alchemical Publications: Anachronism, Reading Market, and a Scientific Lineage Redefined," pp. 23-43 in *New Narratives in Eighteenth-Century Chemistry*, ed. Lawrence M. Principe, (Dordrecht: Springer, 2007).

<sup>55</sup> Hjalmar Fors, "Occult Traditions and Enlightened Science: The Swedish Board of Mines as an Intellectual Environment 1680-1760," pp. 239-52 in *Chymists and Chymistry*.



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# Dangerous Liaisons or Unavoidable Associations: Quantum Chemistry at the Crossroads of Chemistry, Physics and Mathematics

Ana Simões\*

## Introduction

In 1967, the Swedish quantum chemist Per-Olov Löwdin (1916 -2000) in the introduction to the *International Journal of Quantum Chemistry* gave a definition of the then forty-year old discipline.<sup>1</sup> Written in a period in which quantum chemistry was experiencing intense growth in networking and in internationalisation and was exploring the potential of a promising instrument –the electronic digital computer– while simultaneously extending its domain to molecules of biological interest, the definition bears witness to the challenges posed at the time when contrasted with the previous state of things. It calls attention to a number of specific features of the subject-matter of quantum chemistry –the elucidation of the electronic make-up of atoms, molecules and aggregates of molecules; the interplay of theory, experiment, mathematics and computational algorithms in building the methodological apparatus of quantum chemistry; its relationship with the disciplines of mathematics, physics, and biology; and finally the assessment of the role of quantum mechanics in providing a unifying framework for the natural sciences and eventually for the life sciences.

It should be no surprise that the evolving relations of the new sub-discipline with respect to physics and mathematics caught the attention of early (and not so early) practitioners of quantum chemistry; who, implicitly or explicitly, had addressed this particular issue in scientific publications, textbooks, writings addressed to non-specialist audiences or via popular science writings. It has also caught the attention of the more historically or philosophically inclined contemporary scientists (physicists and mostly chemists) such as H. Primas, G. Woolley,

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S. Weininger, S. Shaik, W. Kutzelnigg, G. Frenking, R. Hoffman, P. Lazlo, just to name a few, who have participated or currently participate in many discussion forums, of which this conference is an example. Besides reflections offered by participants and chemical practitioners, historians and philosophers of science have contributed to the debate (in journals, journal issues, edited volumes, conferences, etc.).<sup>2</sup> The author's impression is that these debates have often remained trapped inside disciplinary territories, and therefore have not profited from complementary discussions on the same topics across borders.

Herein are discussed three issues which manifest the particularities of quantum chemistry, its epistemological as well as social characteristics, through the evolving articulations and rearticulations with chemistry, physics and mathematics. The first is to trace the historical evolution of quantum chemistry, by analysing those instances in its history when, to put it simply, quantum chemistry was either identifying itself primarily with mathematical physics or applied mathematics or following the semi-empirical approach so dear to chemists. The character of quantum chemistry has been formed through the gradual articulation of its relative autonomy both with respect to physics as well as with respect to mathematics. This paper attempts to argue for the historicity of this relative autonomy. The second issue is that the arguments to follow will not be solely based on what used to be called internalist considerations. Institutional parameters like the naming of chairs, university politics, networking, but also alliances quantum chemists sought to entertain with practitioners of other disciplines were quite decisive in forming the character of quantum chemistry. These two issues also bring forward an intriguing feature of the development of quantum chemistry, that is, its contingent character. It will become apparent that quantum chemistry could have developed differently, and the particular form it took has been historically conditioned. The third point to be discussed is that the gradually articulated relative autonomy of quantum chemistry, and the culture of quantum chemists which had been rather well formed by the early-1960s, was transformed dramatically with the advent of the first digital computers: the main liability of quantum chemistry, the impossibility to perform analytical calculations, was, all of a sudden, turned into an invaluable asset for the further legitimisation of electronic computers. In the early-1960s it appeared that a whole subject depended on this particular type of instrument in order to produce trustworthy results. For roughly 40 years quantum chemists had a large spectrum of methodological, philosophical, and ontological choices as well as a great flexibility in their (inter)disciplinary collaborations and alliances in order to form their idiosyncratic culture. But in a very short while electronic computers undermined the fundamental criterion with respect to which they had made their choices during this preceding phase:



though computations were still impossible to be performed analytically, they could now be made in ways all agreed to be reliable and reach a sophistication and accuracy dependent on the needs of each quantum chemist. The members of a whole scientific community who had, through an historically complicated process, achieved a consensus as to what exactly it is that they were practicing, all of a sudden, became subservient to the limitless possibilities of computations provided by digital electronic systems.

This review concentrates on the period starting with the emergence of quantum chemistry (1927) and ends in the mid-1970s, after the first decades of electronic digital computers. Here in, due to constraints of time and space, the discussions of the relationships of quantum chemistry to biology are excluded. This is a fascinating topic worthy debating especially following the extended use of electronic computers after the 1960s which enabled quantum chemistry to encompass macromolecules and molecules of biological interest.

## **Positioning quantum chemistry**

### ***Naming a new sub-discipline***

Evidence of the difficulties encountered in positioning the new field in relation to neighbouring areas such as chemistry, physics and mathematics lies in the multiplicity of names attributed to it extending well into the period when Löwdin wrote the introductory note to the new journal. Extra evidence includes the different names assigned to chairs occupied by its practitioners, the titles of journals used as outlets for their publications, the names of congresses on the topic, and in the descriptions of courses taught on the subject.

The new field has been called mathematical chemistry,<sup>3</sup> subatomic theoretical chemistry,<sup>4</sup> quantum theory of valence,<sup>5</sup> molecular quantum mechanics,<sup>6</sup> chemical physics,<sup>7</sup> theoretical chemistry,<sup>8</sup> as well as by the now standard term, quantum chemistry. Although hard to certain, the first appearance of the designation “quantum chemistry” in the literature is probably that due to Arthur Erich Haas (1884 -1941), the Professor of Physics in the University of Vienna who in 1929 published *Die Grundlagen der Quantenchemie*,<sup>9</sup> a collection of four lectures delivered to the Physico-Chemical Society in Vienna. While this designation was not commonly used during the 1930s when the sub-discipline was carving out its identity vis-à-vis neighboring disciplines, it was increasingly used in textbooks written during and after the 1940s,<sup>10</sup> and finally ascended to a journal’s title, thanks to Löwdin’s creation of the *International Journal of Quantum Chemistry* in 1967.

Increasing specialisation fostered by the steady application of computer programs in solving chemical problems gave way in 1980 to the appearance of the *Journal of Computational Chemistry* and to the split of quantum chemistry into two, computational and non-computational quantum chemistry.

The uncertainty over naming the new sub-discipline, extended over a period of at least 40 years within the overall context of the impossibility of analytical computations. It faded away with the acknowledgement of its autonomous status, to give way shortly to a discussion of new avenues of specialisation opened up by the appropriation of a new tool –the computer. At the same time it forced the community to assess its impact, to choose among diverging methodological viewpoints and cover broader areas of organic, inorganic and bio-chemistry. It also acted as a bond among different groups of practitioners, at a time in which computers were few, big and expensive, so that acquiring a share of computer time became an index of survival fitness. In a quite vivid way, the multiplicity of alternative names used in the first decades, succeeded by stabilisation into “quantum chemistry”, and followed by appending an adjective to the name as a mark of increasing specialisation (computational quantum chemistry, quantum biochemistry,) illustrates very forcefully the evolving identity of the new sub-discipline.

### ***The emergence of quantum chemistry: the appropriation of physics into the chemists’ culture***

The traditional narrative on the history of quantum chemistry as generally offered by chemists is built around the conflict between two alternative computational methods to deal with valence problems: the Heitler-London-Slater-Pauling valence bond method (VB) and the Hund-Mulliken method of molecular orbitals (MO). Elsewhere I have proposed an alternative scheme of historical analysis centered on methodological rather than on computational criteria. Specifically, that the views of participants on theory building and the role of theory in chemistry form a set of criteria that justifies a different classification: the Heitler-London approach versus the Pauling-Mulliken approach, or to put it briefly, the “German approach” versus the “American approach”.<sup>11</sup>

Walter Heitler (1904-1981) and Fritz London (1900-1954) accepted that the underlying laws governing the behavior of electrons were already known and, hence, to do chemistry meant simply to deal with equations which were soluble in principle even though in practice they may only produce approximate solutions. They insisted on an approach centred on the input from physics and mathematics not only in relation to the tools to be used but also as to foundational issues. Their

approach to quantum chemistry, dubbed the “German approach due” points to the existence of a group sharing the same values and including other physicists such as Friedrich Hund (1896-1997) and Erich Hückel (1896-1980), was grounded on the first principles of quantum mechanics.<sup>12</sup> Antagonistic to classical chemical modes of representation which relied on pictorial representations – called by Mary Jo Nye the “paper and pencil” tradition within chemistry,<sup>13</sup> they took seriously the inherent non-visualisability of quantum mechanics.

Linus Pauling (1901-1994) and Robert Sanderson Mulliken (1896-1986) thought differently about how the newly developed quantum mechanics could, in practice, be applied to problems of chemistry and, more specifically, to the problem of the chemical bond. By making ample use of semi-empirical methods involving a combination of quantum mechanics, empirical data and pictorial imagery, they developed their respective approaches, whose only criterion for acceptability was practical success. Most significantly, they both shared a common outlook on how to construct their theoretical schemata, on the constitutive features of their theories, on what the relation of physics to chemistry should be, and on the discourse they developed to legitimise their respective theories.

Especially until the late 1930s, there was a strong interaction between the “American” and the “British”, who in the meantime had entered the field, and the “German” communities. At the same time there emerged a consonance between the “American” and the “British” approaches in the aims, tools and methods to be used by the discipline. In a sense the “pragmatic” approach of the Americans with its stress on “chemistry” and “rough semi-empirical approximations” was succeeded and complemented by the British emphasis on “mathematics”. The first generation of British quantum chemists, which included J.E. Lennard Jones (1894-1954), D. Hartree (1897-1958) and C.A. Coulson (1910-1974), perceived the problems of quantum chemistry first and foremost as problems in calculation, and by devising novel calculation methods tried to bring quantum chemistry within the realm of applied mathematics.<sup>14</sup> Their undertaking was particularly effective if not as thrilling as that of the “Germans” or the “Americans”. In this new context, demand for extra rigor was not primarily a demand for a rethinking of the conceptual framework, but rather for developing as well as legitimising mathematical techniques and methods to be used in chemical problems, and that meant they had to get involved with applied mathematics.

This impressionistic characterisation of the discipline in its early days is presented with the purpose of illustrating how much the main task of early practitioners depended on the articulation of quantum chemistry as a sub-discipline within chemistry, partially autonomous in relation to physics and mathematics, despite

being grounded on the mathematical underpinning of quantum mechanics and on the appropriation of quantum mechanical concepts. At the same time their activities carved out an identity for quantum chemistry, some of them offered explicit reflections on the relations of chemistry to physics which stemmed from their daily experience either as participants in preparing the ground for quantum chemistry to appear or as founders and early builders of the new sub-discipline. G.N. Lewis (1875-1946) and N.V. Sidgwick (1873-1952) are examples of the first group and Pauling and Mulliken of the second group.

In a paper published in the first volume of the new *Journal of Chemical Physics* (1933), Lewis opposed the analytical features of chemical theories and the convergent method of chemists to the synthetic characteristics of physical theories and the divergent method of physicists. Chemical theories were, he said, grounded on a large body of experimental material from which the chemist attempts to deduce a body of simple laws which are consistent with the known phenomena;<sup>15</sup> physical theories postulate laws governing the mutual behavior of particles and then attempt “to synthesise an atom or a molecule”.<sup>16</sup>

Sidgwick, one of the most outspoken advocates of resonance theory in the U.K., gave a set of lectures on the covalent link in chemistry while visiting Cornell in the same year the *Journal of Chemical Physics* first appeared (1933).<sup>17</sup> The introductory lecture discussed “The relations of physics and chemistry.” Reasoning along the same lines as Lewis, Sidgwick stressed that the division of scientific knowledge into different provinces was a human-made construct grounded on the acceptance of an increasing scale of complexity in the objects of study when going from mathematics to physics, chemistry and biology. An obvious consequence of increasing complexity was that “the simpler the problem you are examining the more precise is the knowledge you can acquire of it”.<sup>18</sup> While the physicist can restrict his research to ideal systems and to tractable materials, the chemist is forced to extend his work to all pure substances. Therefore, his knowledge of their behavior is necessarily less detailed, less accurate, less deducible from first principles than that of the physicist, and to a still higher degree that of the mathematician.

Sidgwick noted that the frontiers between the various sciences were being crossed at an increasing pace. The line separating mathematics from physics was becoming blurred whilst that between physics and chemistry had vanished as a consequence of the recent development of chemistry along “molecular-mechanical” lines. He realised that “both sciences [physics and chemistry] are now examining the same problems. It is true that they use different methods, but they apply them to the same materials”.<sup>19</sup> He was soon to participate in the popularisation of

Pauling's resonance theory, which after all fulfilled his sharp assessment – it was an embodiment of structural theory, which he elected as the paradigm of a chemical theory, in “molecular-mechanical” lines. As Pauling went further, he was to claim a reformation of the whole of chemistry from the standpoint of resonance theory. This agenda had far reaching implications in the status of chemistry within the hierarchy of the sciences. Pauling believed in the “integration” of the sciences,<sup>20</sup> which he deemed to be achieved through the transfer of tools and methods, the most important kind of transfer being what he called the “technique of thinking”. He came to view chemistry, and specifically resonance theory, as playing a pivotal role within the physical and biological sciences, to such an extent that he regarded that chemistry now occupied the central place, formerly attributed to physics.

Mulliken did not go so far. He differentiated chemistry from physics in terms of the distinct attitudes of chemists and physicists: “Chemists love molecules, and get to know them individually (...) But what about physicists? My impression is that they are more concerned with fields of force and waves than with the individual personalities of molecules or of matter, except perhaps in the case of high-energy particles”.<sup>21</sup> He depicted the dynamic features of the relation of chemistry to physics in terms of a wave analogy. Tidal waves inundate chemistry from time to time. Big tidal waves are relatively rare, small waves are more frequent events. Big tidal waves are composed of small waves. Mulliken identified the first big tidal wave with the emergence of physical chemistry; the next big tidal wave with chemical physics, perhaps “a modern and even more physical variety of physical chemistry”.<sup>22</sup>

As has been shown, in the negotiations involving the status of quantum chemistry, a central place was occupied by the assessment of its relations to chemistry and physics either revealed by the actual practice of scientists or addressed explicitly in their considerations on the topic. Historians and philosophers of science have also addressed the same issue. They have repeatedly couched their discussions by referring back to Paul A.M. Dirac's 1929 statement as an illustration of the reductionist attitude of most physicists (or physically oriented scientists) involved in one way or another with the emergence of quantum chemistry. In fact, in the opening paragraph of his paper, “Quantum Mechanics of Many-Electron Systems”, Dirac (1902-1984) could announce that “the underlying physical laws necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known, and the difficulty is only that the exact application of these laws leads to equations much too complicated to be soluble”.<sup>23</sup>

A large number of historically interesting questions, offering a new perspective on the topic of reductionism, can be asked in relation to Dirac's claim. How did chemists themselves, or those who worked in the field that became known as quantum chemistry, react to Dirac's claim? Did they feel threatened by physicists who felt they could do their job better than themselves? Did they feel indifferent, or did they simply not care? One way to answer these questions is to examine the research papers of chemists that specifically *cited* Dirac's 1929 paper.<sup>24</sup> Was Dirac's paper cited often? Why was it cited? Did chemists specifically cite Dirac's opening paragraph, or just part of it? When they referred to Dirac's claim, why did they do it? What did they consider to be its implications? How did they react to them?

Analysis based on the "Science Citation Index" data base has revealed that seldom has Dirac's paper been cited for its introductory paragraph. Furthermore, analysis showed that the very few chemists who cited Dirac's claim did not take it as a philosophical statement. On the contrary, they took it as a *historical* prediction about the future of chemistry that was, in time, proven wrong. Chemists viewed Dirac's claim as a historical statement because of his inability to predict the importance of relativistic effects and exact computations for chemistry. Historians and philosophers could also look at Dirac's claim as a *historical* rather than as a *philosophical* statement, voiced by one of the most un-philosophical of the founders of quantum mechanics, and expressing his belief that chemistry would become part of physics, and by extension that theoretical chemistry would become an even more physical form of physical chemistry. Seen from this vantage point, Dirac failed to predict that quantum mechanics soon would become a major preoccupation of chemists, and not just of physicists. He did not foresee that a new breed of chemists would appear who shared a culture very different from the reductionist culture of physicists, who embraced different methodological and ontological commitments, and who in this way would be able to attack problems of quantum chemistry successfully.

***Parallel trends in disciplinary development: the uneasy relation of chemists with mathematics***

Perhaps, as remarked in an earlier paper,<sup>25</sup> reductionism is the physicist's epistemological world-view but not the chemist's, therefore if one wants to discuss a number of questions pertaining to chemistry reductionism might be a misplaced category. Perhaps the notion of reductionism expresses a point of view dear to physicists but not to chemists. Though physicists took for granted that chemistry

would be reduced to physics, chemists did not have the luxury of waiting for the fulfillment of that vision. Reductionism may have been a research agenda, but it was one that was impossible to realise, because right from the outset, neither physicists nor chemists could deal analytically with any other molecules except for the simplest of all and even then only in grossly approximate terms.

Are there any other dimensions to reductionism, whose discussion may be considered more fruitful in addressing the same set of problems? Here it is useful to discuss the uneasy relationship of chemists to mathematics and to argue that chemists' relationship with the appropriation of mathematics into their culture was far more complex and difficult than their appropriation of physics. And though the two cannot be considered as totally independent of each other, it can be argued that chemists were more resistant to accepting the use of mathematics rather than the physical concepts, and the physical techniques.

Like any form of appropriation, opinions on the appropriation of mathematics differed among the members of the chemical community –ranging from acceptance to resistance– and can be traced back to different periods of the development of chemistry.<sup>26</sup> The next focus is on identifying this uneasy relationship in the emergence of quantum chemistry. While initial statements by scientists such as Pauling and van Vleck announced the rise of “mathematical chemistry” by calling attention to the potentialities associated with the mathematical apparatus attendant to the formulation of quantum mechanics,<sup>27</sup> in the following period, most of those who successfully managed to establish quantum chemistry as a new sub-discipline were eager to point to the subordinate role of mathematics in the sense of its computational paraphernalia. This was not just a rhetorical strategy to cater to wider audiences, but became a constitutive ingredient of quantum chemistry itself.

Pauling managed to present a coherent treatment of the chemical bond which was appealing to the chemists because of its frequent reliance on the “chemists’ intuition”, and the use of a lot of existing experimental data to be able to explain or predict other experimental data.<sup>28</sup> Though he repeatedly stressed that the understanding of the nature of the chemical bond, built on the appropriation of the quantum-mechanical concept of resonance, was possible only because of the developments due to quantum mechanics, his use of detailed mathematical formulations was reduced to a bare minimum.

In his widely-read textbook, *Valence* (1952), Coulson argued for the mathematisation of quantum chemistry at the same time he considered that quantum chemistry should be understandable by chemists with no mathematical training. The



presentation of the principles of quantum mechanics was circumscribed to two introductory chapters, and in many instances mathematical results were illustrated or complemented by the extensive use of visual representations. This was an implicit acknowledgment that visualisability, instead of elaborate mathematics, still remained one of the constitutive features of chemistry. The message was clear: quantum chemistry is not another instance in the application of quantum mechanics but a new sub-discipline of chemistry. Coulson eagerly emphasised the special role played by the alliance of experimental results and chemical intuition in the suggestion of particular mathematical developments, to such an extent that he had no qualms in asserting that “the theoretical chemist is not a mathematician, thinking mathematically, but a chemist, thinking chemically”.<sup>29</sup> He insisted on this point time and again in meetings, lectures and review papers.

The role and importance of mathematics was to play centre stage not only in the articulation of Coulson’s practice as a scientist and textbook writer but also in popular science lectures addressed to wider audiences.<sup>30</sup> In the Tilden Lecture delivered before the Chemical Society and in his inaugural address as Rouse Ball Professor of Applied Mathematics, both delivered around the time *Valence* came out, Coulson expressed an opinion at odds with that voiced by Dirac in 1929, and asserted that the importance of mathematics for quantum chemistry was not to be found at the computational, but rather at the conceptual level. Quantum chemistry was presented as a branch of applied mathematics, an area positioned between pure mathematics on the one hand, and experimental physics and chemistry on the other, but should never become “an appendage of experiment,” just as in the same way it should never “degenerate into a bastard form of pure mathematics”.<sup>31</sup> The true contribution of quantum mechanics to chemistry was that it showed how the concepts of the experimental chemist fitted together, how “they have all one single rationale; and how this hidden relationship to each other can be brought out”.<sup>32</sup>

These cases have been noted not in order to make any conclusive argument about the relationship of chemists to mathematics, but rather as indicative instances of a trend among chemists which has often been bypassed in the historical and philosophical literature on quantum chemistry. As quantum chemists were assessing how physics could be appropriated into their own culture, there was a parallel and relatively independent discussion among them concerning their appropriation of mathematics. This discussion has gone unnoticed especially due to a shift of attention to the phase in which electronic digital computers were readily adopted in the hope of solving the mathematical difficulties insurmountable without them.



### ***Different cultures within quantum chemistry: the impact of electronic digital computers***

In the aftermath of WWII, in 1948, a meeting was convened in Paris to discuss the most pressing problems faced by all those interested in quantum chemical questions. Among those, that required from the community a concerted effort, were the calculation of molecular integrals involving more than two centres, their tabulation and the numerical results. Three years later, a small group of scientists gathered in Shelter Island, in 1951, to evaluate the results of the actions taken since 1948, and to outline major research strategies. Considered a “watershed”, the conference aimed at obtaining formulas for the troublesome multi-central integrals which acted as “bottlenecks” to the integration of Schrödinger’s equation in the *ab initio* manner. These formulae thus became available to the community of quantum chemists in standardised tables. While at first dependent on human calculations aided by desk calculators, the program soon evolved to form an efficient cooperative network that took advantage of the slowly increasing number of electronic digital computers available to the international community.<sup>33</sup> Their use in quantum chemistry made it possible to seriously consider the delineation of an extensive program of “completely theoretical” (*ab initio*) calculations. They turned into essential tools to calculate the time-consuming integrals of the increasingly sophisticated versions of the MO method (Pariser-Parr-Pople, Self Consistent Field, Hartree-Fock, Configuration Interaction, etc.) and in many instances replaced laboratory experiments as sources of new data, especially in the investigation of molecules otherwise inaccessible to experimentation. It was, in a way, an old dream come true. These calculations contrasted with those “semi-empirical” ones, in which the impossible analytical calculation of certain parameters was substituted by the introduction of their values as given by experimental determinations, and which had become one of the constitutive aspects of quantum chemistry since its early days.

The Conference on Molecular Quantum Mechanics held at Boulder, Colorado in June 1959, was the first major meeting of its kind since the Shelter Island Conference. It was also the first meeting where the many theoretical chemists started to realise that there were divisions separating into different groups within the community of quantum chemists. In the after-dinner speech delivered at the end of the conference, Coulson emerged as one of the more sensitive observers of this situation. For once, Coulson did not preach tolerance but advocated partisanship.<sup>34</sup> He announced the splitting of the community into two distinct groups – Group I included the *ab-initionists* who were interested in exact calculations in molecules including up to 20 electrons, and thus were eager to explore the poten-

tialities of electronic computers; Group II included the *a posteriorists* who remained faithful to semi-empirical methods and denied the importance of exact calculations for quantum chemistry. The split resulted from diverging views concerning the use of large-scale electronic computers.

It was however an oversimplification to reduce the difference among quantum chemists to a difference on their reliance on electronic computers. In their desire for complete accuracy, Group I appeared to be prepared to “abandon all conventional chemical concepts and simple pictorial quality in their results”. Against this, the exponents of Group II argued that chemistry is still an experimental subject, whose results are built into a pattern around quite elementary concepts. Coulson did not make any effort to conceal that his sympathies lay with the latter, and re-emphasised that the role of quantum chemistry is to understand these concepts, and to show the essential features of chemical behavior. Nevertheless, he was also aware that none of these concepts could be made rigorous.

The new turn in the discipline due to the impact of computers was discussed by many others. Did Coulson’s worries strike a sympathetic chord or was Coulson isolated in his assessment of the situation? It is useful to use as a probe the opinions of two quantum chemists whose research programs took advantage of the increasing relevance of computers for large molecules. One such opinion comes from the Italian Enrico Clementi (1931- ), one of Mulliken’s former students, working at the IBM Research Laboratory, expressed in the first volume of the *International Journal of Quantum Chemistry* (1967). Clementi was very assertive in claiming that computers could be extremely useful in the future if, and only if, one departed from the present trend in computational chemistry which pointed “toward the formation of an enormous library of wave functions with little attention to chemistry as such. This, of course, will lead to chemistry but only if we compute a very significant fraction of the possible molecules. Such a goal seems most unrealistic”.<sup>35</sup> He reacted against the increasing “computation” of the discipline if “computation” implied its exclusion from chemical problems. Quantum chemistry without chemistry seemed to be pointless. For him, the only meaningful way to use computers was to write computer programs able to cope with realistic chemical problems such as those occurring in nature. The mathematical model behind such endeavor was, of course, quantum mechanics with as many approximations as a chemical problem could afford to sustain “before becoming an irrational “soup” of floating numbers of questionable physical meaning”.<sup>36</sup> Then if the computer program was meant to solve a “synthetic chemistry problem”, it should be able to start from the component atoms and arrive at the final molecule. If the program was written to solve a “spectroscopic problem,” it should give the

basic spectroscopic constants. If the problem was a “structural problem” the computer program should give internuclear distances and electronic density mappings.

Also at the 1970 Symposium on *Aspects de la Chimie Quantique Contemporaine* held in Menton, France and organized by the C.N.R.S., Alberte Pullman (1920- ), one of the founders of quantum biochemistry in France, noted that the concern for getting better and better values of parameters, integrals, or other quantities, gave the impression that for some quantum chemistry aimed solely at “the reproduction of known results by means of uncertain methods,” contrary to the other sciences which aimed at “using known methods to search for unknown results”.<sup>37</sup> She recalled Coulson’s analysis at the Boulder Conference, and his views of the splitting of the community into contending parties, but did not endorse Coulson’s pessimistic evaluation. She predicted that a new period in the history of quantum chemistry was just starting and that quantum chemistry’s lost unity would soon be recovered. As a consequence of the development of techniques to study all valence electrons, and by extension all electrons, in molecular systems, the split between *ab-initio*nists and *a-posteriori*nists was to give way to the merging of both groups into one single group, which she named the *ab-initio for everybody* group. She added that she feared that “the only division that will persist between quantum chemists will be ... that between wealthy and poor, those who have the means to carry sophisticated calculations and those that do not have them”.<sup>38</sup> Above all, she hoped that the changes will “re-chemistrise” quantum chemistry.

In the interim, Coulson softened his position. He came to recognise that the divorce in the quantum chemical community which haunted him in the early days converged in time into a peaceful cohabitation of two different cultures of practitioners. Few months before his death, in the inaugural lecture as Professor of Theoretical Chemistry (1973) in the new Department of Theoretical Chemistry at the University of Oxford, Coulson recognised that the approaches of the two groups were not in mutual conflict. Both were needed, and complemented each other, so that “the particular approach which a person makes to the use of a computer almost determines his judgment on the relative merits of the two types of study”.<sup>39</sup>

Computers enabled numerical values to be obtained as accurate as those found with the best experiments. But, still, they were just a highly refined tool, like a spectroscope or a calorimeter. Even if Coulson assessed their status as an extra instrument available to chemists, whose ready adoption impinged on experiment – chemistry’s most central feature, he was sure that they were never going to replace laboratories and laboratory work. Their extensive use in quantum chem-

istry also prompted him to reassess the role of mathematics in chemistry and to go deeper in the differentiation of the inputs from physics, mathematics and computation in carving an identity for quantum chemistry. A computer gives numerical values of quantities, but cannot give explanations. To achieve an understanding of what is going on, concepts are needed, all of which lie outside the domain of strict observability, but all of which belong to the framework of chemical theory. And the ability to devise concepts of value, weaving them into the “growing pattern of chemistry,” is what characterises the great chemists and distinguishes them from the “numerologists”. The fun and interest of mathematics definitely did not lie in ever more sophisticated computations thanks to ever more powerful and eventually cheaper computers. In quantum mechanics as it evolved, mathematics had become central to the understanding of the chemical and physical behaviour of atoms and electrons in a way it had never been before. Previously, chemists used mathematics after a model had been devised to deal with some chemical situation, because an equation had to be solved. In these instances, to which chemists were already accustomed, mathematics was peripheral to the chemical situation, stemming as it were from the “outside”. Assessing the long, and at time tortuous, way traversed in the meantime, Coulson, one of the most insightful of all quantum chemists, reiterated once again that both physics and mathematics entertained central links to chemistry, not independent from each other, but never reducible to computations, however sophisticated they might become.

Acknowledging the existence of different trends among groups of quantum chemists, at first clearly antagonistic then becoming non-conflicting or even complementary, and dependent on their opposite views on the use of computers, meant, above all, the recognition that a new culture of doing quantum chemistry was asserting itself and was carving a place in parallel with the more traditional one. That this was possible without disrupting altogether the community of quantum chemists bears witness to its maturity level and the existence of shared values which resisted confrontation with new ones. The question cannot be reduced to the realisation that computers started more or less to dictate to quantum (and theoretical) chemists the kinds of problems they would work on and the ways to deal with these problems. In the process, a new culture emerged identified by a novel style of scientific thinking, in which the increasing complexity of molecular problems was dealt with by means of mathematical modeling, that is, in which the articulation of mathematical models and their computer simulation was accompanied by graphical, numerical or analytical representations.<sup>40</sup>

### **Concluding remarks ... or metaphors and their various meanings**

The ability to bridge boundaries between disciplines was perhaps the most striking and permanent characteristic of those who consistently contributed to the development of quantum chemistry. Moving at ease between physics, chemistry, and mathematics, became a prerequisite to be successful in borrowing techniques, appropriating concepts, devising new calculation methods and developing legitimising strategies. With the era of computers and the development of computer science, quantum chemists were among the first scientists to explore the potentialities of the new tool, and even to collaborate in its development. In this way, they also became participants in what many dubbed as the Second Instrumental Revolution in chemistry.<sup>41</sup> The discussion over changing practices and their implications for the evolving identity of quantum chemistry shows how the history of quantum chemistry illustrates one of the trends which more forcefully characterises the sciences in the twentieth century – the exploration of frontiers and the crossing of disciplinary boundaries, reinforced by the mediation of many new instruments and tools.

If in the case of quantum chemistry this process was associated with its progressive de-conceptualisation while computational and graphical methods took over is a question which still needs extra research. Here in is proposed an alternative metaphor, a change from a geographical one –involving territories, boundaries and bridges– to a biological one –centered on an artificial fiber. It relates to one of the participants in the story, Coulson. To highlight “how much the validity of the scientist’s account depends on the degree of interlocking between its elements”, Coulson called attention, in a quite different context from that of quantum chemistry, to the fact that “the strength of an artificial fiber depends on the degree of cross-linking between the different chains of individual atoms”.<sup>42</sup> In the same manner, one might argue that the explanatory success of quantum chemistry throughout successive developmental stages rested on the degree of interlocking among constitutive elements – chemical concepts, mathematical notions, numerical methods, pictorial representations, experimental measurements – to such an extent that it was not the relative contribution of each component that mattered, but the way in which the whole was reinforced by the *cross-linking* and *cross-fertilisation* of all elements. Furthermore its success depended not only on epistemological but also on social aspects of this *cross-fertilisation*. It involved the establishment and permanent negotiation of alliances among members of a progressively more international community of practitioners, intense networking, and adjustments and re-adjustments within the community, both at the individual,

institutional and at the educational level – in short it involved a gigantic rearrangement in the material culture of quantum chemistry.

## References and Notes

- <sup>1</sup> Per-Olov Löwdin, "Program," *International Journal of Quantum Chemistry* 1 (1967), 1-6, 1: "Quantum chemistry deals with the theory of the electronic structure of matter: atoms, molecules, and crystals. It describes this structure in terms of wave patterns, and it uses physical and chemical experience, deep-going mathematical analysis, and high-speed electronic computers to achieve its results. Quantum mechanics has rendered a new conceptual framework for physics and chemistry, and it has led to a unification of the natural sciences which was previously inconceivable; the recent development of molecular biology shows also that the life sciences are now approaching the same basis. Quantum chemistry is a young field which falls between the historically developed areas of mathematics, physics, chemistry, and biology."
- <sup>2</sup> Journals in which debates surfaced include *Synthese*; *Hyle*; *Foundations of Chemistry*; *British Journal History of Science*; *Studies in the History and Philosophy of Modern Physics*; *Studies in the History and Philosophy of Science*; *Historical Studies in the Physical Sciences*.
- <sup>3</sup> J.H. Van Vleck, "The new quantum mechanics," *Chemical Reviews* 5 (1928), 467-507; Application for the Guggenheim Fellowship, late 1925, Pauling Papers, Box 142, CIT Course Materials, Notes 1922-1930, Tau Beta Pi, CIT Transcripts, "Plans for Study".
- <sup>4</sup> Linus Pauling, "The application of quantum mechanics to the structure of the hydrogen molecule," *Chemical Reviews*, 5 (1928), 173-213.
- <sup>5</sup> J.H. Van Vleck and A. Sherman 1935, "Quantum Theory of Valence," *Reviews of Modern Physics*, 7 (1935), 167-227.
- <sup>6</sup> Conference on Molecular Quantum Mechanics, University of Colorado at Boulder, June 21-27, 1959.
- <sup>7</sup> This designation appeared for the first time with the creation of the *Journal of Chemical Physics* in 1933. Other journals used previously as outlets for papers on the new area included the *Journal of the American Chemical Society*, *Physical Review*, to be followed afterwards by journals such as *International Journal of Quantum Chemistry* or the *Journal of Computational Chemistry*.
- <sup>8</sup> J.E. Lennard-Jones occupied the first chair of Theoretical Chemistry in the world in 1932, but C.A. Coulson was just offered an equivalent chair in Oxford in 1973.
- <sup>9</sup> A.E. Haas, *Die Grundlagen der Quantenchemie* (Leipzig: Akade. Verl., 1929), English translation *Quantum Chemistry. A short introduction in four non-mathematical lectures* (London: Constable & Company, 1930), translated by L.W. Codd.
- <sup>10</sup> Examples are Henry Eyring, John Walter, George Kimball, *Quantum Chemistry* (Wiley Publishers, 1944); Kenneth Pitzer, *Quantum Chemistry* (New York, Prentice-Hall, 1953); Walter Kauzmann, *Quantum Chemistry, an Introduction* (New York: Academic Press, 1957).
- <sup>11</sup> Kostas Gavroglu and Ana Simões, "The Americans, the Germans and the Beginnings of Quantum Chemistry: The Confluence of Diverging Traditions," *Historical Studies in the Physical Sciences*, 25 (1994), 47-110.
- <sup>12</sup> A. Karachalios, "On the making of Quantum Chemistry in Germany," *Studies in the History and Philosophy of Modern Physics*, 31B (2000), 493-510.

- <sup>13</sup> Mary Jo Nye, "Paper Tools and Molecular Architecture in the Chemistry of Linus Pauling," in U. Klein, ed., *Tools and Modes of Representation in the Laboratory Sciences* (Dordrecht: Kluwer Academic Publishers, 2001), pp.117-132.
- <sup>14</sup> Ana Simões and Kostas Gavroglu, "Quantum Chemistry *qua* Applied Mathematics. The contributions of Charles Alfred Coulson (1910-1974)," *Historical Studies in the Physical Sciences*, 29 (1999), 363-406; Ana Simões and Kostas Gavroglu, "Quantum Chemistry in Great Britain: Developing a Mathematical Framework for Quantum Chemistry," *Studies in the History and Philosophy of Modern Physics*, 31 (2000), 511-548; Kostas Gavroglu and Ana Simões, "Preparing the ground for quantum chemistry to appear in Great Britain: the contributions of the physicist R.H. Fowler and the chemist N.V. Sidgwick," *British Journal for the History of Science*, 35 (2002), 187-212.
- <sup>15</sup> G.N. Lewis, *Anatomy of Science* (New Haven: Yale University Press, 1926), 172-174; G.N. Lewis, "The Chemical Bond," *Journal of Chemical Physics*, 1933, 1, 17-28; G. N. Lewis, *Valence and the Structure of Atoms and Molecules* (New York: The Chemical Catalog Company, 1923, reprinted by Dover Publications, 1966), pp. 20-21.
- <sup>16</sup> Lewis, "Chemical Bond," op.cit. (15), 17.
- <sup>17</sup> Gavroglu and Simões, "Preparing the ground", op.cit. (14).
- <sup>18</sup> N.V. Sidgwick, *Some Physical Properties of the Covalent Link in Chemistry* (New York: Cornell University Press, 1933), 3, 4.
- <sup>19</sup> *Ibid.*, 3.
- <sup>20</sup> Linus Pauling, "The place of chemistry in the integration of the sciences," *Main Currents in Modern Thought*, 7 (1950), 108-111 in Barbara Marinacci, ed., *Linus Pauling in his Own Words* (New York: Simon & Schuster, 1995), pp. 107-111.
- <sup>21</sup> R.S. Mulliken, "Spectroscopy, Quantum Chemistry and Molecular Physics," *Physics Today*, 21 (1968), 52-57, on 54.
- <sup>22</sup> *Ibid.*, 56.
- <sup>23</sup> P.A. M. Dirac, "Quantum Mechanics of Many-Electron Systems," *Proceedings of the Royal Society of London A* 123 (1929), 714-733, on 714.
- <sup>24</sup> Ana Simões, "Dirac's claim and the chemists," *Physics in Perspective*, 4 (2002), 253-266. I used the Science Citation Index (1945-1979) to identify all papers citing Dirac's paper. For the period (1929-1945) which is not covered by SCI I only carried a non-systematic search of journals that include papers on quantum chemistry. I just covered a 50-year period assuming that in this period Dirac's paper became "common property" so that citations diminished accordingly to become almost non-existent afterwards.
- <sup>25</sup> Ana Simões and Kostas Gavroglu, "Issues in the History of Theoretical and Quantum Chemistry 1927-1960," in C. Reinhardt, ed., *Chemical Sciences in the Twentieth Century: Bridging Boundaries* (Weinheim: Wiley-VCH, 2001), 51-74.
- <sup>26</sup> Some, like Frankland, van't Hoff, W. Ostwald, G.N. Lewis, pushed quite strongly for introducing mathematics into chemistry. Even J. Larmor and J.J.Thomson before him tried to propose a mathematical framework for dealing with chemical problems. But resistance to such programs came from different quarters.
- <sup>27</sup> Pauling Papers, Box 142, CIT Course Materials, Notes 1922-1930, Tau Beta Pi, CIT Transcripts, "Plans for Study"; J.H. van Vleck, "The new quantum mechanics," *Chemical Reviews* 5 (1928), 467-507, on 500, emphasis in original.
- <sup>28</sup> Linus Pauling, *The Nature of the Chemical Bond and the Structure of Atoms and Molecules. An Introduction to Modern Structural Chemistry* (New York: Cornell University Press, 1939)



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- <sup>29</sup> C.A. Coulson, *Valence* (Oxford: Oxford University Press, 1952), preface, on p. v.
- <sup>30</sup> Ana Simões, "Textbooks, popular lectures and sermons: the quantum chemist Charles Alfred Coulson and the crafting of science," *British Journal for the History of Science*, 37(3) (2004), 299-342.
- <sup>31</sup> C.A. Coulson, *The Spirit of Applied Mathematics* (Oxford, 1953), 11.
- <sup>32</sup> C.A. Coulson, "The Contributions of Wave Mechanics to Chemistry," *Journal of the Chemical Society* (1955), 2069-2084, 2069-2070. Coulson repeated and rephrased many of the arguments voiced in his inaugural lecture as Professor of Theoretical Physics at King's College, London, published as C.A. Coulson, "Wave Mechanics in Physics, Chemistry and Biology," *Science Progress* (1948), 36, 436-449.
- <sup>33</sup> W. Aspray, "Computer science and the computer revolution," in Mary Jo Nye, ed., *The Cambridge History of Science, vol.5. The Modern Physical and Mathematical Sciences* (Cambridge: Cambridge University Press, 2003) pp.598-614.
- <sup>34</sup> C.A. Coulson, "Present State of Molecular Structure Calculations," Conference on Molecular Quantum Mechanics, University of Colorado at Boulder, June 21-27, 1959, *Reviews of Modern Physics*, 32 (1960), 170-177.
- <sup>35</sup> Enrico Clementi, "Chemistry and Computers," *International Journal of Quantum Chemistry*, 1S (1967), 307-312, p. 308.
- <sup>36</sup> Ibid.
- <sup>37</sup> Alberte Pullman, "Propos d'Introduction. 1970: Bilan et Perspectives," *Colloque International sur les Aspects de la Chimie Quantique Contemporaine*, 8-13 July 1970, Menton, France, organized by R. Daudel et Alberte Pullman (Paris: Editions du CNRS, 1971), 9-16, on 13.
- <sup>38</sup> Ibid., on 14.
- <sup>39</sup> C.A. Coulson, *Theoretical Chemistry Past and Future* (ed. S.L. Altmann), Oxford, 1974, 20; CP, Ms. Coulson 36, B.16.4, 9.
- <sup>40</sup> S. Schweber, M. Wächter, "Complex systems, modelling and simulation", *Studies in the History and Philosophy of Modern Physics*, 31 (2000), pp.583-609. For them, computers are central components of a Hacking-type revolution – the "complex systems computer modeling and simulation" revolution. Computers generated sweeping transformations of the social, material, economic and cultural contexts, besides introducing a new style of scientific thinking.
- <sup>41</sup> Carsten Reinhardt, *Shifting and Rearranging: Physical Methods and the Transformation of Modern Chemistry* (Sagamore Beach, Mass.: Science History Publications, 2006). In a way this book belongs to a recent trend, to which have contributed authors such as Davis Baird, Christoph Meinel, Peter Morris, and Tony Travis, which pays particular attention to the role of instrumentation and the impact of the so-called Second Instrumental Revolution in chemistry.



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# The New Identity of Chemistry as Biomimetic and Nanoscience

*Bernadette Bensaude-Vincent\**

This paper aims at characterising the distinct features of the recent trends of biomimetic chemistry within the long tradition of chemistry challenging nature through the artificial creation of life. Through a survey of various strategies for mimicking biological materials and biological processes it will be argued that nanotechnology is revitalising the chemists' ambitions to answer the big questions about the origin of life and the universe.

## Introduction

The question of the disciplinary identity of chemistry has emerged as a major focus from several recent historical accounts. A number of historians describe the emergence of the autonomous discipline of chemistry through the eighteenth century before presenting the chemistry of nineteenth century as a "mature science". The biological metaphor so frequent in history of science conveys the image of a natural process, the smooth and unproblematic development of a positive science. The image of chemistry as a maturing discipline was shaped by chemists themselves. From Thomas Thomson in the early nineteenth to J.R. Partington in the twentieth century, via Hermann Kopp, Adolphe Wurtz, Albert Ladenburg, and Marcelin Berthelot, Edward Thorpe, Pierre Duhem, Ida Freund, Wilhelm Ostwald, and Ahron Idhe, to name only a few, chemist-historians have shaped an image of their discipline as a stable and powerful branch of natural sciences. So confident they were in the success of their discipline, that they never realized that the biological metaphor of the growth and maturity of disciplines would necessarily imply the question of its decay. If the analogy of disciplines with living beings is pushed one then should admit that disciplines are bound to die after their golden age.

Who would dare say that chemistry is an old, decaying discipline close to its death, because it no longer attracts students and suffers from a bad public image? However there are many visible symptoms of decay: chemical theory has been

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subjected to aggressive attempts at reduction in the twentieth century after Paul Dirac had claimed that the whole of chemistry could be deduced from the laws of quantum physics. Chemical technologies are now challenged by biotechnology in pharmaceutical industry, and will presumably be increasingly replaced by bio-processes and bio-products in the future. The golden age of chemistry is far behind us. Synthetic products are viewed as poisonous and chemical manufactures as nuisances.

Today chemists are struggling with what they view as a crisis of the public trust in chemistry. They complain about their low prestige, the lack of public recognition of their achievements, and the misguided popular associations of chemistry, with poison, pollution, hazards, death and sorcery. So deep is the depression among chemists today that they are ready to give up their chemical identity by eagerly embracing new labels for their activities, such as ‘molecular science’, ‘materials science’, or ‘nanotechnology’. Is it the death of chemistry?

What can we do as historians of chemistry in the face of this situation?

Two decades of science studies have taught many to avoid the essentialist pitfall. Chemistry is not like a living organism with a trajectory predetermined by nature. Like most sciences, chemistry has been socially and culturally constructed, its current profile is the result of negotiations with the scholastic culture that shaped universities in the early modern period, of repeated battles with the mechanistic paradigm, which came to prevail in the modern period, of repeated tensions with medicine, pharmacy and life sciences. The long-term perspective suggests that the current distrust of chemistry is nothing like a sudden fit, a kind of pathology in the life of the chemical discipline. In the socio-constructionist perspective, the present state of chemistry can be characterised as a new phase of long lasting struggles between chemists and the neighbour sciences, especially physics and the life sciences.

This paper will focus on the tensions between chemistry and life science in order to examine how negotiations at this interface can reconfigure the practice and the ambitions of chemistry.

## **Two faces of chemistry with regard to life sciences**

A glimpse at the *longue durée* suffices to reveal two contrasted postures of chemistry in its relation with medicine and biology, the modest attitude and the arrogant one.

### *1) Chemistry as a service science*

The modest attitude can be illustrated in the 18<sup>th</sup> century when chemistry emerged as an academic discipline. It is clear that the urgency of chemistry for pharmaceutical and medical training was the foundation of the establishment of Chemistry Chairs in many European Universities. In addition, dozens of chemistry courses, public or private, free or charged were delivered for training medical students and apothecaries among others. In an age when training practitioners and enlightening the public were not two separate activities, public experimental demonstrations constructed chemistry as a fashionable and legitimate science. Chemistry enjoyed high prestige and was integral part of the *philosophes'* culture.<sup>1</sup> Moreover the medical and pharmaceutical audiences deeply determined the agenda of chemical research. For instance plant chemistry was one of the first research programs ever conducted, at the Paris Royal Academy of Science in the eighteenth century mainly for studying the medical virtues of plants. For this purpose new analytical techniques – solvent analysis – had to be developed and a new notion of constituent element – as proximate principle - was developed.<sup>2</sup> Medical and pharmaceutical applications fostered the advancement of both chemical science and chemical technologies. Pharmaceutical applications played a key role in the emergence of synthetic industries as well as in understanding biochemical processes. Thus it is clear that the modest attitude, servant of liberal arts such as medicine, was not an obstacle to the advancement of chemical knowledge.

### *2) Promethean chemistry*

The alternative attitude of chemists playing God or at least mimicking life and improving on nature is more familiar because it has deeply influenced the public image of chemists. Most historical examples became legendary. Paracelsus is said to have tried to make an *homunculus* by maceration of sperm in manure over forty nights. The legend of Faust (the scholar who signed a pact with the Evil) originated from a true 16<sup>th</sup> century character, a German alchemist and astrologist named Johann Faust, who boasted his achievements in magic and necromancy. Mary Shelley's *Frankenstein* revived the image of the chemist playing God in the nineteenth century before chemists themselves revived their Faustian ambitions with the emergence of synthetic chemistry. The legend of the synthesis of urea in 1828 as the death sentence of "vital forces" was forged and propagated by chemists such as Hermann Kolbe, Wilhelm August Hofmann and Berthelot.<sup>3</sup> They claimed that the metaphysical belief in a vital force was destroyed by the synthesis of an organic compound previously synthesised by living organisms.

Wohler's synthesis was presented as an epoch-making discovery, the dawn of a new era, where chemists would be able to create organisms.

In reality, the vital force concept was not swept away by the synthesis of urea. As John Hedley Brooke argued, this is a biased interpretation of this synthesis<sup>4</sup>. First the claim is non acceptable since Wohler's synthesis was not a direct synthesis from elements but only a partial synthesis from a cyanate. Second, the anti-metaphysical claim rests on confusion between products and process. Urea is an organic substance but not an organism; it is a product of life but it was not synthesised through the same process in the organism. It was thus easy for physiologists such as Claude Bernard to state that chemists could certainly imitate the products of life but could not imitate the ways of nature. Nevertheless it is clear that the ambition to rival nature and to improve on it has encouraged the advancement of chemical science.

Despite the strong contrast here outlined, modest and arrogant chemists share at least one common attitude. While nineteenth century chemists made efforts to expand the territory of chemistry to physiology they were less inclined to provide a chemical explanation of life processes than eager to interface with physiology and agriculture. In short their ambition was less representing than intervening. Paraphrasing Ian Hacking's words, I would like to emphasise a major and constant feature of the identity of chemistry, "Chemists are laboratory workers, they are learning about matter through making materials". As Gaston Bachelard noticed they rely on facticity to understand nature. This is how he interpreted Berthelot's famous statement: "Chemistry creates its object".<sup>5</sup> Knowing through making, making things and making them pure, as artefacts, is the chemist's approach to nature.

## **The Nanotechnology Challenge**

### *1) What is new with nanotechnology*

Nanotechnology is minimally defined by, design at the nanoscale (1-100 nanometers).

"Working at the atomic, molecular, supra-molecular levels, in the length scale of approximately 1-100 nm range, in order to understand, create and use materials, devices and systems with fundamentally new properties and functions because of their small structure".<sup>6</sup>

I will retain three major features:

- At this scale it is possible to visualise and address a single molecule rather than  $N$  (Avogadro number) of molecules.
- At this scale the boundary between inorganic and organic matter no longer makes sense. DNA is a molecule rather than “the secret of life”. Nano and biotechnology work together.

Molecules, macromolecules as well as genes and proteins, all building blocks of matter and life are viewed as machines performing specific tasks.

This domain instantiates what science policy advisers name the new regime of knowledge production.<sup>7</sup> A regime characterised by the dogma of inter-disciplinarity and networks of research including academe and industrial companies. Research in nanobiotechnology blurs the boundaries between academic disciplines such as physics, chemistry, and biology as well as chemical electrical mechanical engineering. Various combinations are being developed from molecular genetics to synthetic biology that may deeply affect the identity of chemistry and even bring about the end of chemistry as a discipline of its own. What historians of chemistry do have to say about the future of chemistry?

## *2) The death of chemistry*

In 1986, the Nobel Prize for Chemistry was awarded by two IBM researchers Binnig and Rohrer for the Scanning Tunelling Microscope, an instrument emblematic of a new approach to materials synthesis. With STM and AFM scientists cannot only visualise individual atoms but also manipulate them. It opened up a new avenue of research, portrayed in the US National NanoInitiative in 2000 as, “shaping the world atom by atom”.

In 1986, Eric Drexler from MIT described the coming new era in a popular book *Engines of Creation*. He advocated a radically new technology that will handle individual atoms and molecules to be clumped together like the elements of Lego construction sets. Molecular manufacture will make clean and efficient products. By contrast, current organic synthesis is described as an awkward manner of making complex molecular chains by putting molecules together in a vessel, then stirring and hoping that the molecules will fall in the right place to make the desired product.

“Chemists have no direct control over the tumbling motions of molecules in a liquid, and so the molecules are free to react in any way they can, depending on how they bump together. Yet chemists nonetheless coax reacting molecules to form regular structures such as cubic and dodecahedral molecules, and to form unlike-

ly-seeming structures such as molecular rings with highly strained bonds. Molecular machines will have greater versatility in bond making, because they can use molecular motions to make bonds, but can guide these motions in ways that chemists cannot”.<sup>8</sup>

To the champion of molecular manufacture chemical synthesis is a primitive technology. It belongs to the ancient tradition of bulk technology handling billions of atoms that was initiated by flint chipping and is still used for making microcircuits. Chemical synthesis operating on billions of molecules is described as a messy, dirty and hazardous way of manufacturing artefacts. Whereas in Drexler’s ideal molecular manufacture nano-robots pick and place individual atoms to make molecules, chemists rely on the haphazard motions of crowds of molecules in a liquid. Whereas nanotechnologists, just as genetic engineers program molecular machines to perform specific tasks, synthetic chemists cannot control the assembly process of chemical reagents in their vats according to specific plans. Whereas molecular manufactures will be clean and environment-friendly, chemical plants are dirty and polluting. They always expose people to hazards and dangers, while molecular manufactures will be safe. So striking is the contrast between the old and the new styles of synthesis, between top down and bottom up approaches that Drexler wondered: “It is amazing that chemists are able to do anything at all, and in fact, they have impressive and growing accomplishments”.<sup>9</sup>

This depreciative evaluation of chemical synthesis prompted strong reactions in the chemical community. First Drexler’s concept of molecular manufacture has been submitted to merciless criticisms from chemists. Richard Smalley, George Whitesides, and other chemists argued that it was a chemical non-sense.<sup>10</sup> Drexler thinks of molecules as rigid building blocks, that can be assembled like the parts of toys to perform mechanical functions. Drexler’s machines are non feasible because they are not adapted to the special features of the nano-world. As Whitesides emphasised a nanoscale submarine would be impracticable because of Brownian motion, which would make useless all efforts to guide the submarine. For most chemists Drexler is a visionary who knows nothing about molecules. Chemists are the true experts in the molecular world, they have been doing molecular assemblies for centuries and they know that atoms cannot be handled as Lego set constructions. For most of them Drexler’s depreciative description of chemical synthesis emphasises the skills and genius required for making a successful synthesis. They read his statement as a eulogy, in praise of synthetic chemists, who like to portray themselves as artists.<sup>11</sup> Thus in response to the revolutionary claims of the champions of nanotechnology chemists revive the conventional image of the chemist-artist, which was fashionable in the 18<sup>th</sup> century, for instance in the entry ‘chemistry’ written in Diderot’s *Encyclopédie*.

## Changing Practices of synthesis

What can we do with such rhetorical claims on both sides? We have to check them against the real practices of design at the molecular level. Over the past decades how did chemists meet the challenges of nanotechnology and biotechnology?

### *1 Rational design*

New design techniques have been developed that change the self-representation of chemists as artists. The use of computers deeply transformed chemical synthesis as many other activities. Twentieth-century chemists, material scientists and pharmaceutical chemists have developed a variety of computer-assisted methods often referred to as “rational design” by contrast with the empirical, serendipitous processes of synthesis used in the past. Many algorithms are now available for designing molecules with interesting medical, magnetic, optical, or electronic properties., using computation, combination, randomisation.

Computational chemistry is a kind of bottom-up technology based on quantum theory and computers. It was initially basic research close to physics. It was aimed at building up a material *ab initio*, using computer calculations and starting with the most fundamental information about the atoms and the basic rules of physics. But computers can also be used to make molecular mechanics models of large systems for industrial purposes.<sup>12</sup> The technique is a way of avoiding the cost of synthesis. The idea is to find out how well a new compound works before it has been made by modelling its chemical behaviour on a computer. Three different perspectives are used: thermodynamic features, electronic properties and the spatial, molecular conformation. By visualizing the 3-D structure of a compound and rotating it, one can predict how a given molecule interacts with a protein.

Combinatorial chemistry is a different strategy. It consists in reacting a set of starting materials in all possible combinations. The computer eliminates all serendipity in the process of synthesis.<sup>13</sup> Once a the route for synthesis has been selected and optimised, in a few steps and a few months thousands of compounds are synthesised with no other purpose than being systematically stored in a “library” of substances. Then with the help of computer “evolutionary algorithms”, a fittest structure for specific targets will be selected.

### *2) Bio-inspired chemistry*

Another possible response to the nanotechnology challenge is to be found in bio-inspiration.

Whereas 19<sup>th</sup> and 20<sup>th</sup> century chemists challenged natural products with their synthetic products, by the end of the 20<sup>th</sup> century, living creatures were reconsidered either as a source of raw materials desirable for environmental concern or as a source of inspiration for synthetic chemists.<sup>14</sup> Materials scientists aiming at designing composite structures or materials by design (for specific applications) realized that biomaterials present optimal combination of properties and adaptive structures. Sea-urchin or abalone shells are wonderful bio-mineral structures made out of a common raw material calcium carbonate. They present a complex morphology and assume a variety of functions. Similarly, the spider's silk is a fiber extremely thin and robust that offers an unchallenged high strength-to-weight ratio. Wood which originally was the archetype of material is now redefined not only as a composite material made out of long, orientated fibers immersed in a light ligneous matrix but also as a complex structure with different levels of organization at different scales. Nature seems to provide elegant solutions to the problems tackled by modern chemists.

Biomimetic strategies thus prompted new collaborations between biologists and chemists, sometimes under the umbrella of a new discipline Materials Science and Engineering.<sup>15</sup> Biomaterials taught many lessons to chemists: first, most of them are multifunctionnal and offer a good compromise between various functions. Second, biomaterials unlike chemical products are not afraid of impurities, defaults, mixtures, and composites. Third, access to their fine structure reveals that biomaterials present a complex hierarchy of structures with structural features occurring on different size scales.

However bio-inspired chemistry is not confined to attempts at mimicking the exquisite hybrid structures of biomaterials. Nanotechnology prompted a new chemical challenge: how to self-assemble molecules? For designing at the nanoscale, human hands and tools are helpless.<sup>16</sup> Biomaterials rely on a more elegant solution since the building blocks self-assemble without the mediation of assembling tools. Self-assembly is ubiquitous in living systems, and it is extremely advantageous from a technological point of view because it is a spontaneous and reversible process with little or no waste and a wide domain of applications. Two very different strategies –hybridization or mimicry– are being developed to get the self-assembling of molecules.

Using the building blocks of living systems for making devices and machines is just taking advantage of the devices selected by biological evolution. Given that there is little chance that we can emulate nature, who spent billions of years for designing and perfecting high-performance structures, it seems more reasonable to start from the building blocks provided by nature in order to achieve our own



goals. For instance, it is not too difficult to take advantage of the potentials of DNA to make structures at the nanoscale. It is routine practice today in a number of laboratories to use complementary DNA strands for making nanotransistors, or other circuits. In this strategy, chemistry gives way to genetic engineering. By re-combining DNA, bio-engineers use it as a program to make new structures that they control with Atomic Force Microscopy. Steven Boxer, a chemist from Stanford who uses proteins as transistors in electronic circuits, thus describes his strategy: “We’ve decided that since we can’t beat them (biomolecular systems), we should join them”.<sup>17</sup> This hybrid strategy of design uses the building blocks of biosystems –DNA, proteins, bacteria, micelles or colloids– as molecular machines that are re-engineered for technological purposes. Does it mean that nanobiotechnology will bring about the death of chemistry?

The alternative strategy is to mimic the biological processes of self-assembly by using thermodynamics and chemical properties. The challenge that contemporary chemists have to face is to dispense with the information of the genetic code in order to self-assemble the components and to control morphogenesis. To meet this challenge chemists call all the resources of physics and chemistry: chemical transformations in spatially restricted reaction fields, external solicitations like gravity, electric or magnetic fields, mechanical stress, gradients and flux of reagents during the synthesis. They are also playing with a wide spectrum of weak bonds –Hydrogen bonds, Van der Waals forces–, etc. –instead of making and breaking covalent bonds between atoms.

### *3) Chemistry at the school of nature*

Chemists are learning many lessons at the school of nature. In fact, a whole range of novel chemical practices are being developed by biomimetic chemists.

A major lesson retained from nature is that living organisms conjugate inorganics and organics in the making of biomaterials and use templates, i.e. scaffolds that direct the inorganic structure formation. The use of soft moulds to shape hard materials is a key to achieve the synthesis of inorganic materials with all sorts of curved shapes. This branch of chemistry has been recently renamed “nanochemistry” because biomimetic processes are bottom-up syntheses performed at a few nanometres length-scale.<sup>18</sup>

In stark contrast with conventional organic chemistry, which operates at high temperatures, in high vacuum and with organic solvents, a new style of chemistry operates at room temperature, in rather messy and aqueous environments, just as nature does. This chemistry, named “chimie douce” (soft chemistry) by Jacques

Livage in 1977, aims at synthesizing original materials by performing reactions under quasi-physiological conditions, with biodegradable and renewable by-products and with an economy similar to that of nature.

Another branch of chemistry, no longer confined to the interactions between atoms and molecules using strong covalent bonds, is named “supramolecular chemistry” by Jean-Marie Lehn in 1978, and consists in using building blocks such as macromolecules, aggregates and colloids. According to Lehn, its objective is to reproduce the selectivity of the interaction between receptors and substrates in biology, with the help of hydrogen bonds and stereochemistry.

A more recent branch, dynamic combinatorial chemistry –also developed by Lehn– relies on the collective behavior of molecules for getting self-assembly. Lehn summarizes his credo in a simple formula: a glass of water has properties different from a water molecule. The components mixed in a solution explore the possibilities of binding and this dynamics ends up with the correct double helix. Unlike the lock and key static model of recognition, which presupposes that the correct target has been identified, in this process the lock and the key select each other, through a random process of interactions. The basic concepts are “from static to dynamics, from real to virtual, and from prefabricated to adaptive”.<sup>19</sup>

## **New Ambitions for Chemistry**

Self-assembly seems to open a new path for emulating nature’s processes. Are we witnessing a resurrection of the Faustian ambitions of alchemists and synthetic chemists? Nineteenth-century chemists could certainly synthesise the products of life but they failed to imitate the ways of nature in their vessels and furnaces. By contrast, today chemists are working hard to reproduce nature’s processes. The current intensive trend of research on self-assembly could thus bring a landmark in the longstanding rivalry between chemists and biologists.

Indeed, mimicking does not mean reproducing life. For most chemists, it is no longer a question of competing with nature to prove that life can be reduced to the interplay of chemical forces. On the contrary, many contemporary chemists acknowledge and emphasize the differences between the strategies used in the evolution of life and those invented by the laboratory chemist.

However, self-assembly has revived the chemists’ ambition to access the “essence of life”. They hope to self-organize complex metastable structures instead of well-

ordered materials, and thus maybe to shed a new light on the old problem of the origin of life.

For Lehn, controlling the basic forces of self-organisation is the ultimate aim of chemistry. His program of “Constitutional Dynamical Bionanotechnology” revives the greatest ambitions for chemistry as he assumes that something emerges from the collective behaviour of molecules, which results from coupling processes rather than just expressing information contained in the components. As Philip Ball rightly points out, chemists are now addressing the “big questions” about the Big Bang and the origin of life. Some of them are even ready to go further and even attempt to unveil the secret of the emergence of consciousness. For instance, George Whitesides assumes that chemical language can decipher the most complex phenomena: “The nature of the cells is an entirely molecular problem. It has nothing to do with biology”.<sup>20</sup> And since neurons also use chemical mediators, chemists should also be able to contribute to merge silicon electronics with the brain.

In conclusion, although it is risky for historians of science to predict the future, let me venture some remarks on the possible future of chemistry. The current trans-disciplinary regime of scientific research will not bring about the death of chemistry. Far from losing their identity, today chemists are responding to the new challenge of nanobiotechnology by developing new synthetic practices and novel styles of chemistry. Some of them are even reviving the most arrogant attitude as they expand their territory and want to address the big metaphysical questions.

Moreover, as a technoscience, a science combining knowing and making, a science focused on performance and productivity, a science networking a variety of different actors, chemistry could well become THE model science for the twenty-first century.

## Notes

<sup>1</sup> See Christine Lehman, “Between commerce and philanthropy: Chemistry courses in eighteenth-century Paris”. in B. Bensaude-Vincent, C. Blondel eds, *Science and Spectacle in the European Enlightenment*, Adelshot, Ashgate, 2008, pp. 103-116.

<sup>2</sup> F.L. Holmes, *Eighteenth-Century Chemistry as an Investigative Enterprise*, Berkeley, Office for the history of science, 1989. Mi Gyung Kim, *Affinity, That Elusive Dream*, Cambridge, MA: MIT Press, 2003.

- <sup>3</sup> Brooke, J.H. 1968, "Wöhler's Urea and its Vital Force—A Verdict from the Chemists." *Ambix* 15 (1968):84-114. Ramberg, P., 2000: 'The Death of Vitalism and the Birth of Organic Chemistry: Wöhler's Urea Synthesis and the Disciplinary Identity of Organic Chemistry', *Ambix* 47: 170-95.
- <sup>4</sup> Brooke, John Hedley, "Wohler's urea – a verdict from the chemists", *Ambix*, 15, 1968, reprint in *Thinking about Matter. Studies in the History of Chemical Philosophy*, (Aldershot, Variorum, 1995), pp. 84-114.
- <sup>5</sup> Marcellin Berthelot, *La synthèse chimique*, Paris, G. Baillière, 1876, quoted from the 8<sup>th</sup> edition Paris, Félix Alcan, 1897, p. 275.
- <sup>6</sup> Roco, M.C. Williams R.S., Alivisatos P. *Nanotechnology. Research Directions IWGN Interagency Working Group on Nanoscience Workshop Report*, Dordrecht, Boston, Kluwer, 2000.
- <sup>7</sup> M.Gibbons, C.Limoges, H.Nowotny, S.Schwartzman, P.Scott....*The New Production of Knowledge: the Dynamics of Science and Research in Contemporary Societies*, Sage, 1994. H.Nowotny, M.Gibbons, P.Scott, *Re-thinking Science: Knowledge and the Public in an Age of Uncertainty*, Polity Press & Blackwell Pub, 2001.
- <sup>8</sup> Eric K. Drexler, *Engines of Creation, The Coming Era of Nanotechnology*, New York, Anchor Books, 1986, p. 13.
- <sup>9</sup> Drexler, Eric K. "Introduction to nanotechnology" in Markus Krummenacker, James Lewis (eds) *Prospects in Nanotechnology. Proceedings of the 1<sup>st</sup> general conference on nanotechnology: developments, applications, and opportunities* nov 11-14, 1992, Palo-Alto, John Wiley & Sons 1995, p. 2.
- <sup>10</sup> See Smalley, Richard, "Of Chemistry, Love and Nanorobots". *Scientific American*, Sept. 2001, 76. Whitesides, George M., "The Once and Future Nanomachine", *Scientific American*, Sept. 2001, 78-83. See also Jones, Richard, L., *Soft Machines*, Oxford, New-York, Oxford University Press, 2004.
- <sup>11</sup> See Roald Hoffmann, "In praise of synthesis" in *The Same and Note the Same*, New York, Columbia University Press, 1995, pp. 94-100.
- <sup>12</sup> On computational chemistry see Francoeur, Eric, "Cyrus Leventhal, the Kluge and the origin of interactive molecular graphics", *Endeavour*, 26 (1) 2002, 127-131.
- <sup>13</sup> X.-D. Xiang et al. *Science*, 268 (1995), 1738 ; Xiang, *Annual Review of Materials Science*, 29 (1999), 149.
- <sup>14</sup> M. Sarikaya, I. Aksay (eds.), *Biomimetics : Design and Processing of Materials* (Woodbury, AIP Press, 1995. C. Sanchez (ed.) *Biomimétisme et matériaux*., Paris, Observatoire français des techniques avancées, vol. 25, 2001, pp. 139-143.
- <sup>15</sup> B. Bensaude-Vincent, "The construction of a discipline: materials science in the USA", *Historical Studies in the Physical and Biological Sciences*, 31, Part 2 (2001) 223-248,
- <sup>16</sup> It is to circumvent this obstacle that Drexler imagined "universal assemblers" for his molecular manufacture. The contrast between this mechanosynthetic strategy and the self-assembly in living matter has been emphasized by Richar Jones op. cit. supra.
- <sup>17</sup> Steven Boxer "Exploiting the Nanotechnology of Life", *Science*, 254, 29 November 1991, 1308-09.
- <sup>18</sup> See Geoffrey Ozin, André C. Arsenault, *Nanochemistry: A Chemical Approach to Nano-materials*, London, Royal Society of Chemistry, 2005, p. 10.
- <sup>19</sup> Lehn Jean-Marie, "Dynamic combinatorial and virtual combinatorial libraries", *European Journal of Chemistry*, 5, N°9, 1999, 2455-2463. "Toward self-organisation and complex matter", *Science*, 295, 29 March 2002, 2400-2402.
- <sup>20</sup> Whitesides quoted by Ball, Philip "What chemists want to know", *Nature*, 442/3, August 2006, 500-502, on p. 501.

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# Chemistry in the Low Countries: A Comparison Between North and South, 1600-1900

*Ernst Homburg\**

## Introduction

Aim of this paper is to compare the social history of chemistry in the North (the Netherlands) with in the South (Belgium) of the Low Countries. Many similarities, also many contrasts will be emphasised. To understand these similarities and differences in the social development of chemistry attention will be paid to:

- the differences in economic development,
- the social differences,
- the political factors,
- the cultural and religious factors,
- and in particular, to the influences emanating from the close subject area neighbours of chemistry: medicine and pharmacy; mining and metallurgy; industry; and esoteric alchemy.

First it is necessary to define more closely the geographical boundaries of the area studied, since these boundaries shifted over time. Five major time periods can be distinguished, between 1600 and 1900:

### *(1) 1600-1648: a period of war*

These were the years of the 80 years war (1568-1648) between Protestants and Catholics; and between Spain, France and the Republic (1579). In Germany these were the years of the 30 years war (1618-1648). The peace treaties of Westphalia and Münster 1648 defined the border between the north and the south.

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*(2) 1648-1795: four major political entities*

The 'North' consisted in this period of:

- the Republic of the Seven United Provinces;
- and of areas ruled by the States General (States' Brabant, Maastricht (partly), etc.).

The 'South' mainly consisted of:

- the Southern Netherlands, under Spanish rule until 1713, and under Austrian rule from 1715 to 1795;
- and of the Episcopate of Liège, ruled by a Prince-Bishop.

*(3) 1795-1813: a period with a strong French influence*

The South were occupied by France from 1795 to 1813, and the North from 1810 to 1813, but also between 1795 and 1810 the French influence in the North was very strong.

*(4) 1815-1830; the years of United Kingdom of the Netherlands*

The North and the South were united during these years, which ended with the Belgium revolution.

*(5) 1830-1900: Belgium and the Netherlands were two independent states*

As until today.

During all five time periods the North and the South also had many things in common. In the first place, this part of Europe was quite densely populated. During the years 1600-1900 more people lived in the South, compared to the North. This distribution changed completely during the 20<sup>th</sup> century (see Table 1).

The Low Countries were not only densely populated, they also were highly urbanised during the entire period. Between 1600 to 1800, before the Industrial Revolution, 40% of the population of the Republic lived in cities, and in the Province of Holland, 60%. In the 16<sup>th</sup> centuries cities such as Antwerp, Ghent and Bruges had populations between 30,000 and 100,000 inhabitants. After the outbreak of the Dutch revolt in 1568, the cities in the South went into decline. The population of Antwerp sank from 100,000 to 40,000 between 1580 and 1620, and the population of Ghent from 50,000 to 30,000. At the same time the towns in the

**Table 1**  
***The population of the North and the South, 1600-present (x 1,000,000)***

Year	North	South
1600	1,4	-
1650	1,8	-
1700	1,8	-
1750	1,8	2,0
1800	1,9	3,2
1850	3,1	4,3
1900	5,1	6,7
Present	16,0	10,0

North grew tremendously, partly by the influx of highly skilled refugees from the South: Amsterdam from 40,000 to 100,000 inhabitants, Leiden from 23,000 to 44,000, and Harlem from 15,000 to 39,000 (Table 2).

**Table 2**  
***The population of the largest towns of the Low Countries, 1580-1620***

Town	Population
Antwerp	100,000 > 40,000
<i>Amsterdam</i>	40,000 > 100,000
Ghent	50,000 > 30,000
<i>Leiden</i>	23,000 > 44.000
<i>Harlem</i>	15.000 > 39,000
Bruges	30,000 > ??

*italic* = towns in the North.

For a discipline such as chemistry, which in the early modern period strongly depended on urban life (e.g. pharmacists, universities, trade, industry), this high degree of urbanisation of the Low Countries is something to be noted, explicitly. It should also be noted, that there were many medium-sized towns, but, even in the 19<sup>th</sup> century, no true metropolis in the Low Countries, comparable to London, Paris, Vienna and Berlin. Amsterdam, and later Brussels, would come closest to those examples (see Table 3), but social life in the Netherlands and Belgium was too decentralised for the emergence of a true metropolitan culture. The strong development of metropolitan chemistry in cities such as London, Paris and Berlin, did not have an equivalent in the Low Countries.

**Table 3**  
***The population of the largest towns of the Low Countries around 1800***

Town	Population
<i>Amsterdam</i>	200,000
Brussels	66,000
Antwerp	56,000
<i>Rotterdam</i>	55,000
Ghent	55,000
Liège	50,000

*italic* = towns in the North.

With respect to economic development the differences between the North and the South were larger than those of population. In the North, the 17<sup>th</sup> century was its 'Golden Age.' The Republic became a world power. However, during the 18<sup>th</sup> century there was stagnation, both of the economy and of the population, but despite this, the Republic still remained one of the most prosperous countries of Europe. In the 19<sup>th</sup> Century, industrialisation took place, but at a very slow pace. Until 1900 the Netherlands mainly remained a country of merchants, strongly oriented to both Germany and its colonies (Dutch Indies).

By contrast, between 1600 and 1800 the South strongly lagged behind with respect to economic growth. During the 19<sup>th</sup> century though, Belgium was one of the leading industrial countries of Europe. Now economic growth of the South was stronger than that of the North. The Netherlands started to lag behind, in industrial development.

Also in political life, during the entire period there were strong contrasts between the North and the South. In the North, between 1600 and 1795 there was a high degree of local autonomy. Then, between 1795 and 1900 a gradual process of centralisation and unification took place.

In the South, by contrast, there was already a growing process of centralisation between 1600 and 1795, especially under the Austrian rule. The period 1830-1900 was characterised by a strong influence of liberalism (and by conflicts between the liberals and the catholics), that seems to have been even stronger than in the quite liberal North.

In the cultural and religious domains the North was dominated by the Calvinist church, though Catholicism was tolerated. There was a great freedom of the press, and, as a result, a strong publishing industry, which produced books for the entire



European market. During the 19<sup>th</sup> century a strong growth of Catholicism took place in the North.

The South, by contrast, was dominated by the Catholic Church. During the entire period there were tensions between state power and church power. There also was censorship, first executed by the Jesuits, later by court advisors, such as Van Swieten (until 1795) (see below). During the 19<sup>th</sup> century the influence of liberalism grew strongly. In contrast to the period before 1795, there was then freedom of education.

### **Different degrees of institutionalisation of chemistry in North and South, 1600-1740**

The major driving forces for the institutionalisation of chemistry in the 16<sup>th</sup> and 17<sup>th</sup> centuries Europe were (a) Court alchemy; (b) Mining and metallurgy; and (c) Medicine and pharmacy.

Applied to the Low Countries, the situations were as follows:

- Court alchemy played only a limited role at the courts in Brussels and Delft. It was quite important though at the court at Liège under Ernest of Bavaria, Prince-Bishop from 1580 to 1612, who actively supported the Paracelsian movement.
- Mining and metallurgy were absent in the North, and were not very important in the South before the end of the 18<sup>th</sup> century; with the exception of the investigation of mineral waters. Van Helmont, for instance, published on the analysis of mineral waters in 1624. After about 1760 mining and metallurgy started to play a growing role in the South.
- Against this background, between 1600 and 1740 medicine and pharmacy, the preparation of so-called ‘chymical remedies’ especially, certainly were *the* major driving force of the institutionalisation of chemistry in the Low Countries. Important roles were played by Jean Baptiste Van Helmont in the South, and by Franciscus de Boë Sylvius in the North.

Institutionalisation of chemistry took place within the medical faculties of the Universities and other Institutes of Higher Learning, and in the context of the training of pharmacists.

In the field of higher learning the differences between the North and the South were huge. In the South, there was only one University, at Louvain/ Leuven, founded in 1425. Since 1562 there also was a university at Douai, until these territories were taken by the French in 1667-1668; but there seems to have been no

teaching of chemistry at that university. In the North, by contrast, six Universities were founded between 1575 and 1656 (Table 4). Moreover, there were also nine so-called 'Illustrious Schools' (or 'Illustrious Athenea', or 'Illustrious Gymnasia') in the North, established between 1599 and 1683, some of which later obtained university status (Harderwijk in 1648; Nijmegen in 1656; and Amsterdam in 1876). These 'Illustrious Schools' were a kind of arts faculties, which prepared the students for more advanced studies in law, theology and medicine at the universities. In that respect their teaching overlapped with the first 1-2 years of a university study. On the other hand, several of these schools, especially those in the area ruled by the States General (i.e. the schools at Bosch, Breda and Maastricht), were mainly directed to the training of clergymen for the Calvinist church. Although there was no exact equivalent of those schools in the South, there may have been certain similarities with the Jesuit Seminaries (and/or Colleges) of the South, as far as the teaching of theology was concerned. From the point of the institutionalisation of chemistry though, it is important to note that most of the 'Illustrious Schools' had chairs of medicine at some time in their existence, whereas the Jesuit Seminaries had not.

**Table 4**  
*The establishment of institutions of higher learning in the South and the North, 1425-1740*

<b>Universities (South)</b> 1425 Louvain 1562 Douai	<b>Universities (North)</b> 1575 Leiden 1585 Franeker 1614 Groningen 1636 Utrecht 1648 Harderwijk 1656 Nijmegen
<b>Illustrious Schools (South)</b> Jesuit Seminaries (there were at least 23 Jesuit Colleges founded between 1542 and 1649, but it remains unclear how many had departments of higher learning annexed to them; and it is even more unclear whether there was any teaching of medicine and/or chemistry)	<b>Illustrious Schools (North)</b> 1599 Harderwijk 1630 Deventer 1632 Amsterdam 1636 Bosch (Bois le Duc) 1636 Rotterdam 1646 Breda 1650 Middelburg 1655 Nijmegen 1683 Maastricht

If one looks at the institutionalisation of chemistry at the German universities, which has been well studied, four phases can be distinguished:

- First, between about 1560 and 1620 chymistry and chemiatry were introduced as teaching subjects at Medical Faculties of universities of the German Protestant States. At some universities, practical laboratory training was also offered, so that medical doctors could learn to make their own chymical medicines. Chemistry textbooks, in Latin, were written for these students of medicine. Chairs, laboratories and textbooks were the hall-marks of the institutionalisation of university chemistry.
- Then, between about 1600 and 1680, the new occupation of the ‘chymist’ emerged. These chymists started to supply medical doctors with chymical medicines.
- A third period, between about 1650 and 1720, was marked by the integration of the chymical medicines into the pharmacists’ repertoire (in addition to Galenics). In Prussia, for instance, a special decree of 1685 declared that Prussian pharmacists were not allowed to buy their medicines from chymists. They should make these medicines themselves. As a result, a market for chemistry textbooks in the vernacular emerged, directed to pharmacist’s apprentices.
- As another result, the practice of chemistry shifted from the medical men to the pharmacists. During the fourth period, between about 1720 and 1770, the teaching of practical chemistry at the Medical faculties declined.

The institutionalisation of chemistry in the Low Countries did not closely follow this German pattern. There was a more or less ‘reverse order’ of institutionalisation, in the sense that chemistry was first embraced by the pharmacists, before it got a firm foothold at the universities.

In the South, the major chemist during the first part of the 17<sup>th</sup> century was Jan Baptist van Helmont (c1579-1644), who was a critical follower of Paracelsus. He was a contemporary of the first Germany Professors of chymiatry and chymistry, and like them engaged in a revolution in medicine, by introducing an experimental approach. But unlike his colleagues in the German Protestant States he did not get a position at a university. Between 1624 and 1644 he had great problems with the Inquisition and with the Theology Faculty of the University at Louvain. As a result of this religious and political opposition, chemistry only hesitatingly entered pharmacy in South, and before it was accepted by the Medical Faculty at Louvain.

The first book published in the South on chymistry *and* pharmacy was Jan Bisschop's *Pharmacia Galenica & Chymica* (Ghent 1653). Contrary to similar publications in the North, this book was not by a true adept of chymistry. Jan Bisschop was a Jesuit pharmacist, who had studied and worked in Vienna. He was rather critical towards chymical medicines. His book went through many editions.

Eight years later (1661) the *Pharmacia Galeno-Chymica Antverpiensis* was published, and in 1665 an agreement between the town of Louvain and the university paved the way for the local pharmacists to study at university. Not much later, a medical doctor, Adrien Regnault, started to give private courses in chemistry at Louvain. So, it can be concluded that in the 1660s and 1670s chemistry started to be recognised in the South by some, as a subject relevant to pharmacists. This is further confirmed by the fact that in 1676 an edition of Glaser's *Traité de Chimie* was published in Brussels. Probably this was the first chemistry textbook in the vernacular published in the South. In 1683 pharmacists received a monopoly on the preparation of medicines from King Charles II. Medical doctors were not allowed to make their own medicines. Chymical medicines now had an officially recognised position in the South, and medical doctors should have enough knowledge to prescribe them. Therefore in 1685 an official chemistry chair was created at Louvain University, for Regnault.

Also in the North, chymistry was first introduced into pharmacy, before it entered medicine. Like in the South (Glaser) there were influences from France (Beguin, see below); but in the North there were also strong German influences.

In 1614, so about 60 years earlier than in the South, the first chemistry textbook in the vernacular was published in the Republic. It was a translation of Jean Beguin's, *Tyrocinium chymicum* with the Dutch subtitle, *Dat is de eerste proeve der chymie, vervatende meest alle manieren van preparatien der chymische medicamenten, nut ende bequaem voor medecijs, apteckers, ende chyrurgijns* (The first steps in chemistry, containing all methods to prepare chymical medicines, useful and adapted for medical men, pharmacists and surgeons) (Utrecht 1614). A second edition appeared in 1623, and a third in 1669. In 1640 H. à Mijnsicht published his *Thesaurus et armentarium medico-chymicum* (Leiden), and about 1644 the town of Nijmegen issued a decree that each pharmacist had to make his own chymical medicines, and was not allowed to buy them from others.

In the following decades strong German influences became apparent. Probably as a result of the devastating effects of the 30 years war in Germany, several trained chemists and pharmacists left their country for Holland. One of them was the famous Rudolph Glauber, who between 1640 and 1670 lived for almost 25 years

in Amsterdam, where he produced chemicals in a commercial laboratory. He had a great influence in Holland, partly via his co-workers and pupils.

Another German was Albert Kyper, who came from Königsberg in Prussia in 1638. In 1643 he studied at Leiden University, where he noted the lack of a chemical laboratory. This is a clear sign that the institutionalisation of chemistry in Germany was more advanced at that date. Three years later, in 1646, Kyper was appointed a teacher of medicine, anatomy and chemistry at the Illustrious School at Breda. He was the first teacher of chemistry at a Dutch Institute of Higher Learning. He used instruments in his teaching, or even fitted up a small laboratory, but this teaching of chemistry was discontinued when he became Professor of Medicine in Leiden in 1650.

Regular complaints by pharmacists (Leiden, 1647; Rotterdam, 1673) that medical doctors were producing medicines make it clear that the preparation of chymical remedies was practiced in the Republic in those years. From the 1650s onwards also the number of chemistry teachers grew. In 1656 Jacob Uwens started his lessons in anatomy and chemistry at the University of Nijmegen, and 1658-1659 even three persons, Sylvius, Stam and Marggraff started to give private lessons in chemistry to medical students at Leiden. An official chair was created for Sylvius in 1666. In the 1690s the Universities of Utrecht and Groningen followed, but at Utrecht the private teaching of chemistry had already started much earlier (Table 5).

After the death of Van Helmont, there were no major chemists in the South that followed in his footsteps. Between 1644 and 1740 only a few chemical textbooks and treatises were published. By contrast, in the North there were several important teachers of chemistry and publicists of the subject.

Franciscus de Boë Sylvius (1614-1672) was certainly the most important academic chemist in the Republic during the second half of the 17<sup>th</sup> century. He was a follower of Van Helmont, and a friend of Glauber. After his appointment to a medical chair at Leiden University in 1658 he became a very influential teacher, who, as a iatrochemist, systematically tried to explain all physiological processes in terms of the actions of acids and bases. His doctrines were integrated into Cartesianism by several of his followers, such as Blankaart. In this form, Sylvius' Cartesian iatrochemical doctrines had an enormous influence on Dutch chemistry.

Steven Blankaart (1650-1704), although not a chemist known for original discoveries, should be mentioned among the important Dutch chemists of the second half of the 17<sup>th</sup> century. He was a great populariser of Sylvian and Cartesian

**Table 5**  
***The start of chemistry teaching at Institutions of Higher Learning in the South and the North, 1425-1795***

<b>Universities (south)</b> 1670s? Leuven: Regnault <i>1685 Leuven: Regnault</i>	<b>Universities (north)</b> <i>1656 Nijmegen: Uvens</i> 1658-59 Leiden: Sylvius (1638 from Germany and France), Stam, and Marggraff (from Germany) <i>1666 Leiden: Sylvius in Med. Fac.</i> 1668 Utrecht: De Maets <i>1669 Leiden: De Maets in Phil. Fac.</i> 1694 Utrecht: J.C. Barchusen (from Germany) <i>1696 Groningen: Eyssonius</i> <i>1720 Franeker: Muys</i> <i>1754 Harderwijk: Van Haastenburg</i>
<b>Illustrious Schools (south)</b> Jesuit Seminaries?	<b>Illustrious Schools (north)</b> 1646 Breda: Kyper (1638 from Germany) <i>1785 Amsterdam: Van Rhyn</i> 1789 Deventer: Westenberg

*italic* = institutions with an official chemistry chair.

medicine, and the most prolific writer on chemistry in the North. Between 1678 and 1693 he published six major treatises on chemistry and pharmacy in the vernacular and most of them went through several editions. Blankaart also translated foreign textbooks into Dutch, for example, Lancilotti's book from the Italian in 1680, and Lemery's *Traité de chimie* in 1683. Blankaart also coined the Dutch word for chemistry 'scheikunde.' The fact that this word is used until today shows the influence of Blankaart's writings. He published following titles on chemistry:

- 1678 *De nieuwe hedendaagsche stof-scheiding ofthe chymia*
- 1678 *Nieuw lichtende praktijk der medicynen ... nevens de hedendaagse chymia* (7 editions 1678-1735!)
- 1680 Carlo Lancilotti, *De brandende salamander, ofte Ontleedinge der chymicale stoffen: zijnde een weg-wijzer, oft institute om sich in alle operatien der schey-konst te oeffenen : Item den ontwaakten chymist*
- 1683 *'t Nieuw-licht des apotheker, of Nieuwe-gronden en fondamenten der artzeni- en chymise-bereiding*

- 1683 Nicolas Lemery, *Het filosoophische laboratorium, of Der chymisten stook-huis: Leerende op een korte en ligte wyse alle de gebruikelijkste medicamenten op de chymische wyse bereiden* (4 editions 1683-1725)
- 1693 K. Digby *Theatrum chemicum, ofte Geopende deure der chymische verborghtheden*.

In addition, Blankaart also published many works on medicine and botany.

At Utrecht University chemistry was taught by Johann Conrad Barchusen (1666-1723), who came from Germany as well. In 1694 he started giving private lessons on chemistry at Utrecht, and then he fitted up a laboratory. He was elevated to a formal university position in chemistry in 1698, the same year he published his textbook, *Pyrosophia*.

There can be no doubt that compared to the South chemistry in the North stood on a much broader basis in 1700 (cf. Table 5). This broad basis was a fertile ground in which to produce a star of first magnitude in chemistry, not only in the Republic, but in Europe as a whole, namely, Herman Boerhaave (1668-1738). As a student of medicine, Boerhaave followed the lectures on chemistry given at the university by De Maets and Le Mort, a pupil of Glauber. His most important teacher in practical chemistry was the pharmacist David Stam (1633-1711), who taught chemistry at Leiden since 1658. In 1702 Boerhaave started to give private lessons in chemistry. In 1709 he was appointed Professor of Medicine and Botany, and from 1718 to 1729 he also was Professor of Chemistry, as successor to Le Mort. His famous textbook *Elementa Chemiae* was published in 1732. Boerhaave's influence on the teaching of both chemistry and medicine has been enormous. Students from all over Europe came to Leiden to follow his lessons; in particular, students from Scotland, England and Germany.

### **Decline in the North and new initiatives in the South, 1740-1795**

After the death of Boerhaave, chemistry in the North went into decline. At the same time, the cameralist policies of the Austrian rulers Maria Theresia and Joseph II led to an important renaissance of science in the Habsburg empire, and an improvement of economic life.

The career of Gerard van Swieten (1700-1772) illustrates perfectly this shift of the centre of gravity in chemistry from the Republic to the Austrian lands. Van Swieten was from a Catholic noble family. He started as pharmacist's apprentice

in Amsterdam and Leiden, in the apothecary shop of Nicolaas Stam, the son of Boerhaave's teacher David Stam. Parallel to this he followed Boerhaave's lectures from 1717 till 1738. 1720 he started his own 'chemist's shop' at Leiden, and in 1725 he was awarded the medical doctorate. After Boerhaave's death, Van Swieten started writing and publishing comments on Boerhaave's work, from 1742 onwards. This made him famous throughout Europe. In 1744 he was appointed personal physician to Maria Theresia, and moved to Vienna. Five years later he was made responsible for the reform of the Medical Faculty at Vienna. As part of these reforms, Van Swieten established a Chair of Botany and Chemistry. Chemistry was a subject which had not been taught in Vienna before. Van Swieten was also instrumental in attracting other important Dutch (Catholic) scientists to the Habsburg capital, for example, Nicolas Jacquin in 1752, and Jan Ingen-Housz in 1768.

After the reforms of medical teaching at Vienna, also the other medical faculties in the Habsburg Empire were restructured, first Prague, then Budapest, and also Louvain. In 1754 the Count de Neny independently started a whole series of reforms of Louvain University. One of these was the founding of Cabinet of Experimental Physics in 1755. Two years later, with the support of the Viceroy Charles de Lorraine, a Chemical University Laboratory was erected to replace the private laboratories used by the chemistry teachers until then. De Neny also ordered the Professors of Chemistry should use Boerhaave's textbook, and produce annual reports on their lectures and laboratory work.

**Table 6**  
*The founding of chemical laboratories at the Universities, 1660-1760*

South	North
1757 Louvain	1669 Leiden
	1695 Utrecht
	1707 Groningen
	1752 Franeker

With four university chemical laboratories in 1757 the North was still leading in that respect, but the establishment of the chemical laboratory at Louvain was an important new start (Table 6). During the second half of the 18<sup>th</sup> century the level of chemical teaching and research in the South was definitely on a higher level than during the first half of the century.

Also in some parts of economic life there was a shift from the North to the South. After the great prosperity of the 17<sup>th</sup> century, the economy of the Dutch Republic



stagnated during the 18<sup>th</sup> century, and some sectors even went into decline. One of these sectors was calico printing, which was the most important consumer of products of the contemporary chemical industries. Between 1678 and about 1725 Amsterdam had been, by far, the foremost European centre of calico printing. Dozens of calico printing shops, each with 25 to 40 workers, were active in the Amsterdam area. Between 1700 and 1750 though, competing calico printing centres emerged in London, Hamburg, Augsburg, and Geneva. After 1750, Dutch calico printing increasingly went into decline. Especially between 1783 and 1788 many works had to close because of strong foreign competition. In 1815 only one factory was left, of the dozens that had existed before. Only after the separation of Belgium and Holland in 1830 did Dutch calico printing industry have a renaissance. After 1830 several Belgium calico printers moved to Leiden and Harlem, in order to profit from the large Dutch colonial market.

These changes in the prosperity of the Dutch textile printing industry did not leave the chemical industries untouched. About 1750 there were more than 10 *aqua fortis* (nitric acid) works around Amsterdam, and several other chemical works. In those years the Dutch chemical industry was still one of largest in Europe, at least if the size of the population is taken into account. Later, sulphuric acid partly took over the role played before by nitric acid. Between 1764 and 1774 the lead chamber process was introduced into the Republic by J. Farquaharson, a partner of Roebuck, the inventor of the process. In 1790 a second sulphuric acid plant was built by Anthony Le Blanc. As a result of the decline in calico printing, between 1795 and 1815 all nitric and sulphuric acid works closed down. Only after 1830 was sulphuric acid manufacture reintroduced in the North, hand in hand with the resurrection of the Dutch calico printing industry.

In the South, calico printing started relatively late. The first factory was founded in Antwerp by Quirinus Vlemincks in 1751, with technical know-how from the North. Two years later the firm Jan Beerenbroek & Co. built a large calico print works at Dambrugge, close to Antwerp, with the help of technical experts from Germany and Holland. A monopoly was granted for 25 years, and as result the factory expanded tremendously. In 1767, so 'officially' before the 'Industrial Revolution,' the incredible number of 576 workers (including women and children) were employed in the Dambrugge factory. Despite the monopoly he had granted, Viceroy Charles de Lorraine founded his own court manufacture at Tervuren in 1758, with the help of the chemist Pierre de Schavye. In 1778 the 25-year period of the monopoly elapsed, and in the following years several new calico print-works were founded at Ghent and Brussels. One of these was a large company founded

at Ghent by Abraham Voortman, who was a Catholic calico printer from the North.

The establishment of calico print works in the South created a market for the chemical industry. After the Tervuren factory had been founded, Thomas Murry from England set up, in 1759, three sulphuric acid and aqua fortis works near Brussels, with the Royal protection by Charles de Lorraine. In 1762, Murry's sulphuric acid works were taken over by the State. In the following decades also mining and metallurgy developed in the South. As a consequence, between 1759 and 1790 more than 10 sulphuric and nitric acid works were founded near Brussels and Liège. During the French wars and the Continental blockade several of these works closed down. By 1815 only the factory of Vander Elst had survived. But during the 'United Kingdom' southern calico printing flourished again, and between 1815 and 1830 five new sulphuric acid works were erected near Ghent and other industrial centres.

Reflecting on these contrasting developments in the North and the South, it is concluded that they were partly the result of contrasting economic policies. In the South there was a policy of strong dirigism and protectionism by the enlightened Austrian state. Examples are the 1753 monopoly granted to the Dambrugge factory, the 1754-1757 university reforms at Louvain, the 1758 calico print works erected by Charles de Lorraine himself, and the role of the state in sulphuric acid manufacture 1759/ 1762.

In the North nothing comparable existed. Politics in the Republic of the Seven United Provinces was highly decentralised. A strong centralised economic policy was absent. Local elites determined the rules and regulations of the towns. The positive side of the coin, however, was that private initiative could flourish and that civic society was strongly developed. It can be no surprise therefore that it was not the state, but private persons who first expressed their worries on economic decline, especially in calico printing. Between 1779 and 1785 several pamphlets were written, as well as initiatives taken by local scientific and patriotic societies that called for action. The pharmacists and chemists Tieboel, Schonck, and Kasteleyn, for instance, argued that town governments should stimulate the teaching of chemistry in order to save the chemical and the calico printing industries.

That local scientific societies played a role in these debates was typical of civic society in the North. Public and private societies and clubs flourished. The great political and religious freedom that characterised the Dutch Republic gave ample room for local initiatives. After the *Hollandsche Maatschappij van Wetenschappen* had been founded in Harlem in 1748, three other important provincial

scientific societies followed. During the last decades of the eighteenth century, between 25 and 40 local natural science societies were founded in the Dutch towns. There were even a few 'chemical societies' among them: such as the Chemisch Gezelschap at Rotterdam (1767), the Scheikundig Gezelschap at Amsterdam (1790), the Gezelschap van Beminnaaren der Scheikunde at Delft (1792), and the Natuur- en Scheikundig Genootschap at Groningen (1801).

In the more centralised and less liberal South there seem to have existed hardly any (local) scientific societies. Fully in line with the centralised policies, in 1772 the Austrian rulers created the Académie impériale et royale des Sciences et Belles-Lettres, as a follow-up of a society founded three years earlier. In 1779 the Prince-Bishop of Liège supported the establishment of the Société libre d'émulation et d'encouragement pour les sciences et les arts. These two societies were the two most important scientific societies under the *ancien régime*. Next to them, there were private scientific societies in Ghent (1776) and in Ostende, as well as the Société de physique expérimentale de Bruxelles, founded in 1788, or earlier. It was only after the inclusion of the southern provinces into the French empire, in 1795, that the founding of new scientific societies really took-off. An early example is the Société de médecine, chirurgie et pharmacie (1795) at Brussels, founded, among others, by the pharmacist Augustin Van den Sande, and the chemist Jean-Baptiste Van Mons.

It should be noted that in the calls for reform by the patriotic and scientific societies of the North the industrial, and agricultural relevance of chemistry was emphasised. From pamphlets and papers written by the chemists and pharmacists Tieboel, Kasteleyn and Driessen in 1785-1787 this becomes very clear. Obviously chemistry had acquired a new meaning. Whereas between 1600 and 1750 chemistry was identified almost exclusively with the preparation of 'chemical medicines', after 1750 this situation changed. The earliest examples of the recognition of the industrial and agricultural relevance of chemistry can be found in the works of the famous German chemist Georg Ernst Stahl (1660-1734) during the early 18<sup>th</sup> century. Other examples are William Cullen's, *The Plan of a Course of Chemical Lectures and Experiments directed chiefly to the improvement of the Arts and Manufactures* (1748) and the writings of Pierre Joseph Macquer (1718-1784) from the 1750s. The earliest example in the Netherlands dates from 1769 when a student society was founded at Groningen University (the future chemists Van Marum and Driessen were among its members) with the explicit aim to apply natural science to agriculture. By 1785 a true sense of crisis was felt. Several scientists argued that more chemistry chairs should be established, in order to educate and train both pharmacists and manufacturers.

### Under French rule, 1795-1814

On the eve of French revolution social unrest came to a climax and even got a revolutionary character, both in the North, and the South. In the North many enlightened citizens were frustrated that the state was moving too slowly. In 1787 there was a patriotic revolt against the Prince of Orange. After the Prince of Orange had resumed power, with the help of German troops, several scientists and intellectuals fled to Belgium and France. One of them was Gadso Coopmans (1746-1810), Professor of Chemistry and Medicine at Franeker. During his stay in Brussels, from 1787 and 1790, he was asked to teach chemistry at the University, that had temporarily been transferred from Louvain to Brussels. When the Brabant revolt failed in 1790 (see below) and the Austrians returned, Coopmans fled to France.

In the South, by contrast, many citizens were frustrated that state was moving too quickly. A clear example was the transfer of the University of Louvain to the capital, Brussels, by the Austrians. Between 1787 and 1789 this led to heated conflicts between the Catholics and the secular Austrian state. This unrest was followed by the Brabant revolution of 1790, which temporarily led the Austrian troops leave the country. Also in the South several chemists were involved. Van Bochaute, for instance, lost his position as Professor of Chemistry at Louvain because he had followed the university to Brussels, and the chemist Van Mons was put into prison.

In 1795 the South and the southern parts of the Dutch republic were occupied by French troops, and integrated into France. In the North the Prince of Orange was forced to leave the country and the so-called Batavian Republic was established, with a constitution, and strongly under French control. In 1810 also the Northern Provinces were included into the French empire.

During these years, the teaching of chemistry expanded strongly. In the North new developments resulted from private initiatives, 'from below'. Already in 1785 local initiatives made the town of Alkmaar established a so-called 'town lecturer' in chemistry, to give chemistry courses to pharmacist's apprentices and manufacturers. In the same year, five Amsterdam merchants donated a large sum of money to the 'Atheneum Illustre' in Amsterdam, to erect a chemical laboratory and to pay the salary of a Professor of Chemistry.

After the patriotic revolt of 1787 had failed, the reform movement lost momentum. But after the creation of the Batavian Republic in 1795 new initiatives again flourished. In 1795 and 1796 no less than six town lecturers in chemistry were appointed in the largest Dutch towns (Table 7). Initiatives to establish large chemical-technological laboratories for industry in Amsterdam and Groningen were only partially successful, because of the lack of finances in those times of war and political instability.

**Table 7**  
***The growth of chemical education in the Low Countries, 1785-1800***

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<b>Town lecturers (North)</b>	<b>Ecoles Centrales (South)</b>
1785? Alkmaar	1797 Brussels: Van Mons
1785 Amsterdam: Van Rhyn (paid by 5 merchants)	1797 Antwerp: Van Aenvanck
1795 Haarlem: Van Marum	1797 Maastricht: Minckelers
1795 Leiden: Brugmans	1797 Luxemburg: Van den Sande
1796 Utrecht: De Fremery	1797 Gent: Coppens
1796 Rotterdam: Rouppe	1797 Mons: Lémerel; Ricourt
1796 Dordrecht: Van der Leeuw	1797 Liège: Robert; Villette; Vanderheyden
1796 Den Haag: Van Maenen	1797 Namur: Christian
1796 Amsterdam: plans for a new (applied) laboratory	1797 Bruges: Beyts; Devaux
1801 Groningen: plans for Chemical- Technical Institute	

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N.B. Maastricht, later part of the North, then was part of the South.

In the South, the teaching of chemistry also proliferated. But here, as to be expected, the initiative came ‘from above.’ As a result of French legislation in 1797 the so-called *Écoles Centrales* were founded in the capitals of the southern departments. The schools had a hybrid character. On the one hand, they were a kind of secondary school, preparing for university study. On the other hand, evening lessons were given to adults. At each school there was a teacher of chemistry, who taught chemistry to the pupils of the school as well as to pharmacist’s apprentices, artisans and manufacturers. The *Écoles Centrales* were an important initiative in the field of natural science teaching, but they were short lived. After their closure in 1802-1803 their role was partially taking over by Lycea, Athenea and *Écoles de Médecine*. Although less intense than between 1797 and 1802, the teaching of chemistry continued in one form or another in most departmental capitals of the South.

Also university life was strongly affected by French rule. In 1797 the University of Louvain was forced to close down, and between 1797 and 1808 there was no University in the South. Students were supposed to study in Paris, or at one of the other French Universities. In 1808 though, an imperial university was founded at Liège, who was short lived. After the integration of the North into the

French empire, the Universities of Harderwijk and Franeker were closed by the French, and the same happened to several of the “Illustrious Schools”.

The last and important consequence of French rule was the suppression of the ‘chemist’s’ profession (i.e. manufacturers of chemical medicines, without a pharmacist’s degree), as a result of the French medical laws of 1801, and the founding of the *École de Pharmacie* at Paris. Probably this was effective immediately in the South in 1801. In the North at least, after the inclusion into the French empire in 1810 the chemist’s profession was suppressed immediately. From then on only pharmacists were allowed to produce medicines.

### **Chemistry during the ‘United Kingdom’ of the Netherlands, 1815-1830**

After the defeat of Napoleon, the Vienna Congress decided that the North and the South should be united into a ‘United Kingdom of the Netherlands.’ Several of the previous French laws remained in force, albeit sometimes in a somewhat modified form. The new King of the ‘United Kingdom,’ William I, tried to create a quite symmetrical situation between the North and the South in university education. In the North, instead of the five previous universities (Nijmegen had closed down long before), only three State Universities remained: Leiden, Utrecht and Groningen. And in South, instead of the single University at Louvain, also three Universities opened their gates: Louvain, Ghent and Liège. There were also three so-called *Athenea* in the North (that replaced the *Illustrious Schools*), in Amsterdam, Deventer, and Franeker, and three in the South, in Brussels, Luxemburg, and Namur. There was a seventh *Atheneum* at Maastricht, a town whose northern or southern status was unclear. In the royal decree of 1815 in which most of these decisions were formulated, it was also ordered that the Chemistry Professor should be part of the newly created Science Faculties, and not, as before, in the Faculties of Medicine.

From the point of view of chemistry teaching, a second important step was taken in 1818, when new rules were formulated for the examination of pharmacists. University education was not mandatory, but the level of chemical knowledge required for examination, made it necessary for pharmacy students to follow lectures given by professional chemists. In the South, the existing lecture courses connected to the hospitals at Antwerp, Brussels, Liège and Ghent were often given by the same persons who also had lectured at the *Écoles Centrales* to fulfill that role. In the North nothing similar existed, and therefore special Medical Schools for the training of pharmacists, surgeons and midwives were created from

1825 onwards in the towns of Haarlem, Hoorn, Maastricht, Middelburg, Alkmaar, Amsterdam, and Rotterdam. All these schools appointed Professors of Chemistry.

A third important event with respect to the teaching of chemistry was a decree by king William I in 1825, that obliged all Universities of the kingdom to give evening lessons on mechanics and chemistry for artisans and manufacturers, in order to raise the scientific level of the national industries. As a result, several chairs in applied chemistry were created, and industrial schools were established at the Universities of Leiden, Ghent and Liège, which in the last two cases, after Belgium independence, developed into engineering schools.

### **Diverging paths, again, 1830-1900**

After the Belgium uprising of 1830, the North and the South were separated again, in practice immediately in 1830, formally only after the peace treaty of 1839, in which it was decided for instance that Maastricht should be a part of the Netherlands, not part of Belgium.

The political cultures of both countries were quite different. In the Netherlands, King William I and his successors followed quite autocratic and conservative policies, in agreement with large parts of the dominant Calvinist church, as well as with the generally quite conservative mentality of the ruling financial and merchant elites. In the highly industrialised Belgium, by contrast, a far more liberal attitude prevailed, that formed a marked contrast to the autocratic situation in the 18<sup>th</sup> century. Nevertheless, conflicts between the liberals and the Catholics continued to play a role during the rest of the 19<sup>th</sup> century.

These differences in political culture led to some marked contrasts between both countries in the field of higher education:

- in the Netherlands, previous study at a gymnasium (Latin school) was required for a university study, but in Belgium access to university was more open;
- in the Netherlands, like in northern Germany, university study was concluded by writing a dissertation (until 1850 mostly in Latin, thereafter in the vernacular), whereas in Belgium state examinations were held;
- in the Netherlands until 1876 there were only State Universities, but in Belgium next to two State Universities (Liège; Ghent), there were also two ‘free’ universities since 1834: a Catholic University at Louvain, and a liberal University at Brussels;
- in the Netherlands Universities, that required the study of Latin and Greek, were strictly separated from technical and industrial education. In 1842 an



engineering school was founded at Delft, which only acquired university status in 1905. In Belgium, by contrast, special schools for engineers and agronomists were integrated into the Universities.

These contrasts show that the Netherlands were a socially more segregated society, with a strict division between the educated and industrial classes, whereas in the more liberal Belgium society this division was less strict.

At the end of the 19<sup>th</sup> century there were important educational reforms in both Belgium and the Netherlands, that led to a situation of a greater educational convergence.

In the Netherlands a law on secondary education in 1863 led to the establishment of about 40 modern secondary schools, with laboratories and chemistry teaching. This law had a great impact on the scientific 'start-level' of university students. In 1877 a law on higher education led to further improvement. Pharmacy now became a university study, and separate special doctorates in chemistry (Dr. Chem.) and pharmacy (Dr. Pharm.) were created, in contrast to the previous general Dr. Phil. degree. During the last decades of the 19<sup>th</sup> century scientific teaching and research at the Dutch Universities flourished, as is illustrated by the Nobel prizes given in the early 20<sup>th</sup> century to several Dutch chemists and physicists, such as Van 't Hoff, Lorentz, Van der Waals, and Kamerling Onnes.

In Belgium university studies were reformed by the laws of 1877 and 1890. Now, also in Belgium, secondary school certificates and/or entrance examinations were required before someone could enter a university. Writing of a dissertation, based on original research, replaced the previous state examinations. And at the end of the 19<sup>th</sup> century there was a strong growth in the number of chairs of chemistry, far greater than that happened in the Netherlands. In the Netherlands, similar to Germany, there were often only 2 or 3 chemistry Professors at a University, who each covered large parts of chemical science. In Belgium, similar to France, there were often a greater number of more specialised Chemistry Professors at each University, with more limited mandates.

## Some Conclusions

From this study a few tentative conclusions have been formulated.

In the first place, it is hoped that the contrasting stories of chemistry in the North and the South have shown that politics, religion and economics do matter in its development in a national arena.



In the second place, it is considered that some national oriented histories of Belgium science, which tend to date the revival of Belgium science in 1830, as a result of the independence of the nation, are incorrect. It has been shown that the revival of Belgium science started between 1750 and 1790, under Austrian rule.

In the third place, history matters: the ranking of nations in the field of science is seldom stable over longer periods of time. In the case of chemistry in the Low Countries, between 1650 and about 1800 Dutch chemistry was leading, between 1750 and 1830 Belgium chemistry gradually took over, at least in quantitative terms. The 1863 law on secondary education in the North gave Dutch chemistry again a strong impetus (Van't Hoff), and northern chemistry again regained much of its previous leading position.

In the fourth and last place, it is suggested that strong States are good for science. For example, the state support to the industry and the Universities given in the South in the 1750s, and also the remarkable effects of the laws on secondary and higher education in the Netherlands of 1863 and 1877, which ended a long period of inactivity by the Dutch state.

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## **Bibliography**

Annette André-Félix, *Les débuts de l'industrie chimique dans les Pays-Bas autrichien* (Bruxelles: Institut de Sociologie, Université Libre de Bruxelles, 1971).

D.P. Blok, et al. (eds.), *Algemene Geschiedenis der Nederlanden*, 15 Vols. (Haarlem: Fibula-Van Dishoeck 1978-1983).

Michel Bougard, "Jean-Baptiste Van Mons, een boegbeeld van de scheikunde," in *Geschiedenis van de wetenschappen in België 1815-2000*, eds. Robert Halleux, Jan Vandersmissen, Andrée Despy-Meyer, and Geert Vanpaemel (Brussel: Dexia Bank 2001), Vol. 1, 146-153.

Albert Bruylants, "Louis Henry (1834-1913) en de scheikunde te Leuven," in *Academiae Analecta* (AWLSK, Paleis der Academiën: Brussel, 1986) (= *Mededelingen van de Koninklijke Academie voor Wetenschappen, Letteren en Schone Kunsten van België* 48 (4) (1986)), 101-112.

- D. Thornburn Burns and Hendrik Deelstra, "Analytical Chemistry in Belgium: An Historical Overview," *Microchimica Acta* 161 (2008): 41-66.
- D. Thornburn Burns and Hendrik Deelstra, "The Rise of Analytical Chemistry in Belgium," *Analytical and Bioanalytical Chemistry* 391 (2008): 1113-1117.
- H. Deelstra, "De wet van 13 mei 1825, of de invoering van technisch onderwijs aan de hogescholen in de Zuiderlijke Nederlanden," in *Acta Octavi Conventus Historiae Scientiae Medicinae Mathesos Naturaliumque excolendae Bergae ad Zonam* [1977] *Pars altera* (Amsterdam: Ronald Meesters, 1978), 27-32.
- H. Deelstra, "Het scheikundig onderwijs en onderzoek te Gent vóór de komst van A. Kekulé in 1858," *Tijdschrift voor de Geschiedenis der Geneeskunde, Natuurwetenschappen, Wiskunde en Techniek* 2 (3) (1979): 110-117.
- H. Deelstra, "Het scheikunde-onderwijs aan de Rijksuniversiteit te Gent tijdens de 19de eeuw," in *Academiae Analecta* (AWLSK, Paleis der Academiën: Brussel, 1986) (= *Mededelingen van de Koninklijke Academie voor Wetenschappen, Letteren en Schone Kunsten van België* 48 (4) (1986)), 113-128.
- H.A. Deelstra, "Achtergronden van de wetten van 1890-1891 en de invloed op het natuurwetenschappelijk onderwijs in België," *Gewina* 16 (1993): 187-191.
- H. Deelstra, "La professionnalisation de la chimie en Belgique," *Sartoniana*, 8 (1995): 85-101.
- Hendrik Deelstra, "Bijdragen van de Belgische apothekers tot het wetenschappelijk onderzoek rond de eeuwwende," *Scientiarum Historia* 26 (1-2) (2000): 97-108.
- Hendrik Deelstra, "De scheikunde aan de universiteiten en hogescholen," in *Geschiedenis van de wetenschappen in België 1815-2000*, eds. Robert Halleux, Jan Vandersmissen, Andrée Despy-Meyer, and Geert Vanpaemel (Brussel: Dexia Bank 2001), Vol. 1, 159-178.
- Annette Félix, "L'enseignement de la chimie à la Faculté de Médecine de l'ancienne Université de Louvain," in *Academiae Analecta* (AWLSK, Paleis der Academiën: Brussel, 1986) (= *Mededelingen van de Koninklijke Academie voor Wetenschappen, Letteren en Schone Kunsten van België* 48 (4) (1986)), 77-86.
- Robert Halleux, "Chimistes provinciaux et révolution industrielle: le cas de la Belgique," *Archives internationales d'histoire des sciences* 46 (1996) : 6-22.
- Robert Halleux and Anne-Catherine Bernès, "Le cour savante d'Ernest de Bavière," *Archives internationales d'histoire des sciences* 45 (1995) : 3-29.
- Ernst Homburg, *Van beroep 'Chemiker': De opkomst van de industriële chemicus en het polytechnische onderwijs in Duitsland (1790-1850)* (Delft: Delftse Universitaire Pers, 1993).
- E. Homburg (et al.), *Chemische techniek in Nederland in de negentiende eeuw* (Zutphen: Walburg Pers, 1994).
- Ernst Homburg, "From Colour Maker to Chemist: Episodes from the Rise of the Colourist, 1670-1800", in *Natural Dyestuffs and Industrial Culture in Europe, 1750-1880*, eds. R. Fox and A. Nieto-Galan (Nantucket, MA: Watson Publ. Int., 1999), 219-257.
- Ernst Homburg, "The Netherlands: Keeping the ranks closed: The Dutch Chemical Society, 1903-1914," in *Creating Networks in Chemistry: The Founding and Early History of Chemical Societies in Europe*, eds. Anita Kildebæk Nielsen and Soňa Štrbáňová (Cambridge: Royal Society of Chemistry, 2008), 186-222.

E.H. Kossmann, *The Low Countries, 1780-1940* (Oxford: Clarendon Press 1978).

G.A. Lindeboom, "David en Nicolaas Stam apothekers te Leiden," *Bulletin. Kring voor de Geschiedenis van de Pharmacie in Benelux* Nr. 48 (okt. 1973): 18-25.

Joel Mokyr, *Industrialisation in the Low Countries, 1795-1850* (New Haven and London: Yale University Press 1976).

Carmelia Opsomer and Robert Halleux, "De natuurwetenschappen, de scheikunde en de geneeskunde", in *Geschiedenis van de wetenschappen in België van de Oudheid tot 1815*, eds. Robert Halleux, Carmélia Opzoomer, and Jan Vandersmissen (Brussel: Gemeentekrediet 1998), 229-258.

F.L.R. Sassen, "Levensberichten van de hoogleraren der Kwartierlijke Hogeschool te Nijmegen," *Numaga* 9 (3) (1962): 104-127.

Ferd. Sassen, *Het wijsgerig onderwijs aan de Illustre School te Breda (1646-1669)* (Amsterdam: Noord-Hollandsche Uitgevers Maatschappij, 1962) (= *Mededelingen van de Koninklijke Nederlandse Akademie van Wetenschappen, Afd. Letterkunde*, Nieuwe Reeks, deel 25, No. 7).

H.A.M. Snelders, "Chemistry at the Dutch universities: 1669-1900," in *Academiae Analecta* (AWLSK, Paleis der Academiën: Brussel, 1986) (= *Mededelingen van de Koninklijke Academie voor Wetenschappen, Letteren en Schone Kunsten van België* 48 (4) (1986)), 59-75.

H.A.M. Snelders, *Geschiedenis van de scheikunde in Nederland: Van alchemie tot chemie en chemische industrie rond 1900*, Vol. 1 (Delft: Delftse Universitaire Pers, 1993).

Henricus A.M. Snelders, "Jean-Servais Stas, een boegbeeld van de scheikunde," in *Geschiedenis van de wetenschappen in België 1815-2000*, eds. Robert Halleux, Jan Vandersmissen, Andrée Despy-Meyer, and Geert Vanpaemel (Brussel: Dexia Bank 2001), Vol. 1, 154-158.

Philippe Tomsin, "De industriële scheikunde," in *Geschiedenis van de wetenschappen in België 1815-2000*, eds. Robert Halleux, Jan Vandersmissen, Andrée Despy-Meyer, and Geert Vanpaemel (Brussel: Dexia Bank 2001), Vol. 2, 195-206.

[C. Vandenghoer, et al.], *300 jaar chemie te Leuven, 1685-1985. Tentoonstelling in de Universiteitshal, 14 november – 7 december 1985* (Leuven: J. Roegiers, 1985).

L.J. van der Wiele, "Het 'Licht der Apothekers'," *Bulletin. Kring voor de Geschiedenis van de Pharmacie in Benelux* Nr. 27 (okt. 1961): 1-9.

L.J. Vandewiele, "De farmacie onder Koning Willem I," in *Acta Octavi Conventus Historiae Scientiae Medicinae Mathesos Naturaliumque excolendae Bergae ad Zonam* [1977] *Pars altera* (Amsterdam: Ronald Meesters, 1978), 33-43.

L.J. Vandewiele, *Geschiedenis van de farmacie in België* (Beveren: Orion, 1981).

L.J. Vandewiele, "Broeder Petrus Gillis S.J. (1620-1697) auteur van *Medicina Pharmaceutica* of *Drogbereidende Geneeskunst*," *Bulletin. Kring voor de Geschiedenis van de Pharmacie in Benelux* Nr. 69 (mrt. 1986): 8-16.

Geert Vanpaemel, "Jan Frans Thijsbaert (1736-1825) en de School voor Experimentele Fysika te Leuven," *Tijdschrift voor de Geschiedenis der Geneeskunde, Natuurwetenschappen, Wiskunde en Techniek* 7 (4) (1984): 172-182.

Geert Vanpaemel, "J. B. van Mons (1765-1842) en het scheikunde-onderwijs aan de Rijksuniversiteit Leuven," in *Academiae Analecta* (AWLSK, Paleis der Academiën: Brussel,

1986) (= *Mededelingen van de Koninklijke Academie voor Wetenschappen, Letteren en Schone Kunsten van België* 48 (4) (1986)), 87-100.

Geert Vanpaemel, "Onderwijs voor 'de meer beschaafde klasse'. Het Museum voor Wetenschappen en Letteren te Brussel (1826-1834)," *Scientiarum Historia* 23 (1) (1997): 3-19.

Geert Vanpaemel and Brigitte Van Tiggelen, "The profession of chemist in nineteenth-century Belgium," in *The Making of the Chemist*, eds. David Knight and Helge Kragh (Cambridge: Cambridge UP 1998), 191-206.

Brigitte Van Tiggelen, "De la chaire aux laboratoires: Louis Henry et la professionalisation de la recherché en sciences naturelles en Belgique," *Gewina* 16 (1993): 192-203.

Brigitte Van Tiggelen, "Les réformes dans l'enseignement des sciences à la faculté des arts de Louvain au XVIIIème siècle," *Scientiarum Historia* 23 (2) (1997): 41-71.

Brigitte Van Tiggelen, "De natuurwetenschappen en de scheikunde", in *Geschiedenis van de wetenschappen in België van de Oudheid tot 1815*, eds. Robert Halleux, Carmélia Opzoomer, and Jan Vandersmissen (Brussel: Gemeentekrediet 1998), 339-364.

Brigitte Van Tiggelen, "De scheikundige theorieën," in *Geschiedenis van de wetenschappen in België 1815-2000*, eds. Robert Halleux, Jan Vandersmissen, Andrée Despy-Meyer, and Geert Vanpaemel (Brussel: Dexia Bank 2001), Vol. 1, 179-194.

Brigitte Van Tiggelen, *Chimie et chimistes de Belgique* (Bruxelles: Éditions Labor, 2004).

Brigitte Van Tiggelen and Hendrik Deelstra, "Belgium: From Industry to Academia: The Belgium Chemical Society, 1887-1914," in *Creating Networks in Chemistry: The Founding and Early History of Chemical Societies in Europe*, eds. Anita Kildebæk Nielsen and Soňa Štrbáňová (Cambridge: Royal Society of Chemistry, 2008), 23-42.

G.P.J. Verbong and E. Homburg, "Chemische kennis en de chemische industrie". In *Geschiedenis van de techniek in Nederland. De wording van een moderne samenleving, 1800-1890*, eds. H.W. Lintsen, et al., Vol. 5 (Zutphen: Walburg Pers 1994), 242-269.

Jan de Vries and Ad van der Woude, *The first modern economy: success, failure, and perseverance of the Dutch economy, 1500-1815* (Cambridge: Cambridge University Press, 1997).

D.A. Wittop Koning, *Compendium voor de geschiedenis van de pharmacie van Nederland* (Lochem/ Gent: De Tijdstroom, 1986).

## II

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### Symposia

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## 1. EARLY MODERN CHEMISTRY AND MECHANICAL PHILOSOPHY

*Introduction by Hiro Hirai*

The seminars, organised by Bernard Joly (University of Lille 3) for three years (2004-2007) in Lille (France), treated intensively questions concerned with the interrelations between corpuscular and mechanical philosophy and chemistry in early modern period. These seminars had as guest speakers a number of leading scholars on matter theories in the early modern period (Lawrence Principe, Antonio Clericuzio, Christoph Lüthy, Stephen Clucas, etc). The seminars' participants also formed an important part of two international conferences: "Robert Boyle and Natural Philosophy" (Bordeaux, March 2005) and "History of Alchemy and Chemistry" (Philadelphia, July 2006). Two more conferences have been held since in the same perspective: "Chemistry and Medicine" (Lille, March 2007) and "Chemistry in the Work of Philosophers" (Lille, June 2007). The present special session aimed to provide within the framework of the 6<sup>th</sup> ICHC a show-case of the outcome of this movement, by focusing upon interactions, boundary and neighborhood between early modern chemistry and mechanical philosophy. The following papers were read:

- Bernard Joly, *Etienne François Geoffroy, a French Chemist between England and Germany*.
- Rémi Franckowiak, *Eighteenth Century Chemistry, between natural Philosophy without Nature and Physics without Reason*.
- Luc Peterschmitt, *Homborg's Chemistry: a certain Truth in a disputable Physics*.
- Hiro Hirai, *Mechanical Agent in Renaissance Matter Theories*.

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## Eighteenth Century Chemistry: Between Natural Philosophy Without Nature and Physics Without Reason

Rémi Franckowiak\*

The aim of this paper is to show that, in 18<sup>th</sup> century Chemistry, at least in French Chemistry, there was nothing anymore but an instance of reality to consider, the existence of bodies itself. Chemistry gave up the idea of Principles, of a Reason or of a Nature transcending all the chemical phenomena, which prevailed hitherto in this science, without necessarily taking refuge in another system supposing an inaccessible reality; the only existing reality is that which appeared in laboratory. In this direction, without real base hidden behind tangible appearances, therefore without Nature, Chemistry abandoned natural Philosophy by definition. Chemistry's works fitted in at that moment with 'tragic processes' consisting in refusing any 'metaphysical' argument from any thought system to practise a clearly experimental physics,<sup>1</sup> or rather something which sounds like an oxymoron, a 'physics of artifice'; the artifice would not be anymore a continued Nature since, for chemists, Nature was henceforth defined as continued artifice. Without any order in Nature, Chemistry felt free to push back its boundaries and so chemical theory and practice find their "application to Physics, natural History, Medicine & animal Economy" to repeat the second part of Macquer's title in 1766 to *Dictionnaire de Chimie*,<sup>2</sup> and as one can see in the *Encyclopédie* of Diderot and d'Alembert.<sup>3</sup> Certainly 18th century Chemistry still seems to declare itself as a part of natural Philosophy and Physics. However, at the very beginning of the century, Wilhelm Homberg openly claimed a practice which was not simple Physics, but precisely, "chemical Physics".<sup>4</sup> The way, in the *Système des Connaissances Humaines* at the beginning of the first volume of the *Encyclopédie*, which leads from a Knowledge Tree trunk to Chemistry, was not rightly followed by Gabriel-François Venel in his article "Chymie" in the third volume: Chemistry was not either for him simple Physics or as he named it "ordinary Physics".<sup>5</sup> Chemistry was distinguished clearly by its studies on the world's small body phenomena; but if ordinary Physics tried to explain this world, Venel warned "all there will be badly".<sup>6</sup> This concern of distinguishing Chemistry like particular

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Physics, like particular natural Philosophy, was neither exclusively justified by its characteristic experimental dimension, neither by its interest for phenomena whose cause is invisible. From a chimico-chemical point of view, it was not the sign of a rupture either between Alchemy and Chemistry. This distinction stems rather from a new way of looking at bodies, starting with the search for an order –not natural– but artificial one. Chemists had opted for a world without nature, without metaphysical reason, without invisible reality hidden behind the palpable one; a world in which they approved unconditionally the existence of chemical bodies and facts, and not anymore a world in which they accepted their existence provided that there was any theoretical explanation. Eighteenth-century chemists place themselves within a world they absolutely didn't know. They gradually gave up their naturalist representations, while settling in artificialist ones. Adopting a tragic point of view on the matter probably gave them more self-confidence or even certain superiority in the field of natural sciences. Their work was not been curbed anymore by alleged natural materials they could only purify or damage or imitate, in other words they could only undo and redo what nature did; now their productions were their own creations: they were able to make by their own. Chemists gave up any *a priori* intellectual command of matter. This renunciation could look like a kind of humility when faced with the matter which was hard to know. So Homberg did not think to isolate true principles of mixts anymore (inaccessible anyway in their pure forms), but he only wanted to set apart some indecomposable and perfectly tangible substances from the last resolution of mixts. As for Venel, he claimed the right to work in “vagueness” and “approximation”,<sup>7</sup> and out of any system (for him, “Cartesian, corpuscular, Newtonian, academic or experimental” systems), even if he admitted that, in the history of Chemistry, systems played a role in the formation of Chemistry as a science.<sup>8</sup> However this humility did not mean chemists gave up the hope of any progress in Chemistry, but it only expressed their ‘artificialist’ posture, in opposition to the naturalist posture which was current in Chemistry hitherto. But chemists were far from being pure empiricists. They fully accepted the complication of the world of substances they tried to order, after the simplicity of the former world of Chemistry of Principles become chaotic.<sup>9</sup> Vagueness and approximation would become a scientific practice for Venel, “a specific right” (*un droit particulier*) of Chemists who have “their own & independent manner” (*leur manière propre & indépendante*); what it was about here was their “boldness” (*hardiesse*) (“one said the madness” (*on a dit la folie*), Venel writes), “Chemists’ enthusiasm” which “can give rise to genius” (*qui peut prêter au genie*).<sup>10</sup>

The idea of Nature in Chemistry was suggesting, not only a natural foundation for the chemical compounds –in other words being behind the appearance–, but



an order which transcended the disorder and the diversity of chemical substances. In the absence of a metaphysical postulate, or a Reason of things, 18th century chemists, like Homberg, or Geoffroy with his famous affinity table, or Rouelle with his neutral salt table, tried to gather chemical facts and bodies in wholes which made sense. With 1702 Homberg's Chemistry definition, matter took on the form of simple bodies still identified like principles, and ceased being a constraint in Chemistry's practice; knowledge of matter was not certain, neither was it a precondition. It was not anymore necessary for these principles to be themselves principles; it was enough that they were principles for the chemist in his relation with experiment. The "undisputed truth" (*la vérité certaine*) was related, for Homberg, with the immediately verifiable existence of bodies in laboratory. So that substances called salt, mercury, sulphur, water and earth were not anymore, as they were in the former century, instances of true principles of same names. Salt, mercury, sulphur, water and earth represented mainly, in the truth of their existence, some genera of bodies containing several species. For example, the genus salt was the genus of soluble and sapid substances, and contained the following species, acid salts, "salts which smell like urine" and alkaline salts, to which Homberg added another species, the essential salts, obtained by extraction, then crystallisation.<sup>11</sup> Therefore Chemistry was organised according to a new order, and not anymore according to an order following the triple nature of the production and conservation force sometimes called universal Spirit or universal Nature.<sup>12</sup> The very start of 18<sup>th</sup> century corresponded to a multiplication of the number of substances: after Salt Chemistry in 17<sup>th</sup> century for example, the "chemistry of the salts is the great affair" of the next century, according to Jean-Jacques Rousseau.<sup>13</sup>

In the first decades of 18th century, chemical bodies tended thus to take their place in a new general plan of all substances. Chemical experiments didn't really focus on a single subject, but on a species or a genus, as one can see, for instance, with Homberg's studies on the force of acid salts and alkali salts in 1699 and 1700, and on volatile salts of plants in 1701.<sup>14</sup> It was specially the case in 1718 with Etienne-François Geoffroy's "table des différents rapports observés entre différentes substances".<sup>15</sup> Taxonomy and nomenclature in Chemistry become real methods of knowledge. The absence of a transcendent order after the disappearance of the idea of Nature was offset by the classification and a certain nominalism. Chemists probably gave a name or classified more to know, than to recognise.<sup>16</sup> Geoffroy's table summed up approximately 75 possibilities of combinations of classified bodies according to a relative order of binding tendency in relation to the body at the top of each column. Geoffroy did not contend himself with summarising the behaviours of the 19 substances contained in the table, but gathered

some of them in 4 species in order to expose their general behaviours (these species were the acid liquors, salts, metals and the absorbing earth). According to Geoffroy, the table required only to be supplemented with additional experiments in order to identify other affinities.<sup>17</sup> The lack of theoretical explanations about the various phenomena of selective substance displacements shown in the table (in 1718 Geoffroy's memoir as well as in his 1720 memoir on the same subject<sup>18</sup>) must have disconcerted the French Academicians at the time. Fontenelle, the perpetual secretary of the Academy, most probably suspected a Newtonian reason to explain them, since he was the first to translate Geoffroy's word "*rapport*" by "attraction".<sup>19</sup> However in the review he gave on the table, he wrote that it is to be regretted that there is no reason to justify the various binding suitabilities between bodies observed in laboratory; he said: "[...] But from what active principle can one understand this more [or less] suitability?" ("*[...] Mais quel principe d'action peut-on concevoir dans ce plus [ou moins] de convenance ?*").<sup>20</sup> A few chemists initially tried to put forward some reasons for these affinities (e.g. Stahlian reasons for Gilles Boulduc, Cartesian ones for Louis Lemery<sup>21</sup>) before accepting the table for what it was:<sup>22</sup> The table just revealed a very useful order to establish operational strategies in Chemistry and to deduce the mechanism from confusing operations; and that "whatever the [active] principle may be", as Fontenelle rather disconcertingly notes it.<sup>23</sup> Geoffroy was indeed neither a Newtonian nor a Stahlian chemist but a chemist in close touch with his time; a chemist whose way of practicing Chemistry fully showed an artificialist approach. Therefore it is not needed to postulate any theoretical structure for the substances of the table, it was enough to contemplate this table; the reason is in the table and not in the bodies. Knowledge of a particular natural body was then substituted by artificial knowledge of the place of this body in a particular table's column. But it is to be noticed that Geoffroy did not speak of a law, even less a natural law, but quite simply of a "rule" in the chemical behaviours which he reported.<sup>24</sup> Indeed, what his table was about was not an external power (like subtle matter, or Newtonian attraction) which would drive bodies to be combined or to be separated. Affinities were what made combinations possible and not what caused them. In this artificialist approach, affinities in Chemistry were always considered just like laboratory results; in contrast a naturalist approach just considered the antecedents. In other words, chemical affinities were related to some circumstances in laboratory and not to the essence of bodies; they just referred to the possibility for elements to unite. Certain combinations happen to occur, others did not: Geoffroy's affinities did not refer anything else than this shared possibility to unite.<sup>25</sup> Chemical affinities were not used differently by chemists till the middle of the century;<sup>26</sup> hence the incomprehension of Buffon who wished to subsume all

these phenomena under a mathematical expression based on Newton's law attraction.<sup>27</sup> His incomprehension was not on the doctrine, Buffon is very well informed about Chemistry, but on the way of thinking about the chemical phenomena of the artificialist Chemistry which used no metaphysical postulate. He wrote: "The darkness of Chemistry is mainly due to the fact that one not much generalized its principles, & because one did not join them together with those of the high Physics. Chemists adopted affinities without understanding them, i.e. without understanding the relation between the cause and the effect which is nevertheless not other than that of the universal attraction" (*"L'obscurité de la chimie vient en grande partie de ce qu'on en a peu généralisé les principes, & qu'on ne les a pas réunis à ceux de la haute physique. Les chimistes ont adopté les affinités sans les comprendre, c'est-à-dire sans entendre le rapport de la cause à l'effet qui, néanmoins n'est autre que celui de l'attraction universelle"*).<sup>28</sup> However, affinities according to Venel, could only be a relative property of a heterogeneous matter.

The result of element combinations in the left part of Geoffroy's table was the subject of a new research on an artificial classification. This part related to the saline bodies, acid salts and alkali salts, which combined and formed mixts called "neutral salts". In 1743, Guillaume-François Rouelle suggested in a communication published in the volume for 1744 of *Mémoires de l'Académie Royale des Sciences*, a table of neutral salts according to their external crystalline shapes, and also according to the degree of heat and the way they crystallised. The memoir's title was very significant besides: "Memoir on neutral salts, in which one suggests a methodical division of these salts, which facilitates the means to reach the theory of their crystallisation" (*"Mémoire sur les sels neutres, dans lequel on propose une division méthodique de ces sels, qui facilite les moyens pour parvenir à la théorie de leur cristallisation"*).<sup>29</sup> Actually, this study was very far from a naturalist study of the crystallisation phenomena, but it was carried out with the hope to produce a theory of crystallisation by using nominalism and arbitrary taxonomy. Neutral salts did not relate back to natural reality, but to a subjective definition also containing some well defined categories of substances: "I call [...] neutral salt, any salt formed by the combination of any acid, either mineral or vegetable, with a fixed alkali, a volatile alkali, an absorbing earth, a metal substance, or an oil".<sup>30</sup> Neutral salts gather, according to the expression of Rouelle, in "families" or "classes", because of their common shapes and properties, and not according to a similarity of nature; they can undergo a "methodical division" by regarding closely "the only phenomena of crystallization". The reasons of the neutral salts' dissolution and of their crystallisation were unknown for Rouelle; he only noted that the opinions on those subjects are divided. He just observed that the parts of salts group together into crystals, what he called "the first law of crystallisation".

Rouelle defined three degrees of heat, each one divisible into three others,<sup>31</sup> in order to distribute neutral salts into six sections made up of four genera, which contained several neutral salt species; the last section being more or less that of the saline bodies that were unclassifiable elsewhere. The well known neutral salt species which existed in nature had already a chemical symbol to be represented, according to an algebraic model, linking the symbol of the acid with the symbol of the base using the sign “+” he calls “the small cross”. Rouelle also suggested in 1754 a completely different neutral salt taxonomy, but still with an artificialist mind.<sup>32</sup> Rouelle’s work will not be detailed further,<sup>33</sup> however it must be emphasised that a neutral salt was really a chemical concept at this date in history. Later Buffon estimated that the number of possible combinations between acids and bases was 474; in other words, there were 474 possible neutral salts.<sup>34</sup> Lavoisier, after having identified new acids and new salifiable bases, raised this number to 1152 possible neutral salts.<sup>35</sup> The substances about which they talked did not correspond to materials observed in laboratories or elsewhere. Rather, for the most of part, they corresponded to beings to be created, as they already belonged to a world established by artificialist reason; once they are obtained by the chemist, they become, in the words of Bachelard, “concepts which have been attained” (*des concepts réalisés*).<sup>36</sup> To a certain extent, can one say from then on that these saline bodies were natural? Chemists left the realms of actual reality for the realms of possibility. So, not only the real did not reach all the possibility, but the very possibility of Nature was far from being able to compete with the chemists’ possibilities. Artificialist Chemistry clearly went beyond the framework of naturalist Chemistry, and that occurred a hundred years earlier than Bachelard thought.<sup>37</sup>

Space does not permit to discuss the great mechanical philosophers’ interest in Chemistry, which was partly due to its ‘artificialism’. Nor about chrysopoetic works, as pure artificial productions for Geoffroy, Du Fay, Grosse, and Hellot. In the first half of 18<sup>th</sup> century Chrysopoeia became a thought of the present time, i.e. of what actually exists, and not a thought of past time, with its attempts to rediscover the elusive truth of Principles, as Malouin explained in his article “Alchimie” in the *Encyclopédie*.<sup>38</sup> Unfortunately neither is it possible to comment on Venel’s position on Chemistry;<sup>39</sup> Chemistry was according to him completely independent from ordinary Physics because of the absence of any metaphysical postulate. For him, the last two centuries Chemistry were “rich in facts, and in real chemical knowledge” (i.e. experimental) (*riche en faits, en connaissances vraiment chimiques*), but unfortunately “it lost its way by rising up” (i.e. in speculations) (*[elle] s’est égarée en s’élevant*), while prevailing itself to be the art which makes possible to go up to ‘divine Architect’, or even “the rival & reforming art of

Nature (*[l'art] rival & réformateur de la Nature*); generally in Physics, one has often mistaken “abstract notions for truths of existence” (*notions abstraites avec vérités d'existence*).<sup>40</sup> However a paradox must be noted: French Physicists practised a Newtonian Physics which tended to ‘disanimate’ Nature while replacing it by an inert matter which cannot escape a strict mechanism because it was subjected to blind laws. Therefore one can think that 18<sup>th</sup> century Chemistry, by claiming the right to work in ‘vagueness’ and ‘approximation’, unlike Physics which, for Venel, wanted at all costs to explain everything, or which, according to Fontenelle, finds the first origins of everything “by delicate speculations”,<sup>41</sup> let more place in its doctrine to Nature; Chemistry never really left Nature, in fact, it multiplied it.

Nevertheless the change in 18<sup>th</sup> century Chemistry was neither sudden nor necessarily radical. It was not sudden, because the recognition of only one instance of reality –the existence of bodies– was the completion of 17th century Chemistry movement (which has first rehabilitated the ‘corporal’ with the use of Salt Principle (Joseph Du Chesne), then the body with the practice of the second and palpable Principles (in Chemistry handbooks), and at last it had attempted to establish only probable –i.e. not ‘metaphysical’, not indemonstrable– Principles (Samuel Cottureau Du Clos, François Saint André<sup>42</sup>). It was not either necessarily radical, because the practice of Chemistry –which was not reduced to a simple empiricism– to try to determine a coherent order in the substance diversity for which Chemists can still use sometimes an indemonstrable Reason. But one can finally question oneself if the abandonment of a transcending Principle of unit in Chemistry in 18th century was not definitive.

## Notes

<sup>1</sup> For some recent texts on the more materialist approach of phenomena in 18th century Chemistry, see: John C. Powers, “Chemistry Without Principles: Herman Boerhaave on Instruments and Elements”, in Lawrence M. Principe (ed), *New Narratives in Eighteenth-Century Chemistry. Contributions from the First Francis Bacon Workshop, 21-23 April 2005* (Dordrecht: Springer, 2007), 45-61; John C. Powers, “Scrutinizing the Alchemists. Herman Boerhaave and the Testing of Chemistry”, in Lawrence M. Principe (ed), *Chymists and Chymistry. Studies in the History of Alchemy and Early Modern Chemistry* (Sagamore Beach: Watson Publishing International LLC), 227-238; Ku-Ming (Kevin) Chang, “From Vitalistic Cosmos to Materialistic World”, in Principe, *Chymists and Chymistry*, 215-225; Ursula Klein, “Experimental history and Herman Boerhaave’s chemistry of plants”, *Studies in History and Philosophy of Biological and Biomedical Sciences* 34 (2003): 533-567; Ursula Klein, W. Lefevre, *Materials in Eighteenth-Century Science. A Historical Ontology* (Cambridge MA: MIT Press, 2007).

- <sup>2</sup> Pierre-Joseph Macquer, *Dictionnaire de Chimie, contenant la théorie et la pratique de cette Science, son application à la Physique, à l'Histoire Naturelle, à la Médecine, & à l'économie animale*, (Paris: 1766), 2 vol.
- <sup>3</sup> Denis Diderot, Jean Le Rond d'Alembert (ed.), *Encyclopédie, Dictionnaire raisonné des Sciences, des Arts et des Métiers*, (Paris: 1751-1771), 17 vol.
- <sup>4</sup> See Wilhelm Homberg, "Essays de chimie", *Mémoires de l'Académie Royale des Sciences* (1702): 33-52; and Rémi Franckowiak, Luc Peterschmitt, "La chimie de Homberg: une vérité certaine dans une physique contestable", *Early Science and Medicine* 10/1 (2005): 65-90.
- <sup>5</sup> See Gabriel-François Venel, « Chymie », in Diderot, d'Alembert, *Encyclopédie*, 1753, vol. 3; and Rémi Franckowiak, "La chimie dans l'*Encyclopédie*: une branche tour à tour dépréciée, réévaluée et autonome", *Recherches sur Diderot et sur l'Encyclopédie* 40-41 (2006): 59-70.
- <sup>6</sup> Venel, "Chymie", in *Encyclopédie*, vol. 3, 416b.
- <sup>7</sup> Venel, "Chymie", in *Encyclopédie*, vol. 3, 416a.
- <sup>8</sup> Venel, "Chymie", in *Encyclopédie*, vol. 3, 409a.
- <sup>9</sup> See Rémi Franckowiak, "La chimie du XVII<sup>e</sup> siècle: une question de principes", *Methodos* 8 (2008), Chimie et mécanisme à l'âge classique, [On line]. On line from 11 April 2008. URL : <http://methodos.revues.org/document1823.html>.
- <sup>10</sup> Venel, "Chymie", in *Encyclopédie*, vol. 3, 409a.
- <sup>11</sup> See Franckowiak, Luc Peterschmitt, "La chimie de Homberg"; and Rémi Franckowiak, *Le développement des théories du Sel dans la chimie française de la fin du XVI<sup>e</sup> siècle à celle du XVIII<sup>e</sup>*, PhD dissertation (2002), part 2, § 1, forthcoming.
- <sup>12</sup> About this Spirit, see Hiro Hiraï, *Le concept de semence dans la théorie de la matière à la Renaissance de Marsile Ficin à Pierre Gassendi* (Turnhout: Brepols, 2005); Rémi Franckowiak, "Le Sel de Joseph Du Chesne: premier moteur de la Nature", in *Actes du Congrès d'Histoire des Sciences et des Techniques (SFHST, Poitiers, 2004)* (Cahiers d'Histoire et de Philosophie des Sciences: Paris, 2006), 79-92.
- <sup>13</sup> Jean-Jacques Rousseau, *Institutions chimiques* (1746-1753) (Fayard: Paris, 1999), 261.
- <sup>14</sup> In *Mémoires de l'Académie Royale des Sciences*: Wilhelm Homberg, "Observation sur la quantité exacte des sels volatils acides contenus dans les différens esprits acides", (1699): 44-51; "Essais pour examiner les sels des plantes" (1699): 69-74; "Observations sur la quantité d'acides absorbés par les alcalis terreux" (1700): 64-71; "Observations sur les sels volatiles des plantes" (1701): 219-223. See Franckowiak, *Le développement des théories du Sel*, part 2, § 1.
- <sup>15</sup> Etienne-François Geoffroy, "Table des différens rapports observés en chimie entre différentes substances", *Mémoires de l'Académie Royale des Sciences* (1718): 202-212.
- <sup>16</sup> It is what Gaston Bachelard (*Le pluralisme cohérent de la chimie moderne* (Vrin: Paris, (1932) 2000), 22-23) said about the 19<sup>th</sup> and 20<sup>th</sup> centuries chemistry.
- <sup>17</sup> According to Frederic L. Holmes (*Eighteenth-Century Chemistry as an investigate enterprise* (Berkeley: Univ. of California, 1989), 39-41), the affinity table was "a nodal point in the continuing evolution of a pragmatic chemistry of operations oriented around the concept of middle salts".
- <sup>18</sup> Etienne-François Geoffroy, "Eclaircissements sur la Table insérée dans les Mémoires de 1718 concernant les Rapports observés entre différentes Substances", *Mémoires de l'Académie Royale des Sciences* (1720): 20-34.
- <sup>19</sup> See Bernard Le Bovier de Fontenelle's review on Geoffroy's memoir, "Sur les rapports de différentes substances en Chimie", *Histoire de l'Académie Royale des Sciences pour l'année 1718* (1720): 35-37.
- <sup>20</sup> Fontenelle, "Sur les rapports", in *Histoire de l'Académie Royale des Sciences*, 36.



<sup>21</sup> See, in *Mémoires de l'Académie Royale des Sciences*, for Stahlian reasons: Gilles Boulduc, "Mémoire sur la qualité & les propriétés d'un Sel découvert en Espagne, qu'une Source produit naturellement ; & sur la conformité & identité qu'il a avec un Sel artificiel que Glauber, qui en est l'auteur, appelle Sel admirable" (1724): 118-137; "Essai d'analyse en général des nouvelles eaux minérales de Passy ; avec des raisons succinctes, tant de quelques phénomènes, qu'on y aperçoit dans de différentes circonstances, que des effets de quelques opérations, auxquelles on a eu recours pour discerner les matières, qu'elles contiennent dans leur état naturel" (1726): 306-327; "Examen d'un sel tiré de la terre en Dauphiné ; Par lequel on prouve, que c'est un Sel de Glauber naturel" (1727): 375-383; and for mechanistic reasons after 1718: Louis Lemery, "Observation nouvelle et singulière sur la Dissolution successive de plusieurs Sels dans l'Eau commune" (1724): 332-347; "Second mémoire, ou réflexions nouvelles sur une précipitation singulière de plusieurs sels par un autre sel, Déjà rapportée en 1724, & imprimée dans le Tome de la même année, sous le Titre d'*Observation nouvelle et curieuse*, sur la dissolution successive de différents Sels dans l'eau commune" (1727): 40-49; "Sur le Sublimé corrosif ; Et à cette occasion, Sur un article de l'Histoire de l'Académie Royale des Sciences de l'année 1699, où il s'agit de ce Sublimé" (1734): 259-294; "Nouvel éclaircissement sur l'alun, sur le vitriols, et particulièrement sur la Composition naturelle, & jusqu'à présent ignorée, du vitriol blanc ordinaire" (1735): 262-280; "Supplément aux deux mémoires que j'ai donnés en 1735, sur l'Alun et sur les Vitriols" (1736): 263-301.

<sup>22</sup> See for instance, in *Mémoires de l'Académie Royale des Sciences*: Henri-Louis Duhamel du Monceau, "Sur le Sel Ammoniac" (1735): 106-116; "Sur la base du sel marin" (1736): 215-232; and Duhamel, Grosse, "Des différentes manières de rendre le Tartre soluble" (1732): 323-342; "Sur les différentes manières de rendre le Tartre soluble. Seconde Partie" (1733): 260-272. Nevertheless, if Duhamel had had to give some reasons for the affinities, they would have been Stahlian ones; see Duhamel, "Suite des recherches sur le Sel Ammoniac. Troisième Partie", *Mémoires de l'Académie Royale des Sciences* (1735): 500-501 and 504. For this development, see Franckowiak, *Le développement des theories du Sel*, part 2, § 2.

<sup>23</sup> Fontenelle, "Sur les rapports", in *Histoire de l'Académie Royale des Sciences*, 37.

<sup>24</sup> Nevertheless, it is true that Geoffroy writes twice in the first page of his memoir (p. 202) the word 'laws', but in the plural form. Indeed, for him, there are only particular laws for particular substances. Each law - i.e. each '*rapport*' - is about the specific regular behaviour of a single substance (except for the two chemical genera: acid spirits and metal substances). No natural law exists, but a 'steady rule' (*une règle constante*) (p. 212). According to Geoffroy, his Table represents just a 'method' (p. 203), not more.

<sup>25</sup> See Clément Rosset, *L'anti-nature* (Paris : PUF, (1973) 2004), about Empédocle, part III, §2.

<sup>26</sup> Combinations were not "a result of affinities", as Mi Gyung Kim says (*Affinity, That Elusive Dream: A Genealogy of the Chemical Revolution* (Boston: MIT, 2003), 145). The rapport table is not, as she also affirms it (pp. 144-146), "a theoretical system", and affinities do not represent "a theory domain" either, but just a "method" as Geoffroy simply presents it (p. 203). According to him, his table, built from chemical operations, has a practical "utility" (p. 206) indeed, since it is to lighten the practice. Geoffroy writes the "rapport" in Chemistry is a "property" (p. 203) of which the effects are thus observable in laboratory. Once its existence is noted, the "rapport" appears like the "key" - to take again Geoffroy's word (p. 203) - which allows explaining the recombining of substances mixed together, and only "predicts" (p. 206) what was already observed before or in other circumstances.

- <sup>27</sup> See Rémi Franckowiak, “Rouelle, un vrai-faux anti-newtonien”, *Archives internationales d’histoire des sciences* 150-151/vol. 53 (2003): 240-255. About Buffon, see Thierry Hoquet, *Buffon : Histoire naturelle et Philosophie* (Paris: Honoré Champion, 2005).
- <sup>28</sup> Georges-Louis Leclerc comte de Buffon, *Histoire naturelle, générale et particulière, Servant de suite à la Théorie de la Terre, & d’introduction à l’histoire des Minéraux*, Supplément (Paris, 1774): vol. 1, 75.
- <sup>29</sup> Guillaume-François Rouelle, “Mémoire sur les sels neutres, dans lequel on propose une division méthodique de ces sels, qui facilite les moyens pour parvenir à la théorie de leur cristallisation”, *Mémoires de l’Académie Royale des Sciences* (1744): 353-365. See Rémi Franckowiak, “Les sels neutres de Guillaume-François Rouelle”, *Revue d’Histoire des Sciences* 55/4 (2002): 493-532.
- <sup>30</sup> “J’appelle sel neutre moyen ou salé, tout sel formé par l’union de quelqu’acide que ce soit, ou minéral ou végétal, avec un alkali fixe, un alkali volatil, une terre absorbante, une substance métallique, ou une huile” (Rouelle, “Mémoire sur les sels neutres”, 353). Everybody will not accept the oil in the definition of neutral salts, like d’Holbach; see Paul Tiry d’Holbach, “Sel”, in Diderot, d’Alembert, *Encyclopédie*, vol. 14, 903-904.
- <sup>31</sup> See Guillaume-François Rouelle, “Sur le sel marin”, *Mémoires de l’Académie Royale des Sciences* (1745): 57-79.
- <sup>32</sup> See at the bottom of the final table in 1744 Rouelle’s memoir, “Mémoire sur les sels neutres”.
- <sup>33</sup> See Franckowiak, *Le développement des theories du Sel*, part II, § 4.
- <sup>34</sup> Georges-Louis Leclerc comte de Buffon, *Histoire naturelle des Minéraux* (Paris, 1783), vol. 2, 162.
- <sup>35</sup> Antoine-Laurent Lavoisier, *Traité élémentaire de Chimie, présenté dans un ordre nouveau et d’après les découvertes modernes* (Paris, 1789), 128.
- <sup>36</sup> Bachelard, *Le pluralisme coherent*, 68.
- <sup>37</sup> See Bachelard, *Le pluralisme coherent*, 68-69.
- <sup>38</sup> Paul-Jacques Malouin, “Alchimie”, in Diderot, d’Alembert, *Encyclopédie*, 1751, vol. 1, 249a. See Franckowiak, “La chimie dans l’*Encyclopédie*”, 59-60.
- <sup>39</sup> See Venel, “Chymie”; Franckowiak, “La chimie dans l’*Encyclopédie*”, and “Sur un air de chimie dans l’*Encyclopédie*”, in *Revue sur Diderot et l’Encyclopédie*, forthcoming.
- <sup>40</sup> See Venel, “Chymie”, 408b-409a.
- <sup>41</sup> Bernard Le Bouvier de Fontenelle, *Histoire de l’Académie Royale des Sciences, depuis son établissement en 1666 jusqu’en 1686*, (Paris, 1733), vol. 1, 79-81.
- <sup>42</sup> Franckowiak, “La chimie du XVIIIe siècle”.



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## “Mechanical” Agent in Renaissance Matter Theories

Hiro Hirai\*

### Abstract

The present paper aimed to give a new light on the notion of “mechanical” (*mechanicus*) agent, which was often used in the tradition of Renaissance natural philosophy and especially in Paracelsian chemistry. It traced the evolution of this notion from Marsilio Ficino until the time of Joan Baptista Van Helmont and its impact on the natural philosophers of new generation such as Pierre Gassendi and Robert Boyle. By doing this, it suggested a reconsideration of the accepted interpretation made by historians of science in the past. The paper, was based on the author’s book, *Le concept de semence dans les théories de la matière à la Renaissance* (Turnhout: Brepols, 2005) treated in particular the idea of “archeus” of Paracelsian chemists.

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## Etienne-François Geoffroy, un chimiste français entre l'Angleterre et l'Allemagne

Bernard Joly\*

Dans les premières années du XVIII<sup>e</sup> siècle, les chimistes français, sans doute influencés par leurs collègues physiciens, se référaient souvent à une conception mécaniste de la nature d'inspiration cartésienne, sans pour autant qu'il soit possible de parler du développement d'une chimie cartésienne. En effet, la manière selon laquelle Descartes avait traité des objets de la chimie dans la quatrième partie des *Principes de la philosophie*, en réduisant toutes les opérations chimiques à des processus strictement mécaniques faisait obstacle au développement d'une science chimique qui puisse conserver son autonomie et sa spécificité à l'égard de la physique.<sup>1</sup>

C'est surtout Nicolas Lémery (1645-1715) qui contribua au développement d'une chimie strictement mécaniste en popularisant dans son célèbre *Cours de chimie* de 1675<sup>2</sup> une interprétation des relations entre les acides et les alcalis fondée sur l'existence de pointes et de pores qu'il tirait des travaux de François André.<sup>3</sup> Il n'existe rien d'équivalent dans l'œuvre de Descartes, mais on a pu voir cependant dans une telle théorie l'expression d'une chimie cartésienne, dans la mesure où il s'agit de rendre compte des opérations de la chimie, non plus en invoquant l'efficacité de principes ou d'éléments apportant aux corps mixtes leurs propriétés, mais en s'appuyant exclusivement sur la grandeur, la figure et le mouvement des corpuscules de la matière, ce qui est parfaitement conforme à l'esprit de la physique cartésienne.

Dans les premières années du XVIII<sup>e</sup> siècle, Louis Lémery (1677-1743), le fils de Nicolas, fut le principal artisan du développement de cette chimie mécaniste, dont on trouve une spectaculaire application dans le mémoire intitulé "Conjectures et réflexions sur la matière du feu ou de la lumière" qu'il présenta le 13 novembre 1709 devant l'Académie royale des sciences.<sup>4</sup> Il considérait que le feu, comme la lumière et le Soleil lui-même, était constitué des fines particules d'une matière subtile qui s'insinuait dans les pores de la matière et s'y laissait enfermer en con-

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servant toutes ses propriétés, ce qui apportait une élégante solution à l'irritant problème de l'augmentation du poids des métaux que l'on calcine. Mais c'est également à l'occasion de la querelle qui l'opposa à Etienne-François Geoffroy (1672-1731) de 1704 à 1708 à propos de la fabrication artificielle du fer que Louis Lémery eut l'occasion de mettre en œuvre son interprétation mécaniste des phénomènes chimiques. Je ne reviendrai pas ici sur les détails de cette querelle, dont j'ai récemment présenté les principaux moments.<sup>5</sup> Il suffit de rappeler qu'il s'agissait d'expliquer la présence de fer dans des cendres végétales : résultait-elle d'une production nouvelle effectuée à l'occasion de la combustion, comme l'affirmait Geoffroy, ou provenait-elle de la terre, comme le défendait Lémery, en supposant que les fines particules du métal pouvaient dans certaines conditions se hisser dans les fibres du végétal ?

Ce différent permet d'illustrer tout ce qui opposait les deux chimistes dans la manière d'invoquer les acquis de leur formation de chimiste en vue de résoudre un problème difficile. L'éducation que Louis Lémery et Etienne-François Geoffroy reçurent de leur père était bien différente. Si l'on en croit l'éloge de Dortous de Mairan en 1743, Nicolas Lémery dissuada son fils d'entreprendre des études de juriste pour l'orienter vers la médecine et la chimie selon les principes de "la philosophie moderne qui n'était autre en ce temps-là que celle de Descartes."<sup>6</sup> Louis Lémery devint donc docteur en médecine à 21 ans et entra à l'Académie royale des sciences à 23 ans, comme élève de Tournefort, puis de son père. Il semble n'avoir quitté la France qu'en une seule occasion, lorsqu'il raccompagna à Madrid l'infante d'Espagne. Il fit toute sa carrière à Paris, comme professeur de chimie au Jardin royal des plantes à partir de 1708 et comme médecin enseignant à l'Hôtel-Dieu à partir de 1710. Se montrant un fidèle disciple de son père, que Dortous de Mairan n'hésitait pas à appeler "le Descartes de la chimie", il était persuadé que l'application à la chimie des principes du mécanisme cartésien était le seul moyen de faire sortir cette science des obscurités du passé. Il n'était donc pas nécessaire d'exercer sa curiosité à étudier les doctrines anciennes, pas plus que celles des auteurs étrangers qui, faute d'être cartésiens, lui paraissaient attachés aux errements de la tradition alchimique.

Geoffroy, au contraire, reçut une éducation éclectique, puisque Fontenelle rapporte dans son éloge que son père organisait à son domicile des conférences où intervenaient des physiciens, des médecins et des chimistes, parmi lesquels Homberg, qui sera le véritable maître de Geoffroy en chimie.<sup>7</sup> C'est, selon l'expression de Fontenelle, une véritable "éducation d'un fils de ministre" dont bénéficia ainsi le jeune homme, son père considérant que la profession d'apothicaire à laquelle il le destinait requerrait des connaissances très étendues. Mais surtout, Etienne-

François Geoffroy entreprit de nombreux voyages, d'abord à Montpellier, pour recevoir une formation en pharmacie différente de celle que son père pouvait lui prodiguer, puis en Angleterre, où "il gagna l'amitié de la plupart des Illustres d'un pays qui en produit tant", en Hollande, "où il vit d'autres savants, fit d'autres observations, acquit de nouvelles connaissances" et enfin en Italie.<sup>8</sup> Par la suite, il fit comme Louis Lémery une carrière uniquement parisienne, enseignant la chimie au Jardin royal des plantes dès 1707, professeur de médecine au Collège royal en 1709, mais à la différence de son collègue, il ne cessa de s'inspirer des travaux étrangers, et en particulier ceux de Stahl et des proches de Newton, préférant mêler les apports de la tradition alchimique et de la philosophie naturelle des anglais plutôt que de s'enfermer dans le carcan de la physique d'inspiration cartésienne.

### **Geoffroy et les Anglais**

Geoffroy se rendit à Londres en 1698 pour accompagner le comte de Tallard, nommé ambassadeur en Angleterre, en tant que médecin personnel, et cela alors qu'il ne deviendra docteur en médecine qu'en 1704. Il rencontre alors Hans Sloane, secrétaire de la Royal Society dont il devient rapidement membre. Les *Philosophical transactions* gardent la trace du passage de Geoffroy, puisque le numéro de février 1699 présente un bref rapport intitulé "a way to make two clear spirituous inflammable liquors, which differ very little in taste and smell, and being mixted together, do give a fine carnation colour, withouth either sensible fermentation or alteration."<sup>9</sup> Les expériences permettant la fabrication de ces deux liqueurs et la présentation du résultat de leur mélange tiennent en quelques lignes, mais le document se termine en précisant que Geoffroy lui-même présenta les deux substances lors d'une réunion de la Royal Society et qu'il y reproduisit avec succès la première des expériences mentionnées. De retour à Paris, il envoie le 21 décembre 1698 une lettre à Sloane sur les eaux minérales de Saint-Amand, qui est immédiatement publiée dans les *Philosophical transactions*.<sup>10</sup> Il y présente les résultats des opérations chimiques qu'il a effectuées pour déterminer la composition de ces eaux et en déduire ses vertus médicinales.

Geoffroy ayant été admis comme élève de Homberg à l'Académie royale des Sciences dès janvier 1699, il est immédiatement chargé d'établir des liens entre l'institution française et la Royal Society.<sup>11</sup> On trouve un exemple du travail de Geoffroy en 1703 : le procès-verbal du 24 juillet de cette année signale que "M. Geoffroy a lu un extrait des Transactions philosophiques contenant les comparaisons de divers degrés de chaleur faites selon une certaine méthode qu'on exa-

minera par le thermomètre de monsieur Amontons.”<sup>12</sup> En effet, Amontons, qui avait effectué en juin 1702 une présentation du nouveau thermomètre à esprit de vin et expliqué en avril 1703 comment ce dernier pouvait servir d'étalon pour rectifier les mesures faites avec les anciens,<sup>13</sup> développa devant l'Académie le 24 juillet 1703 des remarques sur la table présentée par Geoffroy avec une comparaison entre les mesures effectuées par l'auteur anglais et les siennes.<sup>14</sup>

Dans le même temps, Geoffroy informait bien entendu ses collègues anglais des travaux parisiens : il envoie le 7 mars 1699 une lettre à Sloane, publiée le mois suivant, dans laquelle il présente la nouvelle organisation de l'Académie des sciences qui venait d'être approuvée par le roi.<sup>15</sup> En 1702, il transmet une lettre, qui fut publiée dans les *Philosophical transactions* de mars-avril 1702, dans laquelle Blondel rendait compte à l'un de ses amis des travaux de la séance de l'Académie royale des sciences du 12 novembre 1701.<sup>16</sup>

Dans un tel contexte, la présentation par Geoffroy, devant ses collègues de l'Académie royale des sciences, d'extraits de l'*Opticks* de Newton n'apparaît plus comme un événement isolé. Geoffroy, on le sait, entreprit le 12 juin 1706 la lecture d'extraits de l'ouvrage traduits de l'anglais par ses soins, et ce de manière épisodique jusqu'en 1707. On ignore quels furent les passages ainsi présentés, mais il faut rappeler que la première édition de l'*Opticks* en 1704 ne comportait pas encore la célèbre “Question 31”, qui ne fut introduite que dans l'édition latine de 1706, en tant que “Question 23”, puis dans la seconde édition anglaise en 1717. Pour autant, il semble difficile d'admettre que Geoffroy ait pu ignorer ces éditions successives et qu'au moment où il composa sa “Table des différents rapports” publiée en 1718<sup>17</sup> il ne se soit pas souvenu de ces célèbres pages où Newton décrit les mouvements d'attraction et de répulsion entre les particules d'acides, de sels et de substances métalliques d'où résultent les précipitations. Certes, Newton se contente de présenter un catalogue d'opérations chimiques qui est fort loin de l'ordre méticuleux de la table de Geoffroy, même si certaines de ces opérations, comme celles concernant l'interaction des acides, des alcalis et des métaux se retrouvent chez les deux auteurs. Surtout, Newton veut démontrer une thèse : “There are therefore Agents in nature able to make the Particles of Bodies stick together by very strong Attractions.”<sup>18</sup> Geoffroy au contraire, qui se garde bien de parler d'attraction, se contente d'établir ce qu'il appelle une “proposition d'une très grande étendue dans la chimie”, sans jamais évoquer un agent qui serait responsable de ces “dispositions à se joindre l'une avec l'autre” qui caractérisent certaines substances.<sup>19</sup> Il demeure que l'on voit mal comment il n'aurait pas lui-même aperçu une ressemblance entre sa table et les attractions newtoniennes que Fontenelle signala immédiatement.

Geoffroy profita de ses liens avec les deux institutions savantes pour donner une plus grande diffusion à certains de ses travaux, qu'il présenta devant les deux assemblées : c'est ainsi que la première communication qu'il fit devant l'Académie des sciences le 12 mai 1700, "Observations sur les dissolutions et sur les fermentations que l'on peut appeler froides parce qu'elles sont accompagnées du refroidissement des liqueurs dans lesquelles elles se passent",<sup>20</sup> fut également publiée dans les *Philosophical transactions* de septembre 1701.<sup>21</sup> Mais surtout, on retrouve dans les *Philosophical transactions* de juillet 1709, sous le titre "Experiments upon metals, made with the burning-glass of the Duke of Orleans"<sup>22</sup> la traduction fidèle, à une exception près,<sup>23</sup> de la communication présentée devant l'Académie des sciences deux mois auparavant (2 mai 1709).<sup>24</sup> Il s'agit d'un texte important à plus d'un titre. D'abord parce qu'en utilisant le miroir ardent pour calciner les métaux, Geoffroy croit se livrer à leur analyse et faire apparaître les deux composants principaux de tout métal : un soufre ou substance huileuse et une terre vitrifiable ou chaux, ce qui semble conforter sa position dans la querelle qui vient de l'opposer pendant quatre ans à Louis Lémery. Le second point important, c'est que Geoffroy expose très clairement dans ce texte la réversibilité de la transformation d'un métal en chaux et de sa chaux en métal, mettant ainsi en évidence que la production "artificielle" du fer n'est que la mise en œuvre d'un procédé parfaitement naturel. On aperçoit immédiatement la similitude avec la théorie du phlogistique de Stahl : les théories de Geoffroy concernant la formation des métaux étaient en effet directement inspirées des travaux de Becher et de Stahl.

### Geoffroy et la chimie allemande

Dans le premier mémoire qu'il consacre à cette question en 1704,<sup>25</sup> Geoffroy s'inspire d'une recette proposée par Becher en 1671 dans un supplément à la *Physica subterranea* de 1669 intitulé *Experimentum chymicum novum quo artificialis et instantanea metallorum generatio et transmutatio ad oculum demonstratur*.<sup>26</sup> Becher y présente un procédé de fabrication du fer par la cuisson d'un mélange de limon et d'huile de lin.<sup>27</sup> Trois ans plus tard, Geoffroy se justifie en développant une théorie de la composition des métaux implicitement inspirée de la doctrine de Becher et de Stahl : le fer contient trois principes, un esprit acide, la terre vitrifiable et le principe "sulphureux", que l'on retrouve dans toutes les matières inflammables, comme l'huile de lin.<sup>28</sup> Il explique alors le processus de formation du fer de la manière suivante : "dans la fermentation qui fait la flamme, la partie terreuse s'unit très étroitement avec quelque portion d'acide et de soufre, d'où naissent les nouvelles molécules ferrugineuses". "Fermentation" pour désigner le

processus de combustion, “molécules” pour désigner les agrégats constitutifs d’une espèce minérale, c’est là le vocabulaire de Stahl dans sa *Zymotechnia fundamentalis, seu fermentationis theoria generalis* de 1697. Mais il y a plus. Dans les pages suivantes de son mémoire, Geoffroy insiste sur le rôle du principe “sulphureux” dans la calcination des métaux. Ainsi, écrit-il :

“Quelque fixe que soit le principe sulphureux dans le fer, le grand feu ne laisse pas de l’enlever et de convertir ce métal, après une longue calcination, en une cendre rougeâtre qu’on nomme Safran de Mars [oxyde de fer]. (...) Si on mêle cette cendre avec de l’huile de lin et qu’on les calcine ensemble, on la convertira en fer : et dans cette opération la terre du fer reprend le principe sulphureux qu’elle avait perdue. D’où il paraît qu’en ôtant au fer le principe sulphureux il cesse d’être métal, ce n’est plus qu’une terre susceptible de vitrification ; si au contraire on rend à cette terre son principe sulphureux, elle devient aussitôt fusible, malléable, ductile, en un mot c’est du métal.”<sup>29</sup>

Le soufre principe, dont Geoffroy a emprunté la théorie à Homberg joue bien ici le rôle du phlogistique. La similitude entre les explications de Geoffroy et celles de Stahl, conduit à penser qu’un jeu d’influence, peut-être réciproque, s’était alors établi entre les chimistes parisiens et ceux de Halle.<sup>30</sup>

Treize ans plus tard, Geoffroy se réfère explicitement à Stahl dans les “Eclaircissements sur la table insérée dans les mémoires de 1718 concernant les rapports observés entre différentes substances”.<sup>31</sup> Il s’agit de répondre à de supposées anomalies observées par rapport à ce que prévoit la table des affinités. Ainsi, s’il est vrai que les acides ont plus de rapport aux alkalis qu’aux substances métalliques, comment se fait-il que des substances métalliques détachent parfois les acides des alkalis, comme lorsque l’on met de la limaille de fer en présence d’un sel ammoniac (qui contient de l’acide et de l’alkali) ? C’est qu’il faut distinguer ici le fer de la limaille de fer. Cette dernière contient inévitablement de la rouille, c’est-à-dire un métal “fermenté” ou “putréfié” de telle sorte que son sel alkali fixe, quoique caché, détachera les acides de l’alkali moins puissant auquel ils étaient liés. C’est alors Stahl qui est invoqué, avec une citation latine du *Specimen Beccherianum* où le chimiste allemand reconnaît que dans certains cas des métaux peuvent décomposer le sel ammoniac.

Le mémoire se poursuit avec l’examen d’une nouvelle objection “proposée par M. Neuman, chimiste du roy de Prusse”, dont on apprendra quelques pages plus loin qu’il réside à Paris où il diffuse des problèmes de chimie que Stahl lui fait parvenir par lettre.<sup>32</sup> Il s’agit ici pour Geoffroy de montrer qu’il a eu raison de placer le “principe huileux” au dessus du sel alkali dans la colonne de l’acide vitriolique,

indiquant ainsi le moyen de séparer l'acide vitriolique du sel de tartre. Tout ce passage est une reprise de la doctrine de Stahl, avec cette fois-ci une référence explicite à la *Zymotechnia*. Mais surtout, Geoffroy signale, pour la première fois semble-t-il, que Stahl nomme le principe huileux "principe phlogistique".

Il est donc ici tout à fait clair que Geoffroy tire de la lecture des ouvrages de Stahl, avec lequel il est en rapport par l'intermédiaire de Neuman, de puissants arguments pour défendre sa table des affinités contre les prétendues anomalies que dénoncent ses collègues français.

## Conclusion

Pour conclure, j'évoquerai le *Nouveau cours de chymie suivant les principes de Newton et de Stahl*. Cet ouvrage anonyme paru en 1723 fut parfois attribué à Jean-Baptiste Senac (1693-1770), un médecin qui publia plusieurs ouvrages d'anatomie et de physiologie.<sup>33</sup> Sans doute convient-il plutôt d'y voir l'œuvre de quelques élèves de Geoffroy et de Boulduc s'inspirant des notes prises pendant les cours de leurs maîtres, comme le suggéra plus tard Baron dans la préface de sa réédition du *Cours de chymie* de Nicolas Lémery.<sup>34</sup> Quoiqu'il en soit, au-delà de ses imperfections, l'ouvrage représente assez bien ce que pouvait être à l'époque l'enseignement d'un chimiste ouvert à la fois aux influences anglaises et allemandes. L'ouvrage n'est pas homogène. Dans la première partie, en forme d'introduction générale, l'auteur développe la théorie stahlienne des trois terres de Becher et de l'eau, à partir desquelles se constituent par concrétion les sels (acides et alkalis) et les soufres, composés d'un acide et d'un principe inflammable bientôt nommé phlogistique. C'est également le phlogistique, désigné à nouveau comme "principe sulphureux" qui est invoqué pour rendre compte de la constitution des métaux : l'auteur anonyme s'exprime ici en des termes identiques à ceux des mémoires de Geoffroy que nous avons examinés tout à l'heure. Pour rendre compte des diverses manières dont les corps ainsi constitués entrent en rapports les uns avec les autres, c'est la théorie des affinités qui est développée, et l'on voit alors qu'elle s'accorde parfaitement avec la doctrine de Stahl.<sup>35</sup> La suite de l'ouvrage, intitulée "Les opérations de chymie en général", est bien différente puisqu'elle s'inspire explicitement de la chimie que Keill<sup>36</sup> et Freind<sup>37</sup> ont développée "selon les principes de monsieur Newton" (p. 152). Ce sont alors des explications de type mécaniste qui se substituent à celles de Stahl, dont les théories sont pourtant à nouveau évoquées dans la très longue "seconde partie" de l'ouvrage (plus de 500 pages in 4°) où elles se mêlent aux recettes empruntées aux "cours de chymie" du XVII<sup>e</sup> siècle, et en particulier à celui de Nicolas Lémery.<sup>38</sup>



Les interprétations mécanistes de la chimie de Louis Lémery allaient bientôt sombrer dans l'oubli, en même temps que la physique cartésienne cèderait peu à peu le pas en France à la physique newtonienne, tandis que la table des affinités de Geoffroy marquerait durablement les travaux chimiques pendant plus d'un siècle. J'aime voir dans ce succès le résultat d'une ouverture vers les thèses étrangères qui fit cruellement défaut à la chimie mécaniste de cette époque.

## Notes

<sup>1</sup> Bernard Joly, "Descartes et la chimie" in *L'esprit cartésien*, éd. Bernard Bourgeois et Jacques Havet (Paris : Vrin, 2000), 216-221.

<sup>2</sup> Michel Bougard, *La chimie de Nicolas Lémery* (Turnhout : Brepols, 1999).

<sup>3</sup> François André, *Entretiens sur l'acide et l'alcali* (Paris : 1672). La seconde édition (Paris : 1677), qui contient une réponse aux objections développées par Boyle en 1675 dans ses *Reflections upon the hypothesis of Alkali and Acidum* (*The works of Robert Boyle*, ed. Michael Hunter and Edward B. Davis (London : Pickering & Chatto, 2000), vol. VIII, 409-411) peut être consultée en ligne sur le site [http:// polib.poleuniv-lille-npdc.fr](http://polib.poleuniv-lille-npdc.fr).

<sup>4</sup> *Mémoires de l'ARS* (= *Mémoires de l'Académie Royale des Sciences*), 1711 (1706), 88-96. La seconde date, entre parenthèses, est celle de l'année où fut présentée la communication. La première est celle de l'année de publication de l'édition des mémoires à laquelle je me réfère.

<sup>5</sup> Bernard Joly, "Quarrels between Etienne-François Geoffroy and Louis Lémery at the Académie Royale des sciences in the Early Eighteenth Century : Mechanism and Alchemy", in *Chymists and chymistry. Studies in the History of Alchemy and Early Modern Chemistry*, ed. Lawrence M. Principe (Sagamore Beach : Chemical Heritage Foundation/Science History Publications, 2007), 203-214.

<sup>6</sup> Dortous de Mairan, "Eloge de M. Lémery", in *Histoire de l'ARS* (*Histoire de l'Académie Royale des Sciences*), 1746 (1743), 195-208, plus spécialement 196.

<sup>7</sup> Fontenelle, "Eloge de M. Geoffroy", in *Histoire de l'ARS*, 1734 (1731), 93-100.

<sup>8</sup> Ce sont toujours, bien entendu, les expressions de Fontenelle.

<sup>9</sup> *Philosophical transactions*, vol. 21 (1699), numéro 249, 43-44.

<sup>10</sup> *Philosophical transactions*, vol. 20 (1698), numéro 247, 430-432.

<sup>11</sup> *Procès-verbaux de l'ARS* (*Procès-verbaux de l'Académie Royale des Sciences*), tome 18 (1699), f°143v-144r. La liste des projets des académiciens pour l'année 1699, lue lors de la séance du 28 février définit ainsi le travail de Geoffroy : "Il travaillera sur les sels essentiels et il a même déjà commencé. De plus, comme il est de la Société Royale de Londres et que le commerce des lettres qu'il entretient avec monsieur Sloane qui en est secrétaire lui donne moyen d'être instruit de plusieurs choses particulières et curieuses qui se traitent dans cette société, et qui ne sont pas toujours dans les transactions ou Journaux qu'elle ordonne, il vérifiera et rapportera à la Compagnie les expériences les plus considérables qui viendront à sa connaissance."

<sup>12</sup> *Procès-verbaux de l'ARS*, tome 22 (1703), f°266v.

<sup>13</sup> *Mémoires de l'ARS*, 1720 (1703), 50-56.

<sup>14</sup> *Mémoires de l'ARS*, 1720 (1703), 200-212.

<sup>15</sup> *Philosophical transactions*, vol. 21 (1699), numéro 251, 144-145.

<sup>16</sup> *Philosophical transactions*, vol. 23 (1702), numéro 278, 1097-1105. Il s'agit des communications de Cassini sur le prolongement de la méridienne de l'observatoire jusqu'aux Pyrénées, Boulduc sur les effets d'un nouveau purgatif, Morin sur le passage des boissons dans les urines et Marchand sur la supériorité des plantes de France sur les plantes exotiques (*Mémoires de l'ARS*, 1743 (1701), 171-220).

<sup>17</sup> E. F. Geoffroy, "Table des différents rapports observés en chimie entre différentes substances", in *Mémoires de l'ARS*, 1720, (1718), 202-212.

<sup>18</sup> Isaac Newton, *Opticks* (New-York: Dover publications, 1952/1979), 394.

<sup>19</sup> *Mémoires de l'ARS*, 1720 (1718), 203.

<sup>20</sup> *Mémoires de l'ARS*, 1742 (1700), 110-121.

<sup>21</sup> *Philosophical transactions*, vol. 22 (1701), n° 274, pp. 951-962 : "Observations upon the dissolutions and fermentations which we may call cold, because they are accompagnied with a coolness of the liquors into which they pass. And of a new thermometer. Extracted out of a discourse, which Mr Geoffroy, FRS, made in the public meeting of the Royal Academy of Sciences the 21st of April" (la date est erronée).

<sup>22</sup> *Philosophical transactions*, vol. 26 (1709), n° 322, 378-386.

<sup>23</sup> Dans la version anglaise, Geoffroy prend soin d'effacer les références à ses publications antérieures qui portaient les marques de la polémique avec Lémery.

<sup>24</sup> *Mémoires de l'ARS*, 1742 (1700), 162-176. Le titre était alors "Expériences sur les métaux faites avec le verre ardent du palais Royal".

<sup>25</sup> "Manière de recomposer le soufre commun par la réunion de ses principes et d'en composer de nouveau par le mélange de semblables substances, avec quelques conjectures sur la composition des métaux", in *Mémoires de l'ARS*, 1745 (1704) 278-286. Mémoire daté du 12 novembre 1704.

<sup>26</sup> D'abord publié séparément, l'*Experimentum chymicum novum* fut intégré à la seconde édition de la *Physica subterranea* en 1681. On le retrouve dans l'édition complète de la *Physica subterranea* que Georg Ernst Stahl publia à Leipzig avec le *Specimen beccheriarum*, en 1703, réédité en 1738.

<sup>27</sup> C'est d'ailleurs cette référence à Becher que Lémery reprochera à son collègue quatre ans plus tard, dans le mémoire de 1708 qui clôt la querelle : "Nouvel éclaircissement sur la prétendue production artificielle du fer, publiée par Becher et soutenue par M. Geoffroy", in *Mémoires de l'ARS*, 1709 (1708), 376-402. Mémoire daté du 5 décembre 1708.

<sup>28</sup> "Eclaircissements sur la production artificielle du fer et sur la composition des autres métaux", *Mémoires de l'ARS*, 1708 (1707), 176-188. Mémoire daté du 21 mai 1707.

<sup>29</sup> *Mémoires de l'ARS*, 182. La même opération s'applique à l'antimoine : "Lorsque la plus grande partie de son soufre s'est exhalé, il perd sa forme métallique et il reste en cendre grise, qui fondue prend la forme de verre au lieu de celle de métal qu'elle avait avant la calcination. Si l'on veut rendre à ce verre ou à cette cendre la forme métallique, il ne faut que lui rendre ce principe sulfureux qu'elle a perdu en la refondant avec quelque matière inflammable, comme le tartre, le charbon et toute autre matière semblable, et elle se remet aussitôt en Régule."

<sup>30</sup> Les possibles relations entre Homberg, Geoffroy et Stahl ont déjà été brièvement évoquées par Lawrence Principe ("Wilhem Homberg : chymical corpuscularism and Chrysopoeia in the early eighteenth century" in *Late medieval and early modern corpuscular matter theories*, ed. Christoph Lüthy, John E. Murdoch and William R. Newman (Leiden: Brill, 2001) 535-556) ainsi que par Mi Gyung Kim (*Affinity, that elusive dream* (Cambridge (Ma): The MIT Press, 2003) 150).

<sup>31</sup> *Mémoires de l'ARS*, 1722 (1720), pp. 20-34.

<sup>32</sup> Sur Caspar Neumann (1683-1737), apothicaire du roi de Prusse et professeur au “Collegium Medico-Chirurgicum” de Berlin, voir J.R. Partington, *A History of Chemistry* (London: MacMillan and Co Ltd, 1961), vol. II, 702-706. C’est grâce à un traitement du roi de Prusse, sur les recommandations de Stahl, qu’il put résider quelques années à Paris et travailler avec les chimistes de l’Académie royale des sciences.

<sup>33</sup> Certes, son *Anatomie d’Heister*, qui parut en 1724, fut publiée chez le même éditeur que le *Nouveau cours de chymie*, mais on peut se demander ce qui aurait pu conduire ce médecin, qui ne semble pas s’être intéressé à la chimie par ailleurs, à publier un important ouvrage de chimie, fort bien documenté tant sur le plan historique que scientifique.

<sup>34</sup> *Cours de chymie (...) par M. Lémery (...). Nouvelle édition revue, corrigée et augmentée d’un grand nombre de notes (...) par M. Baron, docteur en médecine et de l’académie royale des sciences* (Paris : 1756).

<sup>35</sup> Parlant de l’acide vitriolique, l’auteur écrit : “on verra dans la Table que je donnerai comme il s’unit encore moins avec certains métaux qu’avec d’autres...” (p. 83), ce qui laisse perplexe : si l’auteur est Geoffroy, pourquoi parle-t-il au futur d’une Table qu’il a présentée cinq ans plus tôt ? Faut-il croire que le texte publié en 1723 avait été rédigé plusieurs années auparavant ? Cela conforterait l’hypothèse de la publication tardive d’un manuscrit issu des notes d’un cours d’abord donné oralement. Voir à ce sujet Bernard Joly, “L’anti-newtonianisme dans la chimie française au début du XVIII<sup>e</sup> siècle”, *Archives internationales d’histoire des sciences*, n°150-151, vol. 53 (2003) : 213-224, plus spécialement 222-224.

<sup>36</sup> John Keill (1671-1721), professeur de philosophie naturelle et d’astronomie à Oxford, présenta en 1708 un mémoire dans lequel il défendait une conception mécaniste et newtonienne des opérations chimiques (*Philosophical transactions*, 1708, n° XXVI, 97-110). Voir Partington, *History of Chemistry*, vol. II, 478-479.

<sup>37</sup> John Freind (1675-1728), collègue de Keill, publia à Londres en 1704 ses *Chymical lectures* dans lesquelles il explique de manière détaillée les opérations chimiques en invoquant des forces attractives. Des pages entières de cet ouvrages sont traduites dans le *Nouveau cours de chymie*.

<sup>38</sup> Baron n’avait pas tort de remarquer que l’ouvrage de Lémery se trouve ainsi pillé, sans que l’on puisse cependant affirmer que le *Nouveau cours de chymie* n’a pour base que son *Cours de chymie*. Comme l’ont fait tous les chimistes pendant le XVII<sup>e</sup> siècle, les auteurs anonymes reprennent des recettes qu’ils amplifient et intègrent dans la nouvelle doctrine à laquelle ils se réfèrent.

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## Homberg's Chemistry: a Certain Truth into a Disputable Physics<sup>1</sup>

*Luc Peterschmitt; Rémi Franckowiak\**

In 1702, Homberg began publication of his *Essays of Chemistry*, which were supposed to be the first part of a whole book on chemistry. He began the first essay by giving the definition of chemistry as: "Chemistry is the art of reducing bodies into their principles by the fire and of composing new bodies by blending different matters".<sup>2</sup>

Such a definition may appear paradoxical: why did Homberg give, three years after the renewal of the Académie Royale des Sciences, such a classical definition of chemistry?<sup>3</sup> Indeed, he clearly recalled the Paracelsian spagyry, the art to join and separate. In fact, this definition was more important than it would have been, if it were only a way to recall a tradition. This definition has to be understood in the context of the end of the 17<sup>th</sup> century and the debates about the rules of scientific knowledge. This definition was a way for Homberg to achieve a double task:

- a) He intended to give to chemistry a theoretical background: it was not only an experimental physics (role to which Fontenelle for example seemed to confine it), in order to make of the mechanical principles (motion, figure, size of particles) but the *explanans* of real scientific explanations;
- b) Homberg inverted the hierarchy between mechanics and chemistry: the latter only was certain, whereas the former was hypothetical, or, to use his own words, was not unquestionable.

This double task was performed by a subtle use of the principles, which articulates the mechanical principles of physics and the chemical principles. Homberg wanted to show that chemistry was the certain part of a disputable physics: it will be shown that there was no paradox here.

First, it is necessary to examine Homberg's commentary on his definition of chemistry. This commentary does not mainly bear on the notion of chemistry as an art,

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even if it was important as will be shown, but it bears on the notion of principle. Homberg distinguished two sorts of principles. In the most general sense, the principles of chemistry were its foundations: these principles were the arrangement, motion and size of the “first matters” –it is important to note the plural here: it is not question of the universal matter of the Cartesian mechanism. However Homberg refused to discuss these principles: it would be, he says, “too long”, because, “we do not have success in establishing nothing which is not incontestable”. The principles of the second sort are the chemical principles: they are “more material and sensible”, and they set up the certain knowledge which was sought.

At the end of this quick commentary, the inversion was achieved: physics was declared to be only hypothetical (as if everybody would easily agree), and its explanations will be, only likely. This did not mean that physics was false. This would be a contradiction: if physics was false, it would not be possible to say that a part of it was true. Moreover, Homberg recurrently declares that he is a physicist. But each time he says this in a peculiar context, it was a way to underline a way of reasoning:

“This reasoning, even if it seems likely, does not prove anything, unless it is sustained by well observed facts, because in physics we are so little clear-sighted that we are often mistaken, even when we believe to possess good facts and reasons”.<sup>4</sup>

To stress the limits of physics was a way to call on new or at least other experiments – which was the task of chemistry. This means that, in short, it was not possible to be a good physicist without being a good chemist. But if chemistry was an art, as Homberg recalled in his definition, then one has to admit that it was not reducible to physics. It was not only that experiments were necessary. In this case the possibility that chemistry would be only an experimental physics would remain. In fact, chemistry had its own principles because it was an art of experimenting: its principles have to allow it to discover and expound facts:

“As chemical physics, which consists only in experiments and exposition of facts seeks only certain truth, it has established this second sort of principles, more material and sensible, by men of which it pretends to explain easily and in its way its own operations and to know thereby more distinctively the bodies it examines in its analyses”.<sup>5</sup>

The principles of chemistry have a certainty of which the principles of physics are deprived: they are supposed to derive from experiment. However, what was the point to say that chemistry was a part of physics? Sure, it was not possible to pro-

duce a true chemistry in a false physics; but how was it possible to pretend to have a certain chemistry inside a disputable physics? This is even more paradoxical when we remember that, during the 17<sup>th</sup> and 18<sup>th</sup> centuries, to adopt a mechanist position often leads, if not always, to dismiss chemistry as a real science.<sup>6</sup> In order to understand the sense of the reference to the physical principles, it is necessary to examine more closely Homberg's discourse about the chemical principles: Sulphur, Salt and Mercury.

The first principle was Sulphur, or the matter of light: to Homberg, it was the only principle that was really active: "I call Sulphur the active principle, because it acts alone and makes the others act".<sup>7</sup> Let examine how Homberg interpreted this activity:

"To make this opinion intelligible and likely, it is necessary to conceive first that the matter of light is always acting, which seems to me to be a necessary attribute of the active principle. Secondly, that this matter can get by itself into the other principles, change their figure, increase their weight and volume, and join them differently to produce all the mixed bodies which can be sensed, which the property we give to our Sulphur principle".<sup>8</sup>

At a first glance, Homberg interpreted the activity of Sulphur mechanistically. This meant mainly that Sulphur was always moving, and modified the composition of bodies according to its motions. However, this motion seems to have no cause other than itself, at least this question of the origin of this continuous motion was not a chemical question (Homberg just suggests that its cause was divine). However, this motion was only a "necessary property of the active principle":<sup>9</sup> it was not sufficient to understand the activity of Sulphur. Indeed, it did not explain how the Sulphur can join the other principles and so on. To explain that Sulphur can modify the chemical nature of the other principles, Homberg endowed it with a non-mechanical property: the "natural gluten" (which describes this property of gluing). The fact that this position seems to originate in the beginning of the 18th century renders it even more significant: the mechanical principles cannot explain by themselves the activity of Sulphur. Sulphur was a plain chemical principle, and not only a name for a body that can be accounted for in mechanical terms.

The second principle was Salt.<sup>10</sup> As pure Salt, it was invisible, seen only when it was joined with another matter. More than of Salt, one should speak of salts, in the plural, which were only what was knowable with certainty. However, Homberg supposed, from 1708, that pure Salt was a sharp point – and this figure accounted for the way in which salts act. However, the Salts were differentiated

from each other by the sulphur, which was always joined to them. Sulphur makes of an imperceptible point, an empirical chemical salt, which can be subjected to experiments: "The difference between salts consists only in the different sulphurs which accompany them".<sup>11</sup> Moreover, Sulphur was what makes the salts act:

"All the actions of acids on alkalis and alike are only performed because they are pushed the one into the other by the matter of light, of which I have proved elsewhere that it was always moving, that it hits the solid parts of all bodies, that is to say that it pushes them continually".<sup>12</sup>

Sulphur moves the salts, and their motion was determined by the association between the pure Salt and the peculiar sulphur that was joined with it. Sulphur makes of Salt a chemical object, of which Homberg gave a physical account.

The last principle with which Homberg dealt was Mercury. It was said to be a principle only because of the failure of its chemical analysis: the principle was confused with common quicksilver. However, to Homberg, it was a principle of metals. The memoirs he devoted to Quicksilver illustrated this position, by proving the composition of gold as Sulphur and Quicksilver. Basically, Homberg pretended to have produced gold by introducing light (that was to say Sulphur) into a prepared quicksilver.<sup>13</sup> What is of interest was the way in which Homberg explained his experiment. The action of the matter of light consists in changing the nature of the quicksilver: it modifies its form, or its figure, or its figure, weight, volume.<sup>14</sup> Once again, the explanation was mechanistic: the matter of light destroyed the spherical particles of quicksilver, so that it became a new metal. Nevertheless, Homberg thought that his explanation was true:

"All what we have said of the destruction of gold and silver being true, that is to say that the important quantity of solar rays coming from the burning glass drove out the matter of light which stopped before in the little channels of the balls of quicksilver".<sup>15</sup>

This reasoning or analysis was a model to understand the production of all sulphurous matters. Homberg constantly called on mechanism. In addition, one may ask, what does remain to chemistry as a part of physics? If chemistry was a part of physics, it was to be understood that it was a sort, or a kind, of physics. The specific difference is well illustrated by the "natural gluten" of the Sulphur, as a non-mechanic property, and certainly, "natural" also means essential. With this property, Sulphur links physics and chemistry. This was shown by Homberg's reflections on Salt and Quicksilver: chemistry deals with facts, physics tries to account for them, when no chemical discourse was possible, it was not possible to explain chemically the principles of chemistry, even if the physical explanation

was not absolutely complete. At least, all this means that the chemical bodies were subject to the physical laws.

It is now possible to understand the signification of Homberg's so called mechanism: why did he call on mechanism and the principle of physics, and declare at the same time that they were at best, likely only? First, it is necessary to note that Homberg was not a mechanist, as the Cartesians had been; and he was neither a chemist who tried to elaborate a solution of "compromise", as Marco Beretta put it,<sup>16</sup> between chemistry and mechanism. However, the mechanical principles gave an interpretive scheme for chemical phenomena, or at least for some of them. This scheme was probable; as already said, this does not mean that it is false. Such principles may be true, but there is no certainty about any mechanical explanation. That is why they do not constitute a veritable science. However, the question remains: why did Homberg not content himself with the certainty of facts and exposition of facts?

A first evident answer, is that he needed explanations to produce a science. Moreover, dealing with principles lead him to a logical problem: it was not possible to explain chemical principles with chemical principles, of course. It is a signification of the mechanical scheme used by Homberg. More generally, it is to be noted that if experiments provide certainty, they are not intelligible immediately or by themselves, that to say that facts are to be explained. The problem is that the action of the principles cannot be observed directly: we can only perceive the effects of their action. This entails that, if the experiment is certain, the detail of what happens during the operation is not that clear (it could be said, it is completely unclear, after all, nobody is in the retort to see what happens here). Using mechanical principles makes of this unperceivable detail of experiment an object of thought, mechanism gives a rigorous conceptual representation of the operation. The experimented fact receives a kind of additional authentication, if it can be explained by the mechanical principles, then it is compatible with the general laws of physics. This gives reasons to think that, for example, it is not an experimental illusion. Finally, it is not impossible to produce gold, even if it seems to be quite incredible. To say it in another way, the physical or mechanical explanation does not account for the fact itself, but it accounts for its possibility. To do that, a probable, or a likely explanation is enough. However, it does not entail that the mechanical principle can explain the chemical fact. In this sense, Homberg is not a mechanist, and in spite of appearances, his chemistry was not mechanical, because it cannot be reduced to physics:

- a) Chemistry is certain, whereas the physical explanations are probable: they do not produce knowledge. In addition, if physical explanations bear on the pos-



sibility of facts, it means that it is chemistry, which decides of the way of using these mechanical principles.

- b) The mechanical interpretive scheme concerns mainly the principles. However, it does not mean that chemistry is not a causal knowledge, just as physics is: the presence of a principle in a mixed body is an explanation of the properties of that body.

Using the mechanical principle gives an additional intelligibility. Nevertheless, it does not add any certainty. That is why, according to Homberg, chemistry was the real science of bodies.

## Notes

<sup>1</sup> This paper is a shortened version of: “La chimie de Homberg: une chimie certaine dans une physique contestable”, *Early Science and Medicine*, 10, 1 (2005), 65-90. The rewriting is mine (Luc Peterschmitt).

<sup>2</sup> “J’appelle Chimie l’art de reduire les corps composez en leurs principes par le moyen du feu, & de composez des nouveaux corps dans le feu par le mélange de differentes matieres” (“Essays de Chimie”, *Mémoires de l’Académie royale des Sciences* (1702) : 33-52, on 33 – all translations from French into English are mine).

<sup>3</sup> F.L. Holmes rightly insisted on the importance of this definition: Homberg gives the general conceptual framework in which other chemists as Geoffroy or Louis Lémery will work – see. F. L. Holmes, “The Communal Context for Etienne-François Geoffroy’s ‘Table des rapports’”, *Science in Context*, 9, 3 (1996), 289-311, on 292.

<sup>4</sup> “Ce raisonnement, quoiqu’il paraisse vraisemblable, ne prouve cependant rien, à moins qu’il ne soit soutenu par des faits bien observés, car en matière de Physique nous sommes si peu clair-voyans, que souvent nous nous trompons même quand nous croyons être bien munis de faits & de raisons” (“Observations sur l’Acide qui se trouve dans le Sang & dans les autres parties des Animaux”, *Mémoires de l’Académie royale des Sciences* (1712): 8-15 on 10).

<sup>5</sup> “Comme la Physique Chimique, qui ne consiste qu’en experience & exposition de faits, ne cherche que la verité certaine, elle a établi cette seconde sorte de principes plus materiels & plus sensibles, par le moyen desquels elle prétend expliquer aisément & à sa maniere ses propres operations, & connoître parlà plus distinctement les corps qu’elle examine par ses analyses” (“Essays de Chimie”, 33)

<sup>6</sup> See Bernard Joly, *Descartes et la chimie* (forthcoming), and Luc Peterschmitt, “The ‘Cartesians’ and Chemistry: Cordemoy, Rohault, Régis”, in, *Chymists and Chymistry, Studies in the History of Alchemy and Early Modern Chemistry*, ed. Lawrence Principe (Philadelphia: Science History Publications/Chemical Heritage Foundation, 2007): 193-202.

<sup>7</sup> “Nous appelons le souffre, principe actif, parcequ’il agit seul & qu’il fait agir les autres. » (“Essays de chimie”, 34).

<sup>8</sup> “Pour rendre cette opinion intelligible & vrai semblable, il faut que je fasse concevoir premiere-ment que la matiere de la lumière est toujours agissante, ce qui me paroît un attribut inséparable du principe actif. En second lieu que cette matiere se peut introduire dans les autres principes; les changer de figure, les augmenter de poids & de volume, & les joindre differemment ensemble pour en produire tous les mixtes qui nous tombent sous les sens, ce qui est le caractere

que nous donnons à notre Souphre principe" ("Suite des Essais de chimie, Article Troisième, Du Soufre principe", *Mémoires de l'Académie royale des Sciences* (1705) : 88-99 on 88-89).

<sup>9</sup> "Un attribut nécessaire du principe actif" ("Suite des Essais de chimie, Article Troisième, Du Soufre principe", 92).

<sup>10</sup> About salt in general in the chemistry of the 17th and 18th centuries, see R. Franckowiak, *Le développement des théories du sel dans la chimie française de la fin du XVIe à celle du XVIIIe siècle*, Thèse de doctorat, Université Lille 3-Charles De Gaulle, 2002, especially pp. 329-375.

<sup>11</sup> "Les sels reçoivent avec beaucoup d'avidité les souffres, mais c'est sans les changer de nature, en quoy leur transposition est differente de celle dont nous venons de parler, c'est-à-dire qu'un souffre animal, par exemple, transplanté dans une matiere saline n'est pas changé en un souffre bitumeux ou autre, il demeure le même, mais il caracterise le sel auquel il se joint; & comme les souffres volatils changent aisément de nature, si par quelque accident le souffre, par exemple, qui aura caractérisé le sel commun, se peut changer en celui qui caracterise le salpêtre, le sel commun deviendra salpêtre, & ainsi des autres; ensorte que la difference des sels ne consiste que dans les differens souffres qui les accompagnent" ("Suite de l'article trois des Essais de chimie", *Mémoires de l'Académie royale des Sciences* (1706): 260-272 on 270).

<sup>12</sup> "Que toutes les actions des acides sur les alcalis & semblables, ne se font que parce qu'ils sont poussez les uns dans les autres par la matiere de la lumiere, que j'ai prouvé ailleurs être toujours en mouvement, heurter contre les parties solides de tous les corps, c'est-à-dire les pousser continuellement" ("Observations touchant l'effet de certains Acides sur les Alcalis volatils", *Mémoires de l'Académie royale des Sciences* (1709): 354-363 on 357).

<sup>13</sup> For a detailed account, see. L. Principe, "Wilhelm Homberg: Chymical Corpuscularianism and Chrysopoeia in the Early Eighteenth Century", in *Late Medieval and early modern Corpuscular Matter Theories*, ed. C. Lüthy, J.E. Murdoch, William.R. Newman (Leiden: Brill, 2001), 535-556, and his "Wilhelm Homberg et la chimie de la lumière", *Methodos*, 8, (2008), <http://methodos.revues.org/document1223.html>.

<sup>14</sup> "Premierement qu'il s'est introduit quelque chose dans ce mercure, puisqu'il est devenu plus pesant: secondement que ce qui s'y est introduit *l'a changé de nature*, puisqu'il ne coule plus, & qu'il devient en partie malléable: troisièmement que ce qui s'y est introduit s'unit parfaitement au mercure; [...] par la precedente operation j'aye prouvé que la matiere de la lumiere qui s'est engagée dans le mercure y est restée inséparablement, même au grand feu, & qu'elle a *changé la forme* du mercure en celle du métal malléable et ductile; [...] Estant donc persuadé que la matiere de la lumiere est la seule qui peut penetrer très-librement tous les corps poreux, & qui est la seule qui agit toujours, comme nous l'avons montré dans la première partie de cet article; & que cette matiere est capable de s'introduire dans tous les autres corps, de s'y arrêter & de les changer par-là de figure, de poids & de volume, nous avons crû que nulle autre matiere ne pouvoit être nôtre Souphre principe & nôtre seul principe actif, que la matiere de la lumiere" ("Suite des Essais de Chimie. Article troisième. Du Souphre principe", respectively 93, 95, 96 – my emphasis).

<sup>15</sup> "Tout ce que nous venons de dire de la destruction de l'or et de l'argent étant vrai, c'est-à-dire, que la grande quantité de raïons du soleil qui partent du verre ardent chassent la matiere de la lumiere qui s'étoit arrêtée dans les petits pertuis des boules du Mercure" ("Suite des Essais de Chimie. Art. IV. du Mercure", *Mémoires de l'Académie royale des Sciences* (1709): 106-117 on 109).

<sup>16</sup> M. Beretta, *The Enlightenment of Matter, the Definition of Chemistry from Agricola to Lavoisier*, Canton: Science History Publications, 1993, 122.

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## 2. CHEMISTRY COURSES AND THE CONSTRUCTION OF CHEMISTRY, 1750-1820

*Introduction by Bernadette Bensaude-Vincent;  
Christine Lehman; John Perkins*

Between about 1750 and 1820 chemistry underwent a series of profound changes. It became an autonomous science; in particular, independent from medicine, it became hugely popular and was for many the enlightenment science par excellence. The conceptual changes of the Chemical Revolution coincided with the increasing engagement of chemistry and its practitioners in industrialisation and as agents of technological change. Its practitioners were often engaged in close and problematic relationships with politics and the state. The process of the professionalisation and institutionalisation of chemistry in general and of chemical research in particular began to accelerate. The chemical expert emerged and chemical expertise was deployed in a wide range of contexts from public health to legal disputes, from military technology to agricultural improvement.

### **What was the role of chemistry courses in these developments?**

Although chemistry is one of the few scientific disciplines whose educational component has attracted historical attention,<sup>1</sup> we lack a clear understanding of how chemical knowledge was spread and how experimental skills were transmitted. Yet, most of the time, the pedagogical component is perceived exclusively through textbooks. Apart from Rhoda Rappaport's classic articles on Guillaume-François Rouelle,<sup>2</sup> oral didactic practices have been largely ignored, for the obvious reasons of a lack of accessible sources. However, recent studies based on archival materials, suggest that Rouelle's demonstrations were but one example among a host of chemistry courses in parallel with the development of laboratories and, at the period, the emergence of research schools in chemistry.<sup>3</sup> However, much of this work has been in the form of separate case studies and the Louvain conference offered an opportunity to review this work and to bring it together within a comparative framework, and, at the same time, to address the wider role of chemistry courses in the creation of modern chemistry.

## Notes

<sup>1</sup> See for instance Hélène Metzger, *Les doctrines chimiques en France du début du XVII<sup>e</sup> siècle à la fin du XVIII<sup>e</sup> siècle*, (Paris: Les Presses Universitaires, 1923), Owen Hannaway, *The Chemist and the Word: The Didactic Origins of Chemistry* (Baltimore: Johns Hopkins University Press, 1975), Anders Lundgren and Bernadette Bensaude-Vincent, eds. *Communicating Chemistry: Textbooks and their Audiences, 1789-1939* (Canton, MA: Science History Publications, 2000). On the relative absence of pedagogy on the map of science see David Kaiser's introduction to the collective volume *Pedagogy and the Practice of Science, Historical and Contemporary Perspectives*, ed. David Kaiser (Cambridge, MA: MIT Press, 2005).

<sup>2</sup> Rhoda Rappaport, "G.-F. Rouelle: An eighteenth-Century Chemist and Teacher," *Chymia* 6, (1960) 68-101; "Rouelle and Stahl – The Phlogistic Revolution in France," *Chymia* 7, (1961): 73-102.

<sup>3</sup> Kathryn M. Olesko, "Tacit Knowledge and School Formation", *Osiris* 8 (1993): 16-29.

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## Chemistry Courses in France in the Mid-Eighteenth-Century: Tradition and Innovation<sup>1</sup>

Christine Lehman<sup>\*</sup>

Chemistry was fashionable in the Enlightenment. In Paris, in the middle of the 18<sup>th</sup> century, the supply of chemistry courses was extensive and the public had a choice between two types: public courses, free and open to all, for instance those at the *Jardin des Apothicaires* and at the *Jardin du Roi* and fee-paying particular courses, which took place in the private laboratory of an apothecary. The teachers were either an apothecary alone, or a duo of a medical doctor as professor and an apothecary as demonstrator, or in the case of the *Jardin des Apothicaires* a group of apothecaries.<sup>2</sup>

These courses addressed in priority physicians and apothecaries. However, in practice, their public was much wider, chemistry also attracted philosophers, craftsmen, bourgeois and... women of quality. In Paris, more than six hundred people attended Rouelle's lectures at the *Jardin du Roi* and a hundred at the *Jardin des Apothicaires*, without taking into account the many auditors of private courses: for example, more than fifty at the one given by Macquer and Baumé. In the provinces large numbers also attended courses, the amphitheatre of the Faculty of medicine of Montpellier could contain "up to four hundred students". Is it possible to explain such a success?

### What was the chemistry taught in France in the middle of the 18th century?

Was it a collection of empirical recipes? Was it a series of spectacular effects or a theoretical framework based on experimental evidence? Was it the entertainment aspect, as extolled by the course advertisements that attracted audiences? Indeed, chemistry courses were not free of theory.<sup>3</sup>

One cannot speak in general of the experiments proposed in a course as they were different in each type of course. They depended on the type of audience they were

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aimed at, on the course objectives and on the didactic choice adopted by each teacher. Rouelle's course at the *Jardin du Roi* was an experimental and applied chemistry course. In front of his extensive and heterogeneous Parisian public, Rouelle demonstrated the spectacular aspects and the applications of chemistry.<sup>4</sup> On the other hand, in Montpellier, Venel gave his private course in front of a smaller number of students, in the limited space of apothecary Montet's laboratory. He delivered knowledge of nature, and how to gain practical know-how.

### **It was an experimental chemistry: What were the functions of experiments?**

Three main functions of experiments can be seen. First, transmission of practical know-how, secondly, improvement of knowledge of nature and third, illustration of chemical theory.

Know-how transmission was the objective that guided the experimental demonstrations of chemistry courses. The teacher dealt with theory and the demonstrator recapitulated the same subject, detailing the operation, and teaching the way to carry it out and the risks encountered in the operation. The demonstrator was in charge of the discourse about detailed operations, and his description also included safety recommendations.

The need to be able to reproduce experiments explains the precision found in student's notes: reactive quantities, the heat of the fire, observations such as colour changes, gas releases etc. were detailed. One had to learn "to do". Venel therefore encouraged his students to manipulate, because, in order to master the operational techniques, they had to "educate themselves in the habit of executing them". Watching operations while listening to explanations was a way to obtain know-how, even though it was clearly accepted that only a long practice could really educate the "artist".

The main function of experiments was to improve students' knowledge, and each teacher developed the subjects he knew the best, reflecting either his work or his particular interests. Beyond the pedagogical purpose, operations not only aimed at showing the components of the analysed body, but also at validating the results of the analysis by recomposition starting from the same components. Chemistry courses contributed to the progress of knowledge by stabilising facts.

Whatever the public, the courses also included experiments meant to illustrate chemical theory, such as the theory of affinities, or to reveal the elements, i.e. Air,

Water, Earth and Phlogiston, which constituted natural substances. In his lectures, Macquer demonstrated the physical properties of air and its role in combustion. Rouelle and Venel proved that air was contained in oak wood and in animal substances such as blood. We do not know whether Rouelle or Venel performed experiments within their lectures on fire. But the list of the equipment used in Macquer's course at the *Jardin du Roi* does show that experiments were performed to illustrate the various properties of fire: either those of fire as an instrument and the way to produce it, or those showing the presence of the element Phlogiston. For example, the friction of two hard bodies such as a stone and a steel piece showed the Phlogiston contained in the latter, indicating that experiments were performed to illustrate a concept as theoretical as Phlogiston.

### It was a theoretical chemistry

Rouelle's and Venel's chemistry courses disseminated several different kinds of theory.

First, a "technical" theory, rarely mentioned. For example, the circulation process through the fire-place of the air necessary for combustion. Venel's theoretical explanation was based on physical properties of hot air. Second, a theory of matter organised in elements, mixt and aggregates, borrowed from Becher and Stahl. And lastly, an interpretative theory of operations by means of Phlogiston and affinities.

However the theory that framed the whole chemistry course was the affinity theory, based on the use of Geoffroy's table.<sup>5</sup> Geoffroy's table was present in the laboratory of the *Jardin des Apothicaires*. It has been found as a draft slipped between the pages of a student's course notes, or as a printed copy glued at the last page of another student's notes. This suggests that printed tables were on sale in bookshops. Venel's students, such as Balme, carefully copied it. Generally, it was Geoffroy's table that was selected as a model. Its omnipresence in manuscript course notes confirms its current use as a tool for interpreting chemical operations. Venel expressed the affinity relations between substances as follows: "So when a simple body applied to a compound body loads itself with one of its parts and precipitates the other, one says that it has more affinity with this dissolved part than it had with the precipitated part".<sup>6</sup>

In order to illustrate affinities, Venel selected the example of the various affinities of nitrous acid. Venel methodically demonstrated step by step the predominance of the affinity of the column head with any substance of the column, over

its affinity with the substance located immediately lower. Recourse to the affinity table was always present in Venel's chemistry course. The table was consulted to validate the preparation process of a substance (acid, alkali or salt), and the purification of metals. It was recurrently referred to in lectures on "*halotechnie*" and on the mineral kingdom.

The affinity table also made it possible to predict precipitations which were "of an immense use in chemistry". As a matter of fact it was customary to obtain substances by successive "precipitations", which could be in the form of either a solid deposit or a gas release.

However the affinity table was also often challenged and sometimes contradicted. For example, the preparation of the corrosive *sublimate* by the direct action of mercury on marine salt was surprising: "This is quite singular and gives a terrific "*soufflet*" to M<sup>r</sup> Geoffroy's affinity table as the affinity of acids is quite higher with the alkalis than with metallic substances".<sup>7</sup> Indeed this preparation contradicts the ranking of the first column. Marine salt (sodium chloride), which results from the union of an acid with a fixed alkali, should never be attacked by mercury located at the bottom.

These few examples taken from Venel's course reveal that, thanks to this table, a chemist changed his method; he did not work blindly but could anticipate and justify his experiments. In the 18<sup>th</sup> century, Geoffroy's table was essential in both the teaching and practice of chemistry. It constituted both the frame of the course and a guide for the experimental work. It was a conceptual tool, a theoretical instrument necessary for understanding chemical knowledge just like the glassware, vessels and furnaces used to perform experiments.

Affinities, based on attractive forces of Newtonian inspiration, challenge the view of Rouelle and Venel as "Stahlian chemists". More generally, the chemistry taught by the teachers Rouelle, Venel and Macquer was neither Stahlian nor Newtonian. It was an entirely new, paradigmatic chemistry, based on experiments and supported by the theory of affinities.

### **As a conclusion...**

Each course depended on its public: medical doctors and apothecaries who wanted to know chemistry so as to better prescribe and prepare drugs; philosophers who wanted to progress in their thinking on matter; landowners interested in the natural resources of their land; craftsmen who wanted to know about the chem-



istry of colours, glass, metallurgy... or, the curious, the strollers who wanted to attend the show, and... ladies of quality.

It is this convergence of interests, and a few great teachers' pedagogy and enthusiasm, which allows us to understand the infatuation with chemistry in the middle of the Enlightenment. It is this chemistry, taught and thus actually practised, this new chemistry, characteristic of the middle of the Enlightenment in France, that reformers of the end of the 18<sup>th</sup> century used as their starting point.

## Notes

<sup>1</sup> A more complete version is to be published together with the other contributions to the Special Session: Chemistry Courses and the Construction of Chemistry, 1750-1820, in a forthcoming special issue of *Ambix*.

<sup>2</sup> Bernadette Bensaude-Vincent, Christine Lehman, "Between Commerce and Philanthropy: Chemistry Courses in Eighteenth-Century France," in *Science & Spectacle in Enlightenment*, eds. Bernadette Bensaude-Vincent and Christine Blondel (London: Ashgate, forthcoming). Christine Lehman, *Gabriel-François Venel (1723-1775). Sa place dans la chimie française du XVIII<sup>e</sup> siècle* PhD Université Paris X-Nanterre, 2006 (Lille: ANRT, 2008).

<sup>3</sup> Bernadette Bensaude-Vincent, Christine Lehman, "Public Lectures in Mid-Eighteenth Century France," in *New Narratives in Eighteenth Century Chemistry*, ed. Lawrence M. Principe (Dordrecht: Springer, 2007), 77-96.

<sup>4</sup> Lissa Roberts, "Chemistry on stage: G.F. Rouelle and the theatricality of Eighteenth-Century Chemistry," in *Science & Spectacle in Enlightenment*, eds. Bernadette Bensaude-Vincent and Christine Blondel (London: Ashgate, forthcoming).

<sup>5</sup> Lissa Roberts, "Setting the Tables: The Disciplinary Development of Eighteenth-Century Chemistry as Read through the Changing Structure of its Table," *The Literary Structure of Scientific Argument. Historical Studies*, ed. Peter Dear (Philadelphia: University of Pennsylvania Press, 1991), 109-113; Ursula Klein and Wolfgang Lefèvre, *Materials in Eighteenth-Century Science. A Historical Ontology* (Cambridge MA: MIT Press, 2007).

<sup>6</sup> Wellcome Institute Library, London, Ms 4914 [Notebook by Balme]: «Cours de Chymie fait chez monsieur Montet apothicaire par monsieur Venel Docteur et professeur en L'université De medecine à Montpellier, 1761.» p.190.

<sup>7</sup> Wellcome Institute Library, London, Ms 4914, p. 149.

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## Chemistry Courses, the Parisian Chemical Community and the Chemical Revolution, 1770-1790

*John Perkins\**

During the latter part of the 18<sup>th</sup> century chemistry enjoyed growing popularity in Europe, in France, and, in particular, in Paris. This popularity was both reflected in, and stimulated by the growth of chemistry courses. In Paris, in 1784 for example, there were 7 courses in public institutions, the majority of them open to the public and free, most with audiences of several hundred. There were another seven courses given by private individuals, fee-paying, with smaller audiences ranging from 10 to 40. In addition there were 10 or so chemists offering courses to private individuals or groups of friends. Alongside these there were 6 courses solely on the subject of gases or elastic fluids, a subject central to chemistry in general and to Lavoisier's anti-phlogiston chemistry in particular, as well as 23 experimental physics courses which included not only gases and their chemistry, but also, in a number of cases, quite substantial amounts of general chemistry.

Some of these are well-known, but their full extent, their growth, their roles in the development of the chemical community in Paris, in the developing infrastructure of chemical research, and in the Chemical Revolution, have not been fully appreciated. This paper aims to address these issues.

### Patterns of development

The development of these courses is summarised in two charts.

1. Chemistry Courses – Professors
2. Chemistry Courses – Number

These charts reveal some significant patterns. The first and most striking is a clear transformation around 1775/76. On the one hand the number of courses in public institutions increased sharply. On the other, there was a relative, if not absolute, decline in the number of private courses. The long process of the institutionalisation of chemical education in France changes at this point, and it did

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so well before the creations and reforms of higher education during the Revolution and Empire Periods.

Second, a number of long-running private courses, the majority taught by apothecaries, came to an end at about the same time, and a new generation of chemical practitioners entered the market. But few of this new generation were apothecaries. The end of the dominance by apothecaries of the market for courses suggests a significant change in their position within the Parisian chemical world. And again, it is one that pre-dated the chemical and political revolutions.

Third, before the mid 1770's the majority of those teaching chemistry, in institutions and privately, continued to be medical practitioners, either as apothecaries or physicians. But after the mid seventies, the majority of those giving courses were more loosely attached to any form of medical practice and its institutions.

Fourth, the general audience for chemistry was growing rapidly and becoming more diverse, in its interest in the subject, its expectations from courses, and in their prior knowledge. This expanding audience changed the nature of the cultural market in which those offering courses had to operate. It had always been competitive, it became much more so. Those offering courses used a variety of ways to make their courses more attractive: a more forceful use of advertising; competitive pricing; a stress on the novelty of their content; an emphasis on the broader utility and economic importance of chemical knowledge and practice.

Fifth, it does seem that in the 1780s more individuals were chancing their hand at chemical teaching, albeit often for a short time, reflecting increased demand, opportunity and competition, as well as the changing structure and dynamics of the chemical community.

The timing of these changes is important. They were not driven by the Chemical Revolution. On the contrary they provide a very important element, indeed it is suggested, the major element, of the context in which the Chemical Revolution took place.

### **The courses and the Parisian chemical world**

The chemical world of Paris in the 1770s and 1780s was large and growing. These courses were the most important structural feature of that world and the community of chemical practitioners at its core. Starting a successful course was for many the route into pursuing a career in chemistry. They were the places where reputations were made and could be exploited. They were also where the public

image of chemistry, and of the chemist, were constructed and disseminated. The courses, and the laboratories attached to them, particularly the smaller and more intimate spaces of private courses, were the site for the development of important networks of friendships, connections and patronage, not least between chemical practitioners and *personnes de distinction*, including the grandest of *les grands*. They were places where a less hierarchical scientific sociability could emerge. They were also where many of those who set up courses in provincial cities received their chemical instruction, learnt the tricks of the lecturing trade, returned for refresher courses and made the social and political connections that were essential to their future provincial success. In addition, several courses, notably those given by Sage and Darcet, played an important role in fostering the deployment of chemical expertise in industry.

Three other important roles they played, in chemical training, in research, and in the Chemical Revolution need emphasis.

### **Courses and chemical training**

One small part of the audience for these courses was made up of those who wished to go on to practice chemistry. Doing so required a number of things: time; money; practical as well as theoretical instruction over an extended period; access to a laboratory and to practical experience in doing chemical operations under the tutelage of an experienced chemist; discussion and debate; guidance in defining research problems; access to a chemical library; access to networks of connection and patronage; and to the public arenas where reputations were made. These requirements were by no means easy to come by. The letters of the Genevan Henri Gosse<sup>1</sup> who came to Paris in January 1779 to continue his training as an apothecary and to pursue his chemical interests show how one person, with not inconsiderable financial support, negotiated to obtain these resources. Over a period of 2½ years he took 7 public and 4 private chemistry courses. He also spent six months in the laboratory of the apothecary Antoine Quinquet and six months in Fourcroy's private laboratory, both for no fee, in return assisting Quinquet in his pharmaceutical preparations and acting as unpaid preparateur for Fourcroy's private courses and as an assistant in his research. Gosse's correspondence provides the most complete contemporary account of chemical training, but a range of other sources show that his was becoming the normal route into chemical research.

## Courses and chemical research

With the exception of Lavoisier, we know little about the laboratory and its social environment as the site of chemical research at the time of the chemical revolution. In Paris in the 1770s and 1780s there was a wide range of laboratories where chemical research was practised. In several of these, especially in the laboratories that supported courses one can see the emergence of the same sort of organisation for research that Lavoisier developed at the Arsenal: the established figure surrounded by a small number of younger collaborators, some there for sustained periods of time, who later were to go on to their own research careers; a mobile population of colleagues, friends and visitors; and the supporting cast of *préparateurs* and assistants. In this respect Lavoisier was not the unique figure that some historians have suggested, the harbinger of a more ‘professional’ organisation of research.

## Courses and the Chemical Revolution

Both a lot and surprisingly little is known about the Chemical Revolution in Paris. Historians have focussed almost exclusively on the Académie and on the campaign carried out by Lavoisier and his group at the Arsenal. The dominant interpretation of the reception of Lavoisier’s chemistry is still that elaborated by Carleton Perrin in 1981<sup>2</sup>—a process of active transmission of the new ideas from the small central group via the *Nomenclature*<sup>3</sup> and then the *Annales de Chimie*, and, apart from resistance by a group around De La Métherie and some in the Académie, their passive reception by the rest of the French chemical community, the majority of whom watched the campaign in silence, and were unenthusiastically converted, indifferent or reluctant to publicly express opposition as the juggernaut rolled on.

However, the number and popularity of chemistry courses indicate a larger Parisian chemical community than the standard model suggests. In addition, it was not passive towards the new chemistry, on the contrary many of its members were actively and critically engaged with it. The courses provide important evidence for this. Between 1780 and 1790 Lavoisier’s views were given widespread and increasingly supportive dissemination through most of the chemistry courses, in particular those given not only by Fourcroy, but by Antoine Louis Brongniart, Gengembre, Deyeux, Deleymerie, Deparcieux, Ribaucourt, Prudhomme, Alexandre Brongniart, Reynard, Vauquelin and Alyon, as well as through many of the experimental physics courses given Sigaud de la Fond,

Brisson, Rouland, Charles and Lefèvre-Gineau. The large-scale water synthesis experiment carried out at the Collège de France by Lefèvre-Gineau which had such a powerful public impact was repeated on a number of occasions in other courses.

Few lecture notes have survived from this period. But we do have an incomplete set of notes taken by the physician Antoine Hardy when he attended Fourcroy's short course of 12 lectures on gases in April 1786. Hardy was from Rouen where he had given a chemistry course in 1781 and 82. He was thinking of restarting it and was taking Fourcroy's course to learn about the latest developments. Hardy's notes<sup>4</sup> show that Fourcroy's presentation of pneumatic chemistry gave preference to Lavoisier's views—as one would expect—but they also record Hardy's own thoughtful engagement with both their strengths and weaknesses. It is suggested that this position, rather than the outright rejection or passive indifference that figure in the standard account of the reception of Lavoisier's chemistry, was more typical of those who were part of the Parisian chemical world in which the courses given by Fourcroy and many others played such a prominent role.

## Conclusion

Studying these courses reveals a larger and more dynamic Parisian chemical community than historians have recognised up till now. It was a community undergoing major changes prior to both the chemical and the political revolutions, changes which provide the context for a broader understanding of both the chemical revolution and the contested history of chemistry during the Revolution and Empire. And they indicate that in order to do so it is necessary to look beyond the limited confines of Lavoisier and the Académie des Sciences.

## Notes

<sup>1</sup> Geneva, Bibliothèque Publique et Universitaire, Ms fr 2615, 2617, 2628, 2633, 2643.

<sup>2</sup> "The triumph of the Antiphlogistans," in H. Woolf, ed., *The Analytic Spirit: essays in the history of science in honor of Henri Guerlac* (Ithaca, N.Y.: Cornell University Press, 1981), pp. 40-63.

<sup>3</sup> Guyton de Morveau, L.B., Lavoisier, A.L., Berthollet, C.L., and Fourcroy, A.F. de, *Méthode de Nomenclature Chimique*, Paris, Cuchet, 1787.

<sup>4</sup> Archives Départementales de la Seine-Maritime, 5E108.

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## Louis Jacques Thenard's Chemistry Courses at the *Collège de France*, 1804-1830

Antonio García Belmar\*

When the echoes of the Chemical Revolution were still in the air, a new radical transformation took place in a central aspect of chemistry: in the way in which it was taught. The emergence of new chemistry teaching methods in the 19<sup>th</sup> century has been depicted as an individual enterprise of a young chemist, Justus von Liebig, who conceived and set up a revolutionary method of laboratory based



Figure 1. Louis Jacques Thenard (1777-1857). Courtesy of Bibliothèque Interuniversitaire de Médecine, Paris, Banque des images (CIPB1034).

teaching, which is usually regarded as opposed to the traditional lecture and experimental demonstration.<sup>1</sup> Herein, this picture is questioned by focusing on two main issues: the alleged sudden emergence of new pedagogical methods (sometimes regarded as a revolutionary change) and the assumed sharp contrast between nineteenth-century laboratory-based teaching and eighteenth-century experimental demonstrations. The discussion relies on the study of the chemistry courses given by Louis Jacques Thenard (1777-1857) at the *Collège de France* during the first third of the 19th century.

First, how lecture demonstrations were previously worked out in the laboratories of the *Collège de France* will be examined. The preparation

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of experiments played a major role in the organization of spaces and manpower. In this activity, the boundaries between teaching and research were blurred.<sup>2</sup> By study of student notebooks, the role and main features of lecture demonstrations, the theoretical and practical knowledge transmitted through them and their position in the structure of Thenard's lessons can be examined.<sup>3</sup> Student notebooks unveil a wide range of experiments that hardly fit in with the usual picture of 'experimental demonstrations' in historical studies on natural philosophy and experimental physics. Thenard's courses, as probably were many others in early nineteenth-century Europe, were spaces of didactic investigation, in which different uses of experiments as didactic tools were tested.

### Teaching and research spaces

Since the late 1770s, when the new building of the Collège de France was constructed, spaces dedicated to chemistry were reduced to a single laboratory placed at the end of the left wing, ground floor. There, Jean Darcet (1724-1801) taught chemistry for more than 30 years. An important change took place when Nicolas Vauquelin (1763-1829) was appointed to the chair in 1800. The big amphitheatre of anatomy was transformed into an amphitheatre of chemistry, with the construction of a big furnace, a big table for demonstrations and a system of water and gas conduits. Thenard's appointment to the chemistry chair in 1804 reinforced this trend and consolidated the standing of chemistry teaching in the *Collège de France*. In barely ten years Thenard completed an entire renovation, which extended the facilities for chemistry to include nearly the whole ground floor and part of the first floor of the building's left wing. The amphitheatre became the centre of a network of interconnected spaces with more defined uses. Directly accessible from the main court, the amphitheatre became the stage where lectures were given. A door behind the stage provided access to the '*atelier de chimie*', the work area where Thenard and his assistants designed and rehearsed experiments until they were ready to be performed in the amphitheatre. Separated from this workshop, and with an independent access to the amphitheatre, the storage room was the place where chemical products and instruments could be stored and protected from the corrosive gases produced during experiments. At the far end of the left wing, Darcet's old chemical laboratory, completely renewed, was reserved for the '*grandes opérations*' chimiques. Finally, an independent stair provided direct access from the ground floor to the laboratories of the first floor, reserved to Thenard and his collaborators.<sup>4</sup> According to the central protagonists, all these changes were required to cover the needs of the lec-



ture demonstrations. As said, teaching was the main activity of the chemistry chair and the one around which spaces evolved, took shape and definition.

The diversification of the use of spaces come along with important changes in the division of labour and staff. The preparation of experiments largely shaped the different occupations of laboratory workers. The '*chef des travaux chimiques*' was responsible of the coordination of the different activities carried out by *préparateurs* and *aide-préparateurs*, the former having the responsibility of assisting Thenard in the performance of experiments during lessons. For Thenard's assistants the design, rehearsal and execution of didactic experiments became a way of being trained in the handling of substances, instruments and operations characteristic of a chemical laboratory. The experiments acquired thus a dual didactic function. Laboratory training proved to be very influential in the professional future of Thenard's assistants. They were recruited from among pharmacy apprentices, who had previous experience in laboratory work and had attended chemistry courses, very often those given by Thenard. For all of them, the pharmaceutical profession should have been the most likely of destinies, and yet almost none of them followed that career. After their stay at the *Collège de France*, these pharmacy apprentices were converted into '*jeunes chimistes*', as Thénard referred to them. They developed careers that combined activities tied to research, industry and the teaching of chemistry, and many of them later took up eminent positions in French academic institutions. Apart from these tasks, another activity was also important in terms of understanding the role of these laboratories. The first scientific publications of many of Thenard's assistants were published during their stay at the *Collège de France*, or in the subsequent years. Some of them were a result of collaboration, very often between laboratory heads and their *aide-préparateurs*. In many cases, lecture demonstrations were the origin of some of these research collaborations.<sup>5</sup>

## Demonstrating chemical facts

Following a well established learning tradition, Thenard chemistry lectures were written down by several of his students in notebooks, "*cahiers de cours*". Many of these documents are still conserved in French archives.<sup>6</sup> Thenard's students notebooks show that his lectures were intended to offer a general introduction to chemistry with the main focus on the study of the properties, extraction methods and uses of a range of "chemical species".<sup>7</sup> For each substance, Thenard offered three different levels of information: description, verification and explanation. First, he offered a description of all the relevant facts about a substance ("*le exposé*

[de] tous les faits qui sont connu”). He followed a constant expository scheme beginning with the known, the substance in its natural state, and proceeded through observation and techniques of analysis and synthesis towards the unknown, that is, the pure substance, which was identified, described and classified according to its physical properties and its chemical behaviour. This sequence conveyed an ideal method for experimental work. Besides the description of the known facts, Thenard offered two other levels of information, about the chemical operations that *verified* (“*constater*”) the previously described facts, and their *explanation* (“*une explication des faits*”).

“*Constater les faits*”, that is to say, to verify chemical and physical properties of substances, meant to reproduce them experimentally. Thenard offered meticulous descriptions of instruments, practical directions and advice for their correct use, warnings in order to avoid accidents and conduct accurate experiments, and also suggestions as to implement and improve experimental designs. Thenard employed simple methods such as physical division, dissolution, filtering or decanting, as well as more complex techniques such as distillation. In all the cases, he employed relatively simple and versatile utensils. Among the dozens of experiments described in Thenard’s student notebooks just a few groups of objects are mentioned, such as “instruments”: thermometers, eudiometers, calorimeters and a few other examples. The other objects were apparatus (“*appareils*”) made of all kinds of glass vessels and connecting tubes, ceramic dishes, furnaces, gas collectors, etc., the traditional items present in an apothecary workshop.

After the “description” and “verification” Thenard proceeded to the “explanation” of the phenomena observed in the demonstrations. To offer the “*théorie des faits*” (theory of facts) meant to explain the changes that substances undergone in their composition during the reaction. To this end, Thenard described the composition of reagents and products, and, then, he compared them and deduced the recombination process. The “*théorie des faits*” offered a plausible explanation of *what* happened in the chemical reaction and *why* it took place under certain experimental conditions. Thenard’s explanations mostly relied on the theory of affinities, a topic to which students were introduced from the first lessons. The “*théorie des faits*” introduced an abstracted ingredient in the lectures that greatly differed from the descriptive character of many other parts of the lecture demonstrations. The listeners were abruptly transported from the observation and description of substances, apparatus and operations to their interpretation in terms of forces and particles which were quantified, calculated and represented on a blackboard by means of algorithms and charts.

During this part of the lectures, many demonstrations were presented in a very “illustrative” vein. They served to *prove* (“*prouver*”) that the “*théorie*” was the “*expression des faits*”. In these cases, experiments played a subsidiary role with regard to the theoretical interpretations that preceded and predicted their outcome. Nonetheless, in other cases, the role of experiment was completely different. That was the case when, for instance, controversies surrounded an interpretation, or when it was accepted that there was not available data for substantiating an explanation. When dealing with the action of caloric, one of the students wrote “in the present state of science, no one can completely decide which of the two theories, is the best”.<sup>8</sup> Experimental demonstrations could also show phenomena for which there were no plausible explanations. For example, after meticulously describing the procedure in which carbon was made to react with nitrogen, Thenard concluded that “so far we do not know (“*on ignore*”) what happens in this experiment”.<sup>9</sup>

In other lectures, demonstrations were transformed into crucial experiments between two rival theories. Take the case of potash and soda. Thenard and Gay-Lussac did not accept the conclusions of the analyses performed by Humphrey Davy who regarded potash and soda as oxides of two new metallic elements. In the midst of the controversy, Thenard described the two alkalis substances in several lectures given by the middle of February 1809. It is worth noting that Thenard employed in his lectures a course of reasoning as well as experiments that resembled those included in the report he presented (with Gay-Lussac) to the *Institut de France* some months before.<sup>10</sup>

The uses of experiment in Thenard's lectures sharply contrast with the stereotyped picture of traditional demonstration lectures. From the conception and production phases, lecture demonstrations could play a broad range of didactic and heuristic roles. Experiments were designed to support Thenard's lectures but they also contributed to the scientific training of his *préparateurs*; and sometimes they turned out to be a starting point for a relevant research that was published in one of the main scientific journals of the time. This multifaceted nature of the experiments in their production phase persisted when they were performed in the amphitheatre. They were used to “make sensitive” (“*rendre sensible*”) and “make more tangible” (“*rendre plus palpable*”) the physical and chemical properties that enabled the chemical species under study to be identified and classified. The experiments also served to show instruments, apparatus and chemical operations at work. Showing chemical apparatus and utensils, explaining their use through diagrams and drawings, describing in detail how to handle them in order to carry out the most common operations in the chemical laboratory, understanding the

physical aspects and behaviour of chemical substances, their uses and dangers, all constituted pedagogic goals of their own, and could be separated from the explanations and interpretations that could be derived from the chemical phenomena being demonstrated.

Finally, besides showing phenomena and instruments, it was equally important to know how to produce the former by means of the latter. According to Thenard's arguments, the ultimate purpose of such detailed descriptions of the instruments, devices and operations was to enable the students to repeat these experiments by themselves, as this was considered the most effective way of learning a science like chemistry. Reproducing and learning to reproduce the facts being "*exposés*", appeared as inseparable aspects of their verification.

### Some conclusions and new questions

One could claim that the *Collège de France* was an exceptional case. According to the foundation documents and statutes, the *Collège de France* was an institution of higher and specialised education meant to train people with previous university or professional backgrounds, and, therefore, hardly representative of the chemistry courses of its time.<sup>11</sup> However, the class registers still conserved at the Archives of the Collège de France offer a different picture.<sup>12</sup> They show that Thenard's audience was mostly made of medical students and pharmacy apprentices. For this public, Thenard and other chemistry professors offered an ordered, systematic and comprehensive introductory course on chemistry, assuming that their audience had never studied this science. Chemistry played a minor role in French secondary school curricula during most of the first part of nineteenth-century.<sup>13</sup> As a result, medical students became one of the most important audiences for the public and private courses on chemistry offered in Paris during that period. A guide for medical students, written by Professor J. P. Maygrier, remarked that "chemistry cannot be learnt in books but in the lectures, where the experiments, which are often repeated and skilfully performed, speak to the eyes rather than to the mind". According to Maygrier, these courses would permit "students who are not initiated in this beautiful science yet" to acquire the necessary knowledge to follow the courses at the Faculty.<sup>14</sup> Pharmacy students registered in Thenard's chemistry lectures obtained their title of "*maître en pharmacie*" after further studies. Aspirants to the title of pharmacist could choose between a long period of apprenticeship with a pharmacist or by three years of academic education in a school of pharmacy followed by a shorter period of apprenticeship. The first group, the most numerous one during the first half of nineteenth-century,

received the traditional apothecary training, whereas the second one mirrored the new idea of professional pharmacists, whose practice was founded on the scientific knowledge provided by chemistry and natural history.<sup>15</sup> In the case of Thenard's students, most of them were members of the second group<sup>16</sup>. Thenard's courses at the *Collège de France*, as well as those given at the *Muséum d'histoire naturelle* or in the large number of private courses mentioned by Maygrier in his guide, offered introductory courses on general chemistry. These chemistry lectures met a demand emerged after the important changes in early nineteenth-century French medical and pharmaceutical education. They were mostly attended by students looking for a basic chemistry training needed to pass the academic and professional examinations.<sup>17</sup>

Most of Thenard's foreign students had also a medical or pharmaceutical background. For them, public chemistry courses given at the Collège de France offered the chance to acquire in a few months a general view of chemistry. In addition and as often happened, it was possible for them to follow similar courses given by other professors, and thus know different ways of organizing and teaching chemistry. Both circumstances proved to be of special interest to foreign students who wanted to acquire, in a short period, the training needed to carry out activities related to the teaching of chemistry in their countries of origin.<sup>18</sup>

Thenard's ideas about the didactic role of chemical manipulations were far from being rare inside the contemporary chemical community. Many early nineteenth-century French chemistry textbook authors defended similar views that shaped the type of experiments included in Thenard's books, the way of describing them and, of course, the instruments and apparatus to be used. The meticulous descriptions of utensils, reagents, and operations resulted in a narrative style approaching that of cookbook recipes intended to be made, rather than an explanation conceived to be understood.<sup>19</sup> Second, arguments supporting practical learning were particularly well understood by those who — like Thenard, his assistants and a good part of his audience — had been trained as pharmacists, within a tradition in which practical learning through personal experience had been the norm for long time and remained so, despite the new training avenues opened up after the reforms pushed through during the Revolution.<sup>20</sup> Finally, many direct testimonies also confirm that these wishes and efforts by professors, authors and editors had the desired effect on the readers of their textbooks and the students on their courses. Mateu Orfila, one of Thenard's students in the course 1808-1809, recalled in his memoirs that, along with reading the principal chemical treatises and attending the lectures, nothing had been more useful in learning chemistry as the repetition of experiments described and shown in lectures and

textbooks.<sup>21</sup> Student notebooks also provide evidence of this kind of autonomous experimental activities. Among the numerous footnotes in which notebooks' authors commented, completed or criticised the contents of Thenard's lessons, there are some in which the opinions were based on experiments conducted by students themselves.<sup>22</sup>

An explanation of an exceptional situation and, even less, for antecedents or a precursor of a revolutionary achievement is not being sought. Most probably, exceptionality is just apparent and a consequence of certain aprioristic ideas, like examples such as the Collège de France oblige one to revisit. At least two explanations are suggested. First, the reconsideration of what "experimental demonstration" means in chemistry when employed as didactic tool. Studies on the form and roles of experimental demonstration in the teaching and popularization of science have mostly focused on experimental physics. However, in chemistry, phenomena, instruments and explanations are of a different nature and this may be one of the reasons why it is so difficult to extrapolate conclusions from one field to the other. The phenomena shown in Thenard's chemistry lessons do not have a value or meaning of their own. It is not about producing a vacuum or an electric fluid and showing the effects produced by these physical phenomena. In Thenard's lessons, phenomena were physical and chemical properties ("*propriétés*") that served to identify and classify chemical species. Experiments did not establish matters of fact. Facts only acquired their meaning in the context of a classificatory system that was built up lesson after lesson. As far as the "instruments" were concerned, as above mentioned, apart from those presented in the introductory lessons (thermometer, calorimeter, and eudiometer), there were no singular instruments shown, but rather a collection of very traditional receivers, tubes and furnaces combined in different ways according to the particular operation to be carried out. The secret was not in the working of the instrument but in the way it was built up and handled. Contrary to what has been stated for experimental physics demonstrations, the minute detail of the descriptions observed in the narratives of chemical experiments does not appear to have as its objective the creation in the reader's mind of an image of experiments and their results as obviating the need for replication, but on the contrary the necessary conditions for its replication.<sup>23</sup>

Second, the co-existence of different uses of experiments as didactic tools should be of no surprise in a period in which a crucial step took place in science teaching methods, and which had a special repercussion in the case of chemistry.<sup>24</sup> The experiment and the laboratory, considered as the main means and place for the acquisition of new knowledge about nature, became the instrument and space *par*

excellence for the teaching of such knowledge. The transformation of the laboratory, the place of individual and private research, into an open and public space dedicated to the transmission of knowledge implied a number of important, conceptual, social and cultural barriers to be overcome. It is difficult to imagine that such an important transformation took place spontaneously and in a single place. It is not a matter of looking for antecedents or influences, but of understanding what type of changes were taking place in terms of how the teaching of chemistry was understood and practiced in other European institutions during the first third of the nineteenth century and how these pedagogical ideas were transmitted and appropriated in different contexts. The *Collège de France* was probably one of a number of places where, during this period, forms of teaching that combined very different didactic models were tried out, mixing rhetorical strategies and ways of using the experiment that decades later would be unthinkable in a teaching space. Examples like the Collège de France shows the importance of a further and more detailed study of chemistry teaching and learning practices in a period in which the didactic models arose that characterise the teaching of this science until almost the present day.

## Notes

<sup>1</sup> William H. Brock, "Liebigiana: Old and new perspectives", *History of Science* 19 (1981): 201-218.

<sup>2</sup> The evolution of spaces and its consequences for the organization of chemistry teaching and research activities at the Collège de France has been studied in Antonio García Belmar and José Ramón Bertomeu Sánchez, "Teaching and research spaces. The chemistry chair of the Collège de France, 1770-1840", in *Spaces and Collections*, ed. Ana Carneiro and Marta Lourenzo (forthcoming).

<sup>3</sup> Thenard's teaching methods and the interplay between teaching and research activities has been studied in Antonio García Belmar, "The didactic uses of experiment: Louis Jacques Thenard's lectures at the Collège de France", in *Chemistry, Medicine and Crime. Mateu J.B. Orfila (1787-1853) and His Times*, ed. José Ramón Bertomeu Sánchez and Agustí Nieto-Galán (Sagamore Beach: Science History Publications, 2006), 25-54.

<sup>4</sup> Files <sup>F13/1082</sup>-1085 of the Parisian *Archives Nationales* contain the collection of documents concerning the works carried out in the building of the Collège de France during the first half of the 19<sup>th</sup> century.

<sup>5</sup> Between 1822 and 1823, Louis René Le Canu (1800-1871), recently named *chef des travaux chimiques* published a series of reports in collaboration with Serbat, who was at the time one of the *aide-préparateurs*. In one of them, they presented a method for obtaining a pure sample of uranium oxide from pitchblende. The new method had been developed "to obtain a certain quantity of uranium that we need for the chemistry lessons at the Collège de France" (L. R. Le Canu and Serbat 'Procédé pour obtenir l'oxyde d'urane pur', *Journal de pharmacie*, 9 (1823), 141-45).



<sup>6</sup>A large selection of these documents can be consultable at <[www.inrp.fr/she/cours\\_magistral/table/index.html](http://www.inrp.fr/she/cours_magistral/table/index.html)>.

<sup>7</sup>These conclusions are based on the analysis of two student notebooks (*Cours de chimie. Redigé d'après les leçons de Mr Thenard, Professeur au Collège de France, par Nicolas Jean Baptiste Gaston Guibourt, année 1809* (vol. I) and *Id. année 1810* (vol. II) (Bibliothèque Interuniversitaire de Pharmacie de Paris (Ms. 22-23) and *Cours de chimie minérale de Mr. Thenard, recueilli et résumé par Joseph Coldefy. Décembre 1808.* (Bibliothèque Universitaire de la Sorbonne, Ms. 1767). See García Belmar, "The didactic uses of experime".

<sup>8</sup>*Cours de chimie. Redigé d'après les leçons de Mr Thenard, Professeur au Collège de France, par Nicolas Jean Baptiste Gaston Guibourt, année 1809* (vol. I) and *Id. année 1810* (vol. II) (Bibliothèque Interuniversitaire de Pharmacie de Paris (Ms. 22-23), I, 65-66.

<sup>9</sup>*Cours de chimie minérale de Mr. Thenard, recueilli et résumé par Joseph Coldefy. Décembre 1808.* (Bibliothèque Universitaire de la Sorbonne, Ms. 1767), 30.

<sup>10</sup>L.J. Gay-Lussac and L.J. Thenard, "Extrait de plusieurs notes sur les métaux de la potasse et de la soude, lues à l'Institut depuis le 12 janvier jusqu'au 16 mai", *Annales de chimie* 66 (1808): 205-217.

<sup>11</sup>This was the main role of the *Collège de France* according to the *Lettres patentes du Roi concernant le Collège Royal données à Versailles le 16 Mai 1772. Régistrées en Parlement le 26 mars 1773* (Paris: Simon, 1773).

<sup>12</sup>*Registre des inscriptions pour le cours de Chimie par M. Thenard au Collège Impérial de France*, ACF, (A-XIV-20), 35 pp. Paris, ACF (A-XIV). The archives of the Collège de France (ACF) contain a collection of inscription registers in several courses with substantial information about the students. Inscription register started in 1808 (excepting those for medicine courses that begun in 1776 (A-XIV-8.)) and followed with continuity until 1830 and with growing discontinuity up to 1841. Signatures include information about student's origin, age and activity.

<sup>13</sup>On chemistry teaching for medical students see A. Bescher, *L'enseignement de la chimie en médecine au début du XIXe siècle* (Lyon: PhD, 1958) and J.R. Bertomeu Sánchez; A. García Belmar "Mateu Orfila's *Eléments de chimie médicale* and the debate about chemistry applied to medicine during the early XIXth century in France", *Ambix* 47 (2000): 1-27. On the evolution of chemistry in 19th century secondary education curricula see Bruno Belhoste, *Les sciences dans l'enseignement secondaire en France, 1789-1914* (Paris: INRP, 1995).

<sup>14</sup>Jacques-Pierre Maygrier, *Guide de l'étudiant en médecine...*(Paris: chez l'auteur, 1818), 41-42.

<sup>15</sup>Jonathan Simon, *Chemistry, Pharmacy and Revolution in France, 1777-1809* (Aldershot and Burlington: Ashgate, 2005).

<sup>16</sup>*Procès-verbaux...*, BIPh (Reg.24-25) and *Régistre...* Paris, ANF (AJ16/2427).

<sup>17</sup>Thenard offered his own views about this issue in a polemic paper about the "necessity of joining the practice to the theory of chemistry, in order to make useful applications to the arts". Among the numerous *citoyens*, who needed to learn chemistry in order to develop their professional activities, Thenard singled out "those who wanted to devote to chemical arts, pharmacy or medicine". To them, a "society of old students of the Ecole Polytechnique", to which Thenard belonged, offered since the beginning of nineteenth-century courses on practical chemistry given by young teachers "trained by the best teachers and experienced in all kind of chemical manipulations" (Louis Jacques Thenard "Sur la nécessité de réunir la pratique à la théorie de la chimie, pour en faire d'utiles applications aux arts", *Annales de chimie* 34 (1800): 106-110).

<sup>18</sup>Antonio García Belmar and José Ramón Bertomeu Sánchez, "Constructing the centre from the periphery. Spanish Travellers to France around the Chemical Revolution", in *Travels of*



*Learning. A Step towards a geography of science in Europe* (Dordrecht: Kluwer Academic Publishers, 2003), 143-188.

<sup>19</sup> As to the narration of experiments in chemistry manuals, see Bernadette Bensaude-Vincent, Antonio Garcia-Belmar and José Ramón Bertomeu-Sánchez, *L'émergence d'une science des manuels. Les livres de chimie en France (1789-1852)* (Paris: Archives Contemporaines, 2003), 183-203.

<sup>20</sup> Simon, *Chemistry, Pharmacy and Revolution...* As to the influence of the pharmaceutical tradition on chemistry teaching models, see Frederic L. Holmes, "The Complementarity of Teaching and Research in Liebig's Laboratory," *Osiris* 5 (1989): 121-164.

<sup>21</sup> M.G. Chapel d'Espinassoux, "La Jeunesse d'Orfila. Fragment d'une autobiographie inédite publié par ...", *Revue Hebdomadaire* 22-3 (1914): 615-34, 86-113, and 632.

<sup>22</sup> García Belmar, "The didactic uses of experiment", 45.

<sup>23</sup> See, among others, G. Sutton and V. Geoffrey, *Science for a Polite Society: Gender, Culture, and the Demonstration of Enlightenment*, (Boulder: Westview Press, 1995) and Larry Stewart, *The Rise of Public Science. Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660-1750* (Cambridge: Cambridge University Press, 1992); the special issue "Science Lecturing in the 18th century" of the *British Journal for the History of Science*, 28 (1), (1995); A.Q. Morton, "Lectures on natural philosophy in London, 1750-1765," *British Journal for the History of Science* 23 (1990): 411-434; J. Mertens, "Shocks and Sparks: The voltaic pile as a demonstrative device," *Isis* 89 (2) (1998): 300-311; T.L. Hankins, J.S. Robert, *Instruments and the Imagination*, (Princeton: Princeton University Press, 1995) (especially the chapter "The magic lantern and the art of demonstration"); J. N. Hays "The London lecturing empire, 1800-50," in: Ian Inkster and Jack Morrell, *Metropolis and Province: Science in British Culture, 1780-1850*, (London: Hutchinson, 1983) and I. Inkster, "The public lecture, an instrument of science education for adults —the case of Great Britain c. 1750-1850," *Paedagogica Historica* 20, (1) (1980): 80-107.

<sup>24</sup> Studies on the teaching of chemistry inside and outside the Universities of Glasgow and Edinburgh in the second half of the eighteenth century and the first third of the nineteenth century offer a good example of the variety of teaching and learning forms that coexisted in this period and the different meanings that the idea of experimental and practical teaching could have. (See on this topic studies by J.B. Morrell 'The chemist breeders: the research schools of Liebig and Thomas Thomson', *Ambix* 19 (1972): 1-58 and D. Fenby 'The lectureship in chemistry and the Chemical Laboratory, University of Glasgow, 1747-1818', in *The development of the laboratory: essays on the place of experiment in industrial civilization*, ed. F.A.J.L. James (Basingstoke: Macmillan, 1989), 22-36. Another example of that is the account made by Smeaton in 1954 of the European institutions where different forms of "laboratory instruction before 1794" were carried out (W.A. Smeaton, 'The early history of laboratory instruction in chemistry at the Ecole Polytechnique, Paris, and elsewhere', *Annals of science* 10 (1954): 224-33.

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## The Interplay of Chemical Teaching with Work and with Research: A Case Study from Germany around 1800, Johann Friedrich August Göttling at Jena

*Jan Frercks\**

### The socio-epistemological structure of chemistry in Germany around 1800

The socio-epistemological structure of chemistry in Germany around 1800 is depicted in Fig. 1. The notion of “socio-epistemological” is the key concept being put forward in order to be able to understand the nature of chemistry in Germany around 1800.<sup>1</sup>

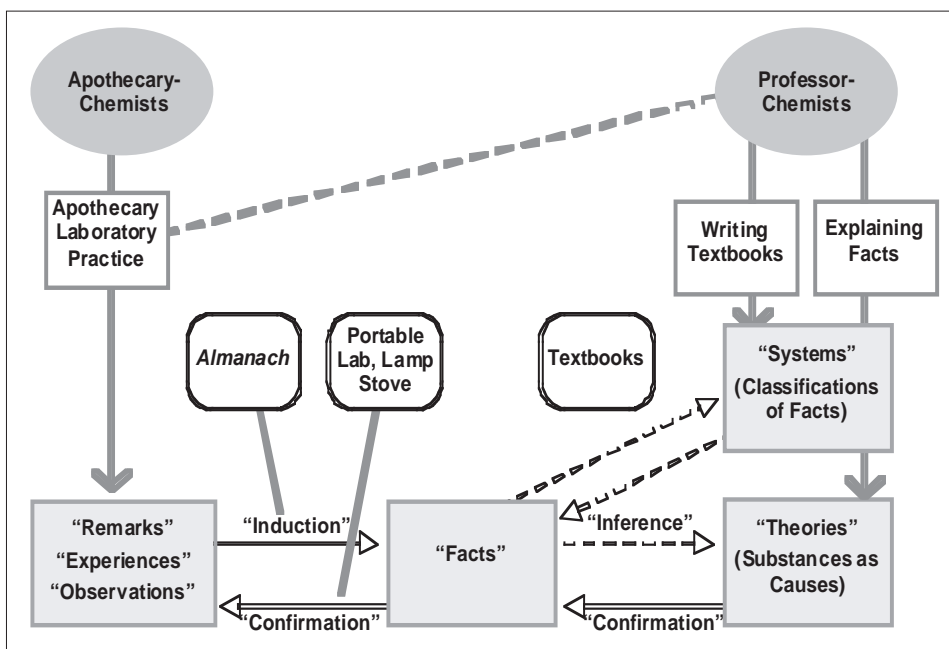


Figure 1. Socio-epistemological structure of chemistry in Germany around 1800.

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Chemistry was practised around 1800 by members of mainly two professional groups: apothecaries and university professors. Their main professional duties, respectively, were producing remedies and teaching chemical knowledge, it did not comprise chemistry as a research practice. There was hardly any institutionalised form of experimental chemistry in the sense of a well-directed pursuit of new chemical knowledge, carried out in dedicated research laboratories.

German chemists coped with this situation by using a three-level epistemology. Although this epistemology was mostly practiced without explicit discussion, at least some textbooks of chemistry explained it and presented it as the right way to do chemistry scientifically. The bottom row in Fig. 1 follows Scherer's *Versuch einer populären Chemie*.<sup>2</sup>

The first level of the epistemology consists of “remarks”, “experiences” or “observations”. These were made either in every day life or in a laboratory. By far the largest number of laboratories was part of apothecary shops, and an increasing proportion of apothecaries became interested in chemistry and communicated casual observations and remarks. They thereby became what Ursula Klein has aptly called “apothecary-chemists”.<sup>3</sup> Journals founded in the period, such as Florenz Lorenz Friedrich Crell's *Chemische Annalen für die Freunde der Naturlehre, Arzneigelahrtheit, Haushaltungskunst und Manufacturen* and, even more so, Johann Friedrich August Göttling's *Almanach oder Taschenbuch für Scheidekünstler und Apotheker* allowed a fast publication of what were sometimes minuscule observations. Although professor-chemists and other people could have provided this kind of raw material for chemical knowledge, the bulk of it came from the apothecary-chemists. In contrast, the main field of chemical research practice on the part of the professor-chemists consisted of doing something with the chemical facts. Either they interpreted the facts theoretically (mainly by tracing them to underlying reactions among different substances, including the physical substances such as the matters of heat, light, etc.) or they gathered and systemised the facts in textbooks (see below).

The epistemology's intermediate level of the chemical facts served to distance both kinds of chemical practice. Facts were seen as the epitome of chemistry: eternal, and belonging to nature rather than to some particular kind of scientific practice. In particular, making observations and remarks as apothecaries did, was regarded as not yet real chemical science. To be sure, it was the professor-chemists who invented this epistemology. Thus, socio-epistemological boundary-work was done in order to save “scientific” academic chemistry from the intrusion of practitioners from outside, whilst at the same time to allow themselves to draw

on the input from apothecaries and others, who were much better equipped in terms of laboratories and instruments than were the professors.

One instance illustrates this very clearly. In 1789, Götting announced in his *Almanach* that from then on he would refuse to publish long, theoretical treatises, and he urged his readers to abstain from sending him this kind of texts.<sup>4</sup> Chemistry strongly relied on the provision of remarks by the apothecaries, but, from the point of view of the professors, these remarks did not turn them into scientific chemists.

The inclusion and transformation of remarks into scientific knowledge was no easy task. Chemists had to somehow enable the transition from observations to facts and from facts to systems and theories. This was achieved by what it is suggested to call “techniques of mediation”. Götting invented several of them; attention will be focused on two of them which are directly related to teaching.

## **Textbooks**

Writing textbooks was one possibility to make something of the chemical facts.<sup>5</sup> Götting's *Handbuch der theoretischen und praktischen Chemie* can be seen as the starting point for an independent genre, textbooks of *theoretische Chemie*.<sup>6</sup> Three things are remarkable with regard to this kind of textbook. First, “theoretical” did not mean something of a deeper explanation, but rather, “non-practical”. Authors of textbooks of “theoretical chemistry” deliberately left out laboratory practice and confined themselves to gathering, selecting and arranging the known chemical facts, which resulted in a number of different chemical systems. Second, these textbooks were by no means didactic adaptations of systems of chemistry, elsewhere published. These textbooks were the very occasion to develop these systems in the first place.

Third, and difficult to explain, although developing ordered systems of, for example, chemical substances is without doubt a contribution to chemistry as a science (note that Mendeleev's periodic table was built in the course of writing a textbook), authors, which were exclusively university professors, couched their systems in a rhetoric of pure didactics. They bothered whether their systems were suitable for learning chemistry, but not whether or not they corresponded to nature.

This begs the questions, why several professor-chemists wrote such textbooks and did so in this particular way. Of course, writing one's own textbook was still

indispensible for academic teachers, if only for improving chances to get a salaried position, to allow the students to dispense with taking notes or to earn a part of one's living from authorship fees. However, this still that does not explain why they chose facts as the content of their textbooks, and why they restricted their own, alleged, role to *presenting* systems of facts rather than to *finding* these systems.

It is suggested that this is to be understood as part of a strategy to professionalise chemistry as a science. On the social level, this meant to claim, scientific chemistry was restricted to professor-chemists. But since these were paid exclusively for teaching, and not at that time for research, they presented their theoretical chemistry as pure didactics. On the epistemological level, this went along with the emerging separation of research and teaching. While in terms of institutions, research was integrated in the universities during the 19th century (in Germany, at least), in terms of epistemological relevance, teaching was deprived its primary role. Since then, the realm of teaching was more and more regarded as subsidiary to research. This modern view seemed to have had already spread among chemists around 1800.

### **Portable laboratories**

The second example mediates between observations and facts. Göttling designed different portable laboratories, which contained a selection of reagents and some very simple chemical equipment.<sup>7</sup> These portable laboratories served several purposes for establishing chemistry as a science, apart from providing Göttling with an extra income in addition to his modest university salary.

For example, portable laboratories made the laboratory mobile. Chemical tasks such as the analysis of mineral waters could then be done without access to an apothecary laboratory. It is known that these portable laboratories were successful. On request, Göttling later offered a cheaper version containing only those reagents that were of particular use for medical analyses.

Likewise, portable laboratories provided facilities for teaching and learning. Students were enabled to learn chemistry from their own experience, outside the lecture hall. Furthermore, the portable laboratories with their sets of instructions merged tacit skills mostly found in apothecary laboratories with textual knowledge about substances usually found in university textbooks. The reagents and the recipes were standardised according to Göttling's own purity standard. Thus,

the portable laboratories provided for standardisation of chemical work and of chemical teaching at the same time.

The manual for the first portable laboratory was entitled *Vollständiges chemisches Probir-Cabinet zum Handgebrauche für Scheidekünstler, Aerzte, Mineralogen, Metallurgen, Technologen, Fabrikanten, Oekonomen und Naturliebhaber* (Complete chemical portable laboratory for chemists, medical doctors, mineralogists, metallurgists, technologists, factory owners, economists and amateurs). Addressing as many potential buyers as possible in the title was common practice. All the more it is telling that Götting mentioned neither apothecaries, nor professors. Thus, the message given by these portable laboratories was clear. Everyone could learn chemistry and practice chemistry, in particular outside its two dominant institutions, the apothecary shop and the lecture hall.

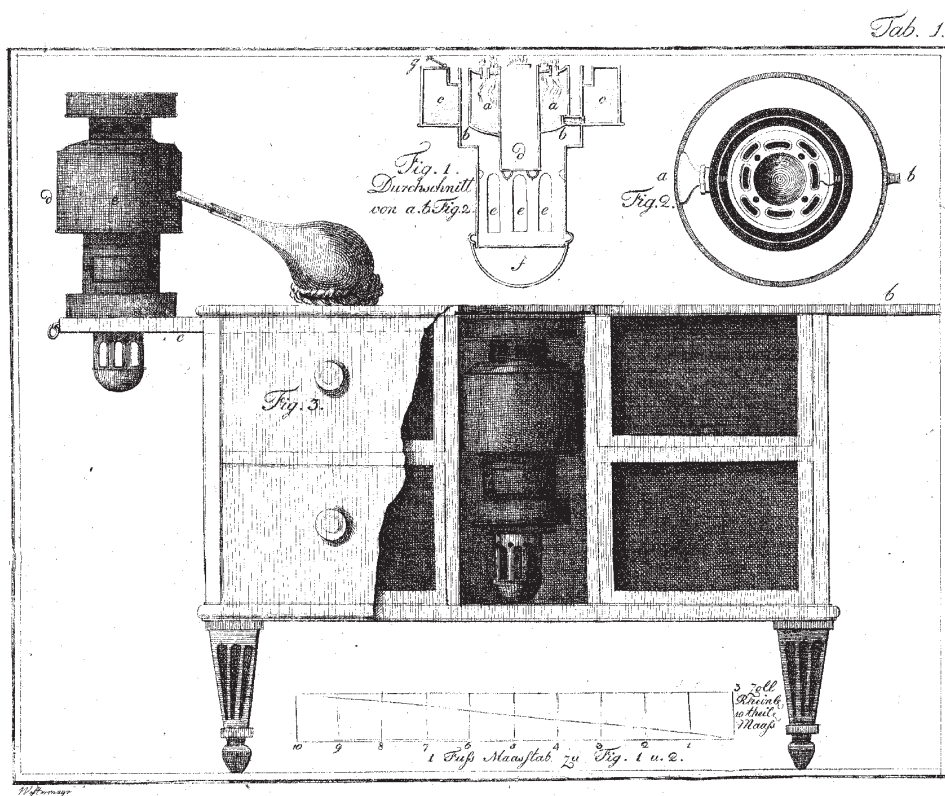


Figure 2. Cupboard with lamp-stove for students, from: *Almanach oder Taschenbuch für Scheidekünstler und Apotheker* 1796. Courtesy Thüringer Universitäts- und Landesbibliothek Jena.

In addition, Göttling invented a lamp-stove for students and even a special cupboard, which allowed turning a student's room into a chemical laboratory with a few touches. Göttling himself used the lamp-stove in his lectures, and it is most probable that the portable laboratories also served him, for bridging the distance between his laboratory and the lecture hall.

## Conclusion

These are two examples of just how intimately the practice of teaching was tied to the construction of chemistry as a research science. Teaching, the core activity of the main group of chemists in Germany around 1800, was inextricably interwoven with chemistry *per se*. Nevertheless, there was also the explicit and implicit denial of any importance of teaching, uttered by the actors themselves. In the epistemology, the way back from theories via facts to observations was seen as a merely didactic “confirmation”.

This discrepancy between daily practice on the one hand and the three separations (between the epistemological levels, between the social groups, and between research and teaching) on the other hand should not be ignored. In contrast, the role of these discrepancies for the formation of chemistry has to be understood.

## Notes

<sup>1</sup> For an extended discussion of the topics of the talk and for the full set of references, see Jan Frercks, “Techniken der Vermittlung: Chemie als Verbindung von Arbeit, Lehre und Forschung am Beispiel von J. F. A. Götting,” *NTM. Journal for History of Science, Technology and Medicine*, 16 (2008), 279–308. A further paper focussing on the socio-epistemological concept of “chemical fact” is in preparation.

<sup>2</sup> Alexander Nicolaus Scherer, *Versuch einer populären Chemie* (Mühlhausen: Danner, 1795).

<sup>3</sup> Ursula Klein, “Apothecary-Chemists in Eighteenth-Century Germany,” in *New Narratives in Eighteenth Century Chemistry*, ed. Lawrence M. Principe (Dordrecht: Springer, 2007), 97–137.

<sup>4</sup> *Almanach* 1789, pp. 176–181. Maybe not accidentally, Götting wrote this at the moment when he had just become a professor at the University of Jena.

<sup>5</sup> This is explained in much more detail in Jan Frercks and Michael Markert, “The Invention of *Theoretische Chemie*. Forms and Uses of German Chemistry Textbooks, 1775–1820,” *Ambix* 54 (2007): 146–171. Textbooks in general receive increasing attention since some years, see for example Bettina Haupt, *Deutschsprachige Chemielehrbücher (1775–1850)* (Stuttgart: Deutscher Apotheker Verlag, 1987), Anders Lundgren and Bernadette Bensaude-Vincent, *Communicating Chemistry: Textbooks and Their Audiences, 1789–1939*, ed. (Canton/Mass.: Science History Publications, 2000), or José Ramón Bertomeu-Sánchez, Antonio García-Belmar, and Bernadette

Bensaude-Vincent, "Looking for an Order of Things: Textbooks and Chemical Classifications in Nineteenth Century France," *Ambix* 49 (2002): 227-250.

<sup>6</sup> Johann Friedrich August Göttling, *Handbuch der theoretischen und praktischen Chemie*, 3 vols. (Jena: Akademische Buchhandlung, 1798-1800).

<sup>7</sup> On portable laboratories see William Arthur, L. B. Smeaton, "The Portable Chemical Laboratories of Guyton de Morveau, Cronstedt and Göttling," *Ambix* 13 (1966), 84-91, and Brian Gee, "Amusement Chests and Portable Laboratories: Practical Alternatives to the Regular Laboratory," in *The Development of the Laboratory: Essays on the Place of Experiment in Industrial Civilization*, ed. Frank A. J. L. James (Basingstoke: MacMillan Press, 1989), 37-59.



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## Chemistry beyond the Academy

*Robert G. W. Anderson\**

Outside the formal courses offered by the universities, which existed largely to fulfil the needs of medical students, chemistry teaching proliferated in a multiplicity of forms in late 18<sup>th</sup> and early 19<sup>th</sup> century Scotland. Some courses were offered with clear aims in mind, but many of those studying chemistry were caught up in a wave of enthusiasm, simply wanting to discover more about what the subject embraced. There were plenty of proficient teachers available who were willing to help fulfill the demand: the names James Anderson (1739-1808), Thomas Thomson (1773-1862), Andrew Ure (1778-1827), Andrew Fyfe (1792-1861), Edward Turner (1798-1837), William Gregory (1803-1858), David Boswell Reid (1806-1863) and George Wilson (1818-1859), all of whom found high-status employment and fame later in their careers, spring to mind.. Benefiting from the success of the courses, publishers were encouraged to provide chemical texts, while instrument makers marketed cheap chemistry equipment, allowing practical work to be undertaken at home. The subject covers a broad spectrum of how chemical knowledge was to spread, and it incorporates the expansion of Scottish universities and how they allowed for the development of extra-mural teaching, the origins and spread of the mechanics movement, and how an understanding of chemistry could be regarded as a social attainment.

Of those who signed on for the lectures of the legendary Joseph Black (1728-1799), relatively few were studying to fulfil the professional requirements needed for graduation in medicine. A number of those attending would become surgeons, but it is clear that many attended out of curiosity for the science or because they thought it would be useful for them in their line of work. It is therefore unsurprising that this created an atmosphere which encouraged teaching outside the university, where to be successful, lecturing skills were paramount. It was not infrequently remarked that while the professors were dull, the extra-mural lecturers brought excitement to the subject. Many students wished to perform experiments for themselves. Black's successor Thomas Charles Hope (1766-1844) had the reputation of discouraging experimentation, driving students into the arms of teachers who were providing courses extramurally. To take account of the inevitable drift

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away from the university-provided teaching, extra-mural courses were allowed to count towards the university requirements of those intending to become doctors, providing the teachers had certain qualifications themselves. Hope had a particularly acrimonious argument with a man he had hired as his assistant, David Boswell Reid, who attempted to have a separate chair of practical chemistry established. Eventually Reid privatised himself, and offered popular extra-mural courses in a house intimidatingly close to Hope's official lecture theatre.

University-style teaching did not account for all demands. Providing for those at a lower end of the social scale who wanted to know something of science, mechanics institutes were established, where courses were given in the evenings for low fees. These had their roots in Glasgow at the end of the eighteenth century in Glasgow University and the Andersonian Institution, though the first independent institute, the Edinburgh School of Arts, was established at in 1821. Here, a systematic course of chemistry was immediately offered by Andrew Fyfe, author of *Elements of Chemistry... A Manual of Chemistry for the Use of Pupils of Mechanics Institutions* (1827). He regularly filled the lecture room, before he was appointed to a professorship at Aberdeen University. Mechanics Institutions sprang up all over Scotland; by 1851 there were about 55 of them, ranging in membership from 20 to 700. Even a small town such as Kelso (in the south of Scotland), with a population of around 4,000, had a regular course of chemistry lectures provided by a local minister of religion, whose course incorporated experimental demonstrations. Over the period 1768 to 1860, forty-six extra-mural teachers have been identified in Scotland (though this includes a few who also provided 'official' university courses). Over the period under consideration, there ran a swathe of fascination for chemistry. Dr Johnson's biographer, James Boswell, who attended Black's special course of chemistry offered to solicitors and advocates, was certainly in the minority when he wrote "We were mainly lawyers... I did not feel much curiosity for the science."

## References

- R. G. W. Anderson, *The Playfair Collection and the Teaching of Chemistry at the University of Edinburgh 1713-1858* (Royal Scottish Museum: Edinburgh, 1978).  
Robert G W Anderson, 'Boerhaave to Black: the Evolution of Chemistry Teaching' *Ambix* 53 (2006) 237-254.  
Jan Golinski, *Science as Public Culture* (Cambridge University Press: Cambridge, 1992).  
Thomas Kelly, *George Birkbeck Pioneer of Adult Education* (Liverpool University Press: Liverpool, 1957).  
J. B. Morrell, 'Practical Chemistry at the University of Edinburgh' *Ambix* 16 (1969) 66-80.  
Lisa Rosner, *Medical Education in the Age of Improvement* (Edinburgh University Press: Edinburgh, 1991).

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### 3. CHEMISTRY IN RELATION TO PHYSICS IN THE XXTH CENTURY

*Introduction by Ana Simões*

The relations of chemistry to physics have been the concern of many practicing chemists throughout time and place. Especially with the emergence and development of physical chemistry in the late 19<sup>th</sup> century, the advent of chemical physics which transformed the landscape of 20<sup>th</sup> century chemistry by consolidating theoretical chemistry as a core component of a discipline traditionally considered as a laboratory science, and the introduction of physical instruments, techniques and methods into several domains of chemistry, the relations of chemistry to physics have been an implicit component of the daily practice of some 20<sup>th</sup> century chemists as well as an explicit part of their oral and written reflections on chemical culture.

Such was the case of the American physical chemist Gilbert Newton Lewis. Excelling in the application of thermodynamics to chemistry, Lewis's scientific interests also included valence theory, theory of radiation and relativity. In the last stage of his career, Lewis tried to devise a new chemistry of deuterium compounds, a field he abandoned for research on photochemistry. In 1926, in the context of a broader reflection on the structure and methods of science offered in a popularisation book called *The Anatomy of Science*,<sup>1</sup> and later on, in 1933, in a paper titled "The chemical bond" published in the first volume of the *Journal of Chemical Physics*,<sup>2</sup> Lewis assessed the relations of chemistry to physics and contrasted the different features of theories in chemistry and physics. He compared the analytical characteristics of chemical theories to the synthetic features of physical theories. Chemical theories are grounded on a large body of experimental material from which the chemist attempts to deduce a body of simple laws which are consistent with the known phenomena. Contrariwise, physical theories postulate laws governing the mutual behavior of particles and then attempt "to synthesise an atom or a molecule."<sup>3</sup> Furthermore, Lewis contrasted the convergent method of chemists and the divergent method of physicists, thereby implicitly acknowledging the theoretical and methodological irreducibility of chemistry to physics.

In this session, Steven J. Weininger, a practicing chemist and historian of chemistry, and three historians of science representing successive generations of schol-

ars, the Sarton medallist Mary Jo Nye, Carsten Reinhardt, the author of the recent book *Shifting and Rearranging*, and the young scholar Néstor Herran, focused on 20<sup>th</sup> century chemistry in its relation to physics addressing the hypothetical reduction of chemistry to physics by looking at different case studies which, in all instances except for one,<sup>4</sup> were associated with the role of isotopes, be it stable deuterium, or the unstable isotopes which made up the new world of radioactivity.<sup>5</sup>

Pointing to the neglect by historians of the chemistry of deuterium and other stable isotopes, Weininger showed how chemists' and physicists' embrace of deuterium depended on its accommodation within different experimental cultures and accessibility to various physical and chemical techniques. Initially the appropriation of deuterium reinforced the distinction between the two disciplines, while later on its use within biochemistry melded together the two distinct analytical traditions.

Mary Jo Nye chose to analyse the scientific trajectories of two physical chemists, Fritz Paneth and Michael Polanyi, both émigrés from Germany to England in the early 1930s, and to discuss their philosophical reflections at the interface of chemistry and physics. Polanyi and Paneth became good friends in the 1920s, and Polanyi was involved in a debate on the nature of isotopes, and specifically on their chemical identity, between Paneth and Georg Hevesy in Vienna and Kasimir Fajans in Karlsruhe. Both Paneth and Polanyi insisted on the distinctive characterisation of chemistry as depending on the inexactness of its ideas, on the importance of exceptions to its rules, on the vagueness of its methods, and on its emergent properties.

By pointing to the hybrid character of radioactivity, a discipline depending on theories, practices and instruments coming from both physics and chemistry, Néstor Herran called attention to the role played by two factors –disciplinary ambiguity and high public profile– in shaping the appropriation of radioactivity in Spain. By stressing both the lack of consideration given so far to the interrelationship between radioactive research and its public representation, enhanced by its potential industrial and medical applications, he discussed the specificities of the appropriation of radioactivity in the context of a European periphery. He argued that its appropriation in Spain depended on overcoming tensions between radioactivity and former chemical ways of thought and doing, and on institutional and individual support by politically conservative scientists able to counteract the image of radioactivity as a subversive science conveyed by the socialist press and other popularisation of science outlets. In this way, historians of science were

invited to consider how much case studies such as this one encourage the revision of received views on the history of radioactivity.

Carsten Reinhardt looked at the leading role of physical instruments as carriers of novel techniques and principal actors in a novel method-making oriented chemistry. He addressed the ambivalent attitudes of chemists who used and appropriated physical instruments and methods into their own culture, in such a way that they could be more readily embraced by the chemical community. Such a process revealed varying degrees of acceptance, went hand in hand with manifestations of resistances from different sectors dependent on the specificities of the receiving chemical cultures, and contributed in the end to build a community of method makers able to cross boundaries not only inside the physical sciences but also of the life sciences and medicine as well.

By extending Lewis's considerations from theory to practice and to instrumentation, by taking into account specific local contexts and public representations of science, the participants in this session enlarged and enriched the discussion of the dynamic relations of chemistry to physics in the 20<sup>th</sup> century, in innovative ways, open for scrutiny.

## Bibliography

Mary Jo Nye, "Physics and chemistry: commensurate or incommensurable sciences?" in Mary Jo Nye, J.L. Richards, R.H. Stuewer, eds., *The invention of physical science* (Dordrecht:Kluwer Academic Publishers, 1992), pp.205-224.

Mary Jo Nye, *From chemical philosophy to theoretical chemistry. Dynamics of matter and dynamics of disciplines 1800-1950* (Berkeley: University of California Press, 1993).

Carsten Reinhardt, ed., *Chemical Sciences in the 20<sup>th</sup> century. Bridging Boundaries* (New York: Wiley-VCH, 2001).

Ana Simões, "Dirac's claim and the chemists," *Physics in Perspective*, 4 (2002), 253-266.

Ana Simões, "Chemical Physics and Quantum Chemistry in the Twentieth-Century," in Mary Jo Nye, ed., *Modern Physical and Mathematical Sciences. Cambridge History of Science. 5th Volume* (Cambridge: Cambridge University Press, 2003, vol.5), pp. 394-412.

Carsten Reinhardt, *Shifting and Rearranging. Physical Methods and the Transformation of Modern Chemistry* (Sagamore Beach, Mass.: Science History Publications, 2006).

## Notes

<sup>1</sup> G.N. Lewis, *The Anatomy of Science* (Yale University Press: New Haven, 1926).

<sup>2</sup> G.N. Lewis, "The Chemical Bond," *Journal of Chemical Physics*, 1933, 1, 17-28.

<sup>3</sup> Lewis, “Chemical Bond,” op. cit. (2), 17.

<sup>4</sup> Carsten Reinhardt, “Applied Neighborhood. Physical Methods and their Perception in Chemistry”.

<sup>5</sup> Stephen J. Weininger, “Deuterium as a probe of the boundaries between physics, chemistry and biochemistry;” Mary Jo Nye, “Philosopher-Scientists at the Interface of Physics and Chemistry: Paneth and Polanyi on Chemistry as an Exact Science;” Néstor Herran, “A ‘subversive element’: publics, disciplines and the rise of Madrid’s laboratory of radioactivity”.

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## Philosopher-Scientists at the Interface of Physics and Chemistry: Paneth and Polanyi on Chemistry as an Exact Science

Mary Jo Nye\*

Many historians and philosophers of science have addressed historical and epistemological questions about the relations between chemistry and physics.<sup>1</sup> Eric Scerri is among philosophers who have argued that theoretical chemistry cannot be reduced to the theoretical physics of quantum mechanics, and others, including chemists such as Stephen Weininger, Roald Hoffmann and Pierre Laszlo, have discussed the existence of distinctly chemical concepts such as molecular shape, aromaticity, steric effects, strain, and reactivity.<sup>2</sup> Recently R. J. Snooks has suggested that a difference between chemistry and physics is physicists' use of thought experiments. Snooks argues that, in contrast to physics, chemistry does not exhibit universal laws that originate in *a priori* reasoning and are open to thought experiments.<sup>3</sup>

On several occasions, Scerri has brought attention to the role in chemical philosophy of the physical chemist Friedrich Adolf Paneth (1887-1958). Fritz Paneth's historical and philosophical papers were collected in a 1964 volume edited by Herbert Dingle, and Paneth was profiled by Klaus Ruthenberg in a 1997 essay on "philosophising chemists" in the journal *HYLE*.<sup>4</sup> Paneth was a colleague of another philosophising chemist, Michael Polanyi (1891-1976), who also has been profiled in *HYLE* and whose anti-positivist writings on the personal and practical character of scientific knowledge are well-known among philosophers and sociologists of science since the 1960s. As with Paneth, the earliest of Polanyi's philosophical writings date back to the 1930s.<sup>5</sup>

For the physical chemists Paneth and Polanyi, the practice of chemistry had a great deal to do with the instruments, mathematical methods, and explanatory theories of physics. Their own chemical research depended on the pioneering work of physicists such as Ernest Rutherford and Niels Bohr, and, at a more personal level, on collaboration with physicists such as Georg von Hevesy, Fritz London,

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and Eugene Wigner. Paneth and Polanyi shared in their attitudes toward the relation between physics and chemistry an emphasis on the irreducibility of the behavior of chemical compounds to merely the properties of atoms, and a conviction of the inexactitude and flexibility of chemical rules and laws in comparison to physical theories. In what follows, the first discussion concerns the professional interactions between Paneth and Polanyi, and then there follow a few examples of their characterisations of the relations between chemistry and physics.

### Chemical Careers

In 1931 Fritz Paneth gave a public lecture on chemical epistemology to the Gelehrte Gesellschaft of Königsberg. The lecture was published immediately in the Society's *Schriften* and much later, in 1962, it was translated into English for the *British Journal of the Philosophy of Science* by Paneth's son, the physicist and philosopher Heinz Paneth, also known by his Anglicised name, Heinz Post.<sup>6</sup> The Austrian-born Fritz Paneth received his doctorate in chemistry in Vienna and became in 1912 an assistant to Stefan Meyer at the Institut für Radiumforschung. Paneth taught in Vienna, Prague, and Hamburg, and he then had appointments in Berlin from 1922 to 1929 and in Königsberg from 1929 to 1933, when he was forced to leave Germany because of his Jewish origins. In England, Paneth had appointments at Imperial College in the University of London. He moved to the University of Durham in 1939. He led the chemistry division of the Joint British-Canadian Atomic Energy Team in Montreal during wartime and returned to Germany in 1953, to the Max Planck Institute in Chemistry in Mainz, after his mandatory retirement at Durham. Paneth is perhaps best known in chemistry for his demonstration in 1929 of the existence of the methyl free radical, using a lead mirror (or film) technique, and for his lifelong work in radiochemistry, including his early pioneering research in Vienna, in collaboration with Georg de Hevesy, using radioactive isotopes in tracer experiments.<sup>7</sup>

Michael Polanyi was born in Budapest, where he completed a medical degree in 1913 and a Ph.D. thesis in physical chemistry in 1919. He studied physical chemistry in Karlsruhe in 1912 with Georg Bredig and Kasimir Fajans, and he briefly returned to Karlsruhe in 1919 after political events forced him to leave his position at the University of Budapest where he was Hevesy's assistant in physical chemistry. Polanyi moved to the Kaiser Wilhelm Institute for Fiber Chemistry in Berlin in 1920, and he became director of the chemical-kinetics research group in Fritz Haber's Institute for Physical Chemistry and Electrochemistry in 1923. Anti-Semitism forced Polanyi to leave Germany in 1933, and he settled at the



University of Manchester, first as Head of Physical Chemistry, and later, from 1948 to 1952, as Professor of Social Studies. In physical chemistry, Polanyi's researches focused on the adsorption of gases on solid surfaces, including a quantum mechanical approach to adsorption which he developed with Fritz London, x-ray diffraction studies of natural fibers and metals, and chemical kinetics, including Polanyi's development with Henry Eyring of what came to be called the "semi-empirical method" for predicting transition states and activation energies for chemical reactions.<sup>8</sup>

Polanyi and Paneth may have first met at an April 1920 meeting of the Bunsengesellschaft in Halle, and they were friends in the 1920s in Berlin.<sup>9</sup> Polanyi had been involved in an earlier debate on the nature of chemical isotopes between Fajans in Karlsruhe and Paneth and Hevesy in Vienna. Fajans and Hevesy each had done research in Ernest Rutherford's laboratory in Manchester, and Paneth first met Hevesy in Rutherford's laboratory in 1913.<sup>10</sup> Paneth spent the summer term of 1913 in Frederick Soddy's laboratory in Glasgow at the time that Soddy was coining the term "isotopes" for atoms having differing atomic mass and the same atomic number, the concept of atomic number having been just established by George Moseley. Collaborating in Vienna, Paneth and Hevesy in 1914 took Soddy's position that different isotopes of the same atomic number were chemically identical rather than just very similar, exhibiting what Paneth and Hevesy called *Vertretbarkeit*, or the facility of replacing each other, in crystallisation and in electrochemical reactions. Fajans, then in Karlsruhe, disagreed and included among his arguments against the chemical identity of isotopes some thermodynamic arguments that he got from an unpublished paper by Polanyi, predicting that two substances of different atomic weight would have different free energies.<sup>11</sup> Polanyi learned from Hevesy that Hevesy and Paneth planned a reply to Fajans, and Polanyi suggested to Fajans that Hevesy, Paneth and Fajans compose a joint paper. Instead, separate articles appeared, with the view of Hevesy and Paneth winning out by the early 1920s, after research by Francis Aston and others made clear that there are many more isotopes than just the radioactive ones that had been discovered first.<sup>12</sup>

## **Philosophical Reflections**

As with the discovery of isotopes, many developments in physics and chemistry in the early decades of the twentieth century highlighted the question of the relations between the practices and epistemologies of chemistry and physics. In his Königsberg lecture of 1931, Paneth drew upon his experiences in radiochemistry

and in free-radical chemistry to discuss some fundamental chemical concepts in light of recent scientific results. He noted that mathematical statements often are not preferable or adequate in chemistry. Sodium chloride is salty, hydrogen sulfide smells bad, cinnabar is red, and gold is lustrous. Chemistry, he noted, referring to a description by Emil Fischer in Fischer's autobiography, is about "bangs and stinks." Physicists no longer speak of molecules and atoms as solid spheres, he said, but as mathematical relations in four-dimensional space. "But successes of chemistry often lie elsewhere", cautioned Paneth, "as in biological and historical sciences."<sup>13</sup>

Paneth reflected on the old conceptions of matter theory, elementary substances, and chemical transformations. How do elements persist in compounds? In what sense does the soft metal sodium and the poisonous green gas chlorine exist in colorless, crystalline table salt? How surprising is it, Paneth asked, that Spinoza was skeptical in the 1660s of Robert Boyle's demonstration that nitre may be synthesised from an acid and a base, which are substances of entirely different properties? A distinction must be made in chemistry, Paneth explained, between a simple substance, such as chlorine, that can be characterised by its qualitative properties and a fundamental, or transcendent, substance, such as chlorine, that persists in compounds as a substance without qualities. The atomic theory of Rutherford and Bohr lets us understand that the unchanged atomic nucleus constitutes the fundamental substance, while the optical, chemical, and other qualities of the simple substance have disappeared.<sup>14</sup> Paneth also attributed this distinction to Dmitri Mendeleev, noting that Mendeleev differentiated the "material constituent, not perceptible to the senses, of a composite body" from the "simple body as a single homogeneous substance."<sup>15</sup> Similarly, said Paneth in 1931, "the radicals of organic chemistry exist, *almost without exception*, in the world of the transcendental alone."<sup>16</sup>

One of the key differences between chemistry and physics, most certainly for Paneth and Polanyi, lay in this phrase "almost without exception." Polanyi made this point in a very brief letter of 1936 to the British journal *Philosophy of Science* a few years after he settled in Manchester and about the time that he found himself defending the semi-empirical approach of his transition-state theory against objections from physicists who demanded *ab initio* calculations for energy states.<sup>17</sup> "The subject of chemical concepts as opposed to physical ones", –Polanyi wrote– "has always been fascinating to me because it shows the great value of **inexact** [the present author's emphasis] ideas." Chemistry, Polanyi continued, is a world of ideas expressed by such terms as "relative stability", "affinity", "tendency", "inclination", and "general expectation", as descriptions of behavior.

“There is not a single rule in chemistry which is not qualified by important exceptions”, he wrote. Chemists would have been ill-advised, he continued, to heed physicists’ counsel to abandon vague methods and to restrict investigations to fields where exact laws pertain. The development of chemistry, wrote Polanyi, “would at that moment have stopped dead.”<sup>18</sup> Neither the characterisation of substances nor synthesis of compounds could be achieved by exact methods. Chemistry is an art, which depends on enlarging the investigator’s field of awareness, a theme that Polanyi would reiterate in later essays and lectures and in his book *Personal Knowledge* of 1958. At a meeting of the Faraday Society in September 1937, Polanyi introduced these ideas about the merits of the inexact in a defense of his semi-empirical approach in transition-state theory by saying: “Personally I attach no importance at the present stage to a precise numerical agreement between theory and experiments, but I believe that the theory can claim to give a reasonable picture of the mechanism of chemical reactions which would otherwise remain in the dark.”<sup>19</sup>

Surprisingly, Polanyi’s magnum opus *Personal Knowledge* includes very little that is specifically chemical epistemology or discussion of the relation between physics and chemistry. Instead Polanyi applied his experiences during a long career in physical chemistry and his readings in psychology, anthropology, and philosophy to an examination of the nature of scientific practice and of scientific knowledge in general. He did use the example of isotopes, however, to discuss tacit changes in the meaning of scientific language. In the early 1920s, Polanyi wrote in *Personal Knowledge*, isotopes were defined by their same atomic numbers and their chemical inseparability. Following Harold Urey’s 1932 discovery of deuterium, however, the criterion of chemical inseparability was tacitly abandoned as an ironclad rule because hydrogen and deuterium were such notable exceptions.<sup>20</sup> Again, then, we see a distinction made between physics and chemistry on the basis of the universal character of the one, and the exceptional character of the other.

Like Paneth in his lecture on epistemology in 1931, Polanyi compared chemical science to the biological and historical sciences, i.e., to natural history. The chemist’s description and understanding of chemical elements and compounds, wrote Polanyi in *Personal Knowledge*, requires the kind of connoisseurship demonstrated in the naturalist’s identification of biological specimens and the taxonomist’s capacity for “delicate discrimination.”<sup>21</sup> Polanyi applied the notion of inexactness to Paneth’s problem of the persistence of elementary substances in chemical compounds, by postulating the unspecifiability of higher levels of organisation from knowledge of characteristics belonging to lower levels. He invoked the existence

in chemical combinations of emergent properties, an idea that harked back to Aristotle in antiquity and to Pierre Duhem, more recently, in the nineteenth century.<sup>22</sup> “Consider the chemical aspects of matter”, wrote Polanyi:

“They are fully determined by atomic physics; yet no Laplacean Mind schooled in quantum mechanics could replace the science of chemistry. For chemistry answers questions regarding the interaction of more or less stable chemical substances, and these questions cannot be raised without experience of the substances and of the practical conditions in which they are handled. A Laplacean knowledge which merely predicts what will happen under *any given* conditions cannot tell us what conditions *should be given*; these conditions are determined by the technical skill and peculiar interests of chemists and hence cannot be worked out on paper. Therefore while quantum mechanics can explain in principle all chemical reactions, it cannot replace, even in principle, our knowledge of chemistry. We may acknowledge this as an incipient separation of two forms of existence.”<sup>23</sup>

The notion of emergent properties in complex systems would become more common in philosophy of science in the later decades of the twentieth century.<sup>24</sup> With respect to chemistry, for example, Mario Bunge wrote in 1997 that:

“At first sight chemistry is included in physics because chemical systems would seem to constitute a special class of physical systems. But this impression is mistaken, for what is physical about a chemical systems [sic] is its components rather than the system itself, which possesses emergent (thought explainable) properties in addition to physical properties.”<sup>25</sup>

## Conclusion

In conclusion, it is to be noted that in their philosophical reflections on the relations between chemistry and physics, there are some striking similarities between the views of the central European physical chemists Fritz Paneth and Michael Polanyi. Their chemical philosophy can be contrasted usefully with the French physical chemist Pierre Duhem, as mentioned earlier. In his philosophy of chemistry and in his philosophy of science more broadly, Duhem was backward-looking, while Paneth and Polanyi were forward-looking. Duhem was an anti-atomist who opposed the use of mechanical models and corpuscular hypotheses in physics and chemistry. Grounding his philosophical views as much in his conservative Catholicism as in his work in thermodynamics and physical science, Duhem taught that physical theory must be purely descriptive and symbolic, lea-

ving statements about reality, such as material reality distinct from sensible appearances, to metaphysics and theology.<sup>26</sup>

Duhem drew upon Aristotelian natural philosophy and medieval Thomism to enhance his understanding of modern physics and thermodynamics. In contrast, Paneth and Polanyi took up the new physics of particles and waves, and of radioactivity and quantum mechanics in their daily practice of chemistry. They admired the new physics, and they knew it well enough to understand differences between current chemical and physical theories and practices. As Paneth said in a talk about inorganic chemistry at a meeting of the British Association in Edinburgh in 1951, “many of the greatest advances in physics have been made on the basis of chemical discoveries . . . . Today there is only one fundamental science of the inorganic world, of which chemistry, physical chemistry, chemical physics, and physics are just different chapters.”<sup>27</sup>

Bernadette Bensaude-Vincent has noted the importance of matter theories and chemical concepts in twentieth-century French philosophy of science. With this in mind, Paneth and Polanyi can perhaps be identified more readily not with Duhem’s epistemology, but with Gaston Bachelard’s 1932 statement of the “pluralisme cohérent de la chimie moderne.”<sup>28</sup> No reductionism here, but a recognition of the fruitfulness of the relations between physics and chemistry, as well as their commonalities and differences.

## References

<sup>1</sup> Among these studies is M. J. Nye, *From Chemical Philosophy to Theoretical Chemistry: Dynamics of Matter and Dynamics of Disciplines, 1800-1950* (Berkeley: University of California Press, 1993).

<sup>2</sup> Eric Scerri, “Just How *Ab Initio* Is *Ab Initio* Quantum Chemistry?” *Foundations of Chemistry* 6 (2004): 93-116. Stephen J. Weininger, “The Molecular Structure Conundrum: Can Classical Chemistry Be Reduced to Quantum Chemistry?” *Journal of Chemical Education* 61 (1984): 939-944; Roald Hoffmann, “Nearly Circular Reasoning”, *American Scientist* 76 (1988): 182-185; Roald Hoffmann and Pierre Laszlo, “Representation in Chemistry”, *Angewandte Chemie, International Edition in English* 30 (1991): 1-16; Pierre Laszlo, “Circulation of Concepts”, *Foundations of Chemistry* 1 (1999): 225-239.

<sup>3</sup> R. J. Snooks, “Another Scientific Practice Separating Chemistry from Physics: Thought Experiments”, *Foundations of Chemistry*, 8 (2006): 255-270.

<sup>4</sup> Eric R. Scerri, “Realism, Reduction, and the ‘Intermediate Position,’” in *Of Minds and Molecules: New Philosophical Perspectives on Chemistry*, eds. Nalini Bhushan and Stuart Rosenfeld (Oxford: Oxford University Press, 2000), 51-72, and Scerri, “Some Aspects of the Metaphysics of Chemistry and the Nature of the Elements”, *HYLE: International Journal for Philosophy of Chemistry* 11 (2005): 127-145; Fritz Paneth, *Chemistry and Beyond: A Selection*, eds. Herbert

Dingle and G. R. Martin, with the assistance of Eva Paneth (New York: Interscience Publishers, 1964); and Klaus Ruthenberg, "Friedrich Adolf Paneth (1887-1958), *HYLE* 3 (1997): 103-106.

<sup>5</sup> On Polanyi as a "philosophising chemist", Mary Jo Nye, "Michael Polanyi (1891-1976)", *HYLE* 8 (2002): 123-127; and Nye, "Historical Sources of Science-as-Social Practice: Michael Polanyi's Berlin", *Historical Studies in the Physical and Biological Sciences* 37, (2007): 411-436. More generally, William Taussig Scott and Martin X. Moleski, S.J., *Michael Polanyi: Scientist and Philosopher* (Oxford: Oxford University Press, 2005).

<sup>6</sup> F. A. Paneth, "The Epistemological Status of the Chemical Concept of Element", Parts I and II, translated H. R. Paneth [Heinz Post], *British Journal for the Philosophy of Science* 13 (1962): 1-14 and 144-160. Originally in *Schriften der Königsberger Gelehrten Gesellschaft, Naturwissenschaftliche Klasse* 4 (1931). Heinz Paneth changed his last name to Post sometime around 1953. Post was Lecturer in Theoretical Physics and later Head of the Department of History and Philosophy of Science at Chelsea College in the University of London (before it merged with King's College). See Scerri, "Realism, Reduction, and the 'Intermediate Position,'" 67, note 2; and Scerri, "Editorial 1", *Foundations of Chemistry* 1 (1999): 1-5. Also, Harmke Kamminga, Steven French, and Melvin Earles, "Introduction", in *Correspondence, Invariance and Heuristics: Essays in Honour of Heinz Post*, eds. Steven French and Harmke Kamminga (Dordrecht: Kluwer Academic Publishers, 1993), xiii-xxiii.

<sup>7</sup> Ruthenberg, "Friedrich Adolf Paneth," Keith J. Laidler, *The World of Physical Chemistry* (Oxford: Oxford University Press, 1993) 265-266; and more extensively, H. J. Emeléus, "Friedrich Adolf Paneth. 1887-1958", *Biographical Memoirs of Fellows of the Royal Society* 6 (Nov., 1960): 226-246.

<sup>8</sup> See R. A. Hodgkin and Eugene P. Wigner, "Michael Polanyi, 1891-1976", *Biographical Memoirs of Fellows of the Royal Society* 23 (1977), 421-448. Also, Michael T. Scott, "Michael Polanyi's Creativity in Chemistry", in *Springs of Scientific Creativity* eds. Rutherford Aris, et al. (Minneapolis: University of Minnesota Press, 1983), 279-307; Mary Jo Nye, "At the Boundaries: Michael Polanyi's Work on Surfaces and the Solid State", in *Chemical Sciences in the Twentieth Century*, ed. Carsten Reinhardt, ed. (Berlin: Wiley-VCH, 2001), 246-257; and Mary Jo Nye, "Working Tools for Theoretical Chemistry: Polanyi, Eyring, and Debates over the 'Semi-Empirical Method,'" *Journal of Computational Chemistry* 28, #1 (15 January 2007), 98-108.

<sup>9</sup> Scott and Moleski, *Michael Polanyi*, 64, 123.

<sup>10</sup> Emeléus, "Paneth", 227, 231.

<sup>11</sup> F. A. Paneth and G. von Hevesy, "Zur Frage der isotopen Elemente", *Physikalische Zeitschrift* 15 (1914): 797-805, and Kasimir Fajans, "Zur Frage der isotopen Elemente", *Physikalische Zeitschrift* 15 (1914): 935-940. See Scerri, "Realism, Reduction, and the 'Intermediate Position,'" 65; and Paul van der Vet, "The Debate between F. A. Paneth, G. von Hevesy, and K Fajans on the Concept of Chemical Identity", *Janus: Revue Internationale de l'Histoire des Sciences de la Médecine et de la Technique* 66 (1977): 285-303, esp. 288-298. Note, in contrast to Snooks's argument against thought experiments in chemistry, Van der Vet's description on p. 295 of a thought experiment by Fajans.

<sup>12</sup> F. A. Paneth, "Ueber den Element-und Atombegriff in Chemie und Radiologie", *Zeitschrift für physikalische Chemie* 91 (1916): 171-198 and Kasimir Fajans, in *Jahrbuch der Radioaktivität und Elektronik* 24 (1917): 314-352, cited in Scott and Moleski, *Michael Polanyi*, 40. For bibliography of other relevant articles see Emeléus, 244-245. On the decision in 1923 by the International Committee on Chemical Elements, see Francis Aston et al., "Report of the International Committee on Chemical Elements", *Journal of the American Chemical Society* 45 (1932): 867-874.



- <sup>13</sup> Paneth, "Epistemological Status", 5-8 (citing Emil Fischer's *Aus meinem Leben* of 1922). Quotation on successes of chemistry, on 6.
- <sup>14</sup> Paneth, "Epistemological Status", 151-153; and, on Boyle and Spinoza, 157-158.
- <sup>15</sup> Paneth, "Epistemological Status", 155, quoting from Dmitri Mendeleev, *Principles of Chemistry* (1891), 23.
- <sup>16</sup> Paneth, "Epistemological Status", 156. On Mendeleev, also Paneth, "Chemical Elements and Primordial Matter: Mendeleeff's View and the Present Position", in *Chemistry and Beyond* ed. Dingle and Martin, 53-72, on 57-58. A translation of "Chemische Elemente und Urmaterie: Mendeleevs Ansicht und der heutige Standpunkt", in *Travaux du Congrès Jubilaire Mendéléev* Academy of Sciences of the USSR (Leningrad, 1936).
- <sup>17</sup> See Nye, "Working Tools for Theoretical Chemistry", 104-105.
- <sup>18</sup> Michael Polanyi, "The Value of the Inexact", reprinted in *Tradition and Discovery* 18, #3 (1992): 35-36.
- <sup>19</sup> Michael Polanyi, in "Discussion", *Transactions of the Faraday Society* 33: 28.
- <sup>20</sup> On Polanyi's role, Scott and Moleski, *Michael Polanyi*, 38-40. Polanyi, *Personal Knowledge*, 111.
- <sup>21</sup> Polanyi, *Personal Knowledge*, 80-81, 351-352.
- <sup>22</sup> Pierre Duhem, *Mixture and Chemical Combination and Related Essays*, trans. Paul Needham (Dordrecht: Kluwer, 2002), originally "La notion de mixte: Essai historique et critique", *Revue Philosophique* 1 (December 1900): 69-99 ; 2 (February 1901): 167-197 ; 3 (April 1901): 331-357 ; 4 (June 1901): 430-467 ; 6 (October 1901): 730-745. See Bernadette Bensaude-Vincent, "Chemistry in the French Tradition of Philosophy of Science: Duhem, Meyerson, Metzger and Bachelard", *Studies in the History and Philosophy of Science* 36 (2005): 627-648. The idea of emergent properties can be found in John Stuart Mill's discussion in the *System of Logic* of what Mill called heteropathic effects in chemical composition. See Mill's *A System of Logic, Ratiocinative and Inductive: Being a Connected View of the Principles of Evidence and the Methods of Scientific Investigation* (New York: Harper & Brothers, 1860), Book III, Chapter 6, Section 1.
- <sup>23</sup> Polanyi, *Personal Knowledge*, 393-394.
- <sup>24</sup> Ansgar Beckermann et al., eds. *Emergence or Reduction?* (Berlin: Walter de Gruyter, 1992) and Philip Clayton and Paul Davies, ed. *The Re-Emergence of Emergence* (Oxford: Oxford University Press, 2006).
- <sup>25</sup> Mario Bunge, "Is Chemistry a Branch of Physics?" *Zeitschrift für allgemeine Wissenschaftstheorie* 13 (1982): 209-223, quoted in Eric R. Scerri and Lee McIntyre, "The Case for the Philosophy of Chemistry", *Synthese* 111 (1997): 213-232, on 219.
- <sup>26</sup> Pierre Duhem, *The Aim and Structure of Physical Theory*, trans from the 2<sup>nd</sup> French edition Philip Wiener (Princeton : Princeton University Press, 1954 ; 1st publication 1906), 10, 25-27, 95-99; Pierre Duhem, "L'évolution des theories physiques de xviiiè siècle jusqu'à nos jours", *Revue des Questions Scientifiques* 40 (1896): 463-499, on 497-498; and Pierre Duhem, *Physique de croyant*, extract from *Annales de Philosophie Chrétienne* (Paris : Bloud et Cie, 1905), 31-34. See Mary Jo Nye, "The Moral Freedom of Man and the Determinism of Nature: The Catholic Synthesis of Science and History in the *Revue des Questions Scientifiques*", *British Journal for the History of Science* 9 (1976): 274-292.
- <sup>27</sup> F. A. Paneth, "The Trend of Inorganic and Physical Chemistry since 1850", in *Chemistry and Beyond*, eds. Dingle and Martin, 29-40, on 37-39. This is a paper read to the Chemistry Section of the British Association for the Advancement of Science at Edinburgh on 9 August 1951 and printed in *Advancement of Science*, 8 (1952), 397 ff.
- <sup>28</sup> On Bachelard's *Le pluralisme cohérent de la chimie moderne* (Paris : Vrin, 1932), see Bensaude-Vincent, "Chemistry in the French Tradition". 641.

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## Applied Neighbourship: Physical Methods and their Perception in Chemistry

Carsten Reinhardt\*

### Abstract

In the twentieth century, more and more chemists applied research techniques with origins in physics to their research. Seeking a contrast from their then common tools of the trade, chemists named them physical methods. This notion carried a bundle of mixed meanings, among them a perceived superiority of the “more fundamental” discipline and the attractiveness of high-technology in an age of electronics. But it also pointed to the “otherness” of physics with respect to the loss of a chemist’s identity when applying the instrumentation of the neighbouring discipline. Some perceptions of physical methods in chemistry were explored, from opposition to embracement, which ranged from attempts to strip away physical meaning to endeavours of using methods as carriers of concepts and theories. In the outcome, some chemists saw their fortunes in using the image of physics and high-technology for the distribution of research methods, building up a community of method makers that did not just cross the boundaries inside the physical sciences, but of the life sciences and medicine as well.

### References

Carsten Reinhardt, *Shifting and Rearranging. Physical Methods and the Transformation of Modern Chemistry*. Sagamore Beach, Mass.: Science History Publications, 2006.

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## **‘A Subversive Element’: Science, Politics and the Early Appropriation of Radioactivity in Spain**

*Néstor Herran\**

In January 1904, two months after being awarded the Nobel Prize, Pierre Curie wrote to his collaborator Georges Gouy: “you have seen this sudden excitement about radium. We enjoyed the advantage of some popularity (...) perhaps this noise is not going to be useless, and it helps me to get a professorship and a laboratory”.<sup>1</sup> Despite its ironical tone, this passage reveals something about the intricate relationship between research and its public representation. It is not unreasonable to think that public interest, aroused by radioactivity’s potential medical and industrial applications and by its supposed opposition to traditional conceptions of matter could have played an important role in the extension of this science. However, the historiography of radioactivity has traditionally ignored the public dimension of this science, and yet there is not a global narrative about the history of its public image. Herein it is intended to examine this quite unexplored dimension of the early history of radioactivity, considering that the appropriation of radioactivity research in Spain was intimately related with hegemonic public and scientific discourses.<sup>2</sup> By exploring public image of radioactivity in the press and popular science books, and following research careers of main representatives of this science in Spain, it is suggested that these discourses not only determined the establishment of research lines, but also shaped the character of radioactivity research in Spain.

### **Radioactivity and its public: the emergence of a new science**

Radioactivity was born in Paris, in a period –the *fin de siècle*– characterised by proliferation of new radiations like the Cathode rays, X-rays, N-rays, Moser rays, Lyman rays or Selenic rays.<sup>3</sup> These new entities received considerable attention not only in scientific journals, but also in the popular science magazines and the press. X-rays were the most fashionable, as they provided a window to the inside of the body. In contrast, radioactivity had a more nuanced reception. The experi-

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ments performed by Henri Becquerel in 1896 on the radiation of uranium appeared in academic journals and were replicated by scientists like William Thompson, C.T.R. Wilson, Gustave LeBon or Adolf Miethe, but they did not raise important public awareness. Differently from X-rays, which revealed invisible structures of human body, “uranic rays” were difficult to detect and did not exhibit any spectacular features. The phenomenon did not get notoriety until Pierre and Marie Curie used it to discover new elements such as polonium and radium, coined the term “radioactivity” and linked the production of radioactive elements with potential medical and industrial applications.<sup>4</sup>

Even if the first International Congress of Physics, held in Paris in 1900, devoted two of its five plenary lectures to radioactivity,<sup>5</sup> radioactivity did not become popular until late 1903, after the award of the Nobel prize to Becquerel and the Curies. The Curies, a young and unconventional couple, were transformed in the heroes of French science, and Pierre Curie’s desire of obtaining a chair and a laboratory was accomplished the same year. Public notoriety helped to consolidate research lines and to promote the incipient radium industry.<sup>6</sup> In January 1904, the industrialist Armet de Lisle, who had provided the Curies with facilities for the production of radium, opened new industrial installations for producing this element and founded the journal *Le Radium*, exclusively devoted to this new element. Directed by engineer Henri Farjas, *Le Radium* initially aimed to be an “instrument of popularisation and research”. Indeed, Farjas also established a collection of books, the “Librairie du Radium et de la Radioactivité”. Although *Le Radium* soon became a strictly academic journal after six months, Farjas kept his editorial work for the promotion of the emerging radium industry, and combined it with participation in one of the first companies producing radium, “La Banque du Radium”.<sup>7</sup>

The establishment of radioactivity as a science and an industry was intimately tied to an increase of its public visibility. Together with academic books published by Henri Becquerel or Marie Curie,<sup>8</sup> there were many popularisation books directed to broad audiences, such as, *Les applications médicales du radium*, written by Foveau de Courmelles, or the best-seller *Le radium et les nouvelles radiations*, by Alphonse Berget.<sup>9</sup> Berget’s book was a quite comprehensive account of contemporary knowledge about radioactivity, which included an historical introduction about this area of research, a summary of current theories about the origin of radioactivity, and chapters about the properties of radium, the different kinds of radiation, emanation,<sup>10</sup> the applications of radium and its effects on living beings. This book can also be considered as pioneer contribution to the history of radioactivity and, according Gustave LeBon, had an important role in the retrospective construction of Becquerel as radioactivity’s discoverer.<sup>11</sup>

Radium's celebrity soon spread out of France. In the United States, Saint Louis Universal Exhibition included an exhibition about radium and a series of conferences on the new element that spurred on public interest.<sup>12</sup> William Hammer, a former Edison's chief engineer, became the main populariser of radioactivity in America, holding a conference tour around the country in which he exhibited radium samples acquired in Paris. Inventor of radium-based paint, his book *Radium and Other Radioactive Substances* was one of the first popularisation books on radioactivity.<sup>13</sup> In the United Kingdom, radium was also prominent in the public sphere by late 1903. A keyword search of "radium" or "radioactivity" in *The Times* reveals the publication of an average of almost 80 articles per year between 1903 and 1906, that is, more than an article per week.<sup>14</sup> In the UK, we should also take into account popularisation efforts made by William Crookes, William Ramsay and Ernest Rutherford, who in 1904 published *Radio-activity*, the most influential book on radioactivity in the Anglo-Saxon world.<sup>15</sup> His collaborator, Frederick Soddy, published the same year *Radio-Activity: An Elementary Treatise, from the Standpoint of the Disintegration Theory* with minor success.<sup>16</sup> Together with these books, there were also more popular books on the subject, such as Selimo Bottone's *Radium, and All About It* or Hampson's *Radium Explained. A Popular Account of the Relations of Radium to the Natural World, to Scientific Thought, and to Human Life*.<sup>17</sup>

In the German speaking world, expert and lay accounts also coexisted. As early as 1902, the industrial chemist Friedrich Giesel published in Germany *Ueber radioaktive Substanzen und deren Strahlen*; in Switzerland, Paul Gruner published in 1906 *Die Radioaktiven Substanzen und die Theorie des Atomzerfalles*; in Austria Hans Mayer's, *Die Neuren Strahlungen. Kathoden-, Kanal, Roentgen-Strahlen und die radioaktive Selbststrahlung*, 1904. Similar movements took place in Denmark, where Kristine Meyer published in 1904 *Radium og Radioaktiver Stoffer samt nyere Opdagelser angaaende Straaler*.<sup>18</sup>

This literature, which shaped appropriations of radioactivity in different national contexts will not be discussed in detail. However, this short review points out the rapid propagation of radioactivity in the European public sphere, which was possibly related to the consolidation and institutionalisation of the discipline. The following sections will deal with these processes in more detail by focusing in the Spanish case, which reveals how a particular configuration of audiences shaped the development of radioactivity within this local context.

## Muñoz del Castillo, Echegaray and the appropriation of radioactivity in Spain

In his study of the development of radioactivity in America, Lawrence Badash argued that chemists investigating radioactivity found more support in the community of physicists.<sup>19</sup> By studying the placement of radioactivity laboratories in academic structures, he showed that most of them were attached to physics departments, and that those in chemistry departments suffered from isolation. This is the case, for example, of Otto Hahn's laboratory. It was established in the Emil Fisher Institute at the University of Berlin, but his colleagues, organic chemists, considered his work with "no direct connection... [to] normal chemistry". The careers of Frederick Soddy, Kazimir Fajans, Georg Hevesy and Marie Curie suggest isolation from the profession of chemistry and a struggle for support. The hybrid character of radioactivity, which gathered theoretical and material resources from both physics and chemistry, was contested by traditional academic structures. Some chemists resisted to accept radioactivity claims in the first decade of the 20th century, in part because transmutation challenged the deep-rooted notion of the stability of elements. In some extent, the use of physical means of analysis to establish the existence of radioactive elements was a serious challenge to established chemical practice.<sup>20</sup>

In the case of Spain, this kind of tensions between radioactivity and chemistry were very rare. A reason for this is that the leader of radioactive research in Spain was José Muñoz del Castillo, a well established academic chemist, Professor of "chemical mechanics" in the University of Madrid. A second reason is that Madrid Laboratory's research line focused on the measuring of radioactivity in mineral waters, soils and fertilisers, which led to a particular appropriation of radioactivity which was at odds with the path followed in other countries. These practices relied and were strongly connected to conventional kind of analyses, such as general analysis of waters and minerals. Last, but not least, Muñoz's theoretical approach to radioactivity was quite unconventional. He refused accepting Rutherford and Soddy's theory of atomic disintegration as explanation of radioactive phenomena. Instead, he regarded radioactivity as a sort of chemical reaction, similar to the capture of hydrogen by metals like platinum. I have previously argued elsewhere that Muñoz's success in shaping Spanish radioactive research was much related to the particular configuration of Spanish academia and patronage, to lack of a radio-elements industry in Spain and other contingent factors.<sup>21</sup> Here, I argue that the root of Muñoz's view of radioactivity can be attached to the configuration of public scientific debate in Spain in the first decade of the twentieth century.

In this study of the early appropriation of radioactivity in Spain, it is considered that Madrid's Royal Academy of Science had an important role, as forum for communicating research results, and its President a representative of hegemonic scientific thinking in Spain. Established in 1847, the Royal Academy gathered the most distinguished members of the scientific community and served as links with foreign scientific communities, as a place to communicate scientific results and as an *agora* to debate the country's scientific policy.<sup>22</sup> Being the president of Madrid's Academy, the leading Spanish organic intellectual was José Echegaray (1832-1916).<sup>23</sup> Trained as engineer and member of the Academy since 1864, Echegaray had played an important role in the institutionalisation of mathematical physics in Spain. Combining a scientific prestige (being one of the main actors in the introduction of thermodynamics and modern mathematics in Spain)<sup>24</sup> with a successful career as a playwright (which served to get the Nobel Prize in literature in 1904) and with a notorious political involvement (being minister in several governments)<sup>25</sup>, Echegaray exerted an important influence in the Spanish academic system. In this sense, he not only acted as the President of Madrid's Academy, but also was the first President of the Spanish Society of Physics and Chemistry (established in 1903), of the Spanish Association for the Progress of Science (established in 1908) and of Spanish Mathematical Society (established in 1908). From these positions of power, he tried not only to consolidate the institutional position of Spanish physics and chemistry, but also to spread his own scientific worldviews.

His perspective, which we can call "Echegarian paradigm", was an adaptation of Laplacian views of science, which was based on three elements: the use of a very abstract kind of mathematical physics as the fundamental explanatory device, the idea that all physical and chemical phenomena could be explained as manifestation of a unified Newtonian force, and the idea that this force could explain from cosmic evolution to the nature of chemical elements. As will be seen, this paradigm shaped Spanish appropriation of new physical and chemical theories like radioactivity, by means of its influence on José Muñoz del Castillo's perspective. Indeed, that these influences were shaped not only by means of conventional academic influence, but taking into account the result of debates in the public sphere, where political ideas crucially intervened.

In order to assess how the hegemony of Echegaray's image of radioactivity was exerted, it is helpful to examine the depiction of radioactivity in the press and in popularisation books written in Spain. Consider, to begin with, Echegaray's own production. He was a very prolific author, who published more than fifty books (without counting his literary production) and hundreds of articles in magazines and newspapers.<sup>26</sup> In his "popularisation" of the discovery of the radioactivity,

Echegaray played with the reaction of surprise and even scandal regarding the new French discoveries, and established a vivid political analogy:

“The ‘radium’ appears to the science as a revolutionary metal, like an anarchist that comes to disturb the order established and to destroy all or most of the laws of the classical Science.”<sup>27</sup>

Faced with this revolution, which seemed to demolish established physical theories, Echegaray asked for prudence, explaining these radiation features by means of most universally accepted theories. This is the purpose of his article “The energies of radium”, appeared in *El Imparcial* in 1903. According to Echegaray, the new phenomenon should not alarm anyone, as it

“complies with the existing law (...) which come into the world of science while respecting its order, as new factories and new industries come to the society without seeking annihilation, but only to increase employment and wealth.”<sup>28</sup>

After saying this, Echegaray sorted out two problematic questions raised by the new phenomena: the “endless production of energy” and the origin of radioactivity. In dealing with the them, Echegaray proposed two different explanations to save the “established order”: the hypothesis proposed by Marie and Pierre Curie, which set out radium’s capacity to produce light and heat from an invisible energy permeating the space, and the planetary atomic hypothesis, which considered atoms as stable systems and radium as a setting-up system, which emitted particles because of its instability. By means of these hypotheses, Echegaray safeguarded the principle of energy conservation, which was the basis of his thermodynamics and concluded that “phenomena caused by radium do not break any important law of physics”. In later popular articles on radioactivity, Echegaray got back to the dichotomy between order and chaos, and to the analogies with the anarchist movement, pointing out his opposition to modern physics. For instance, in an article published in the *Revista de Obras Pública* in 1910, Echegaray still considered that new interpretations of radioactive phenomena reflected the transition from a “individualist” science to a “socialist” science, and that in physical sciences, “the atom is could be destroyed, as the individual is in social sciences”.<sup>29</sup>

Echegaray was not alone in considering as subversive the interpretation of radioactivity as atomic disintegration. A similar opinion can be found in the section “Scientific Notes” that Josep Comas y Solá published in conservative newspaper *La Vanguardia* since 1896.<sup>30</sup> A retrospective article written by Comas in 1907 about the “crisis of matter”, showed his discontent with recent paradigm changes, which he described in the following terms:

“Atomic structure, light, heat, magnetism, electricity, gravity, and life and soul themselves were transparent to us. We had turned the whole universe into a bil-

liard table [...], we climbed to the top of science leaning on our mathematical cue.”<sup>31</sup>

According to Comas, Maxwell contributions in the previous century had laid the foundation of this paradigm shift, but X-rays and radioactivity had given the *coup de grâce* to the old worldview and had opened a period of theoretical confusion:

“Behind this avalanche of new phenomena, which seem to conspire with the aim of demolish all we believed to know, we found the real chaos of a new aimless hypothesis appear, a chaos that does not show consideration for most common sense principles, neither for doctrines we considered as invulnerable.”<sup>32</sup>

These attitudes seem to act in response to a menacing environment for traditional scientific worldviews. What was at the origin of this menace? The atomic disintegration theory could be innovative and scientifically challenging in some aspects, but relating them to revolutionary upheaval seem exaggerated. To make sense of these reactions we need to take into account other alternative contemporary readings of radioactivity, particularly the appropriations of these findings in the socialist milieu.

### **Revolutionary readings of radioactivity in the socialist press**

Treatment of science news was widespread in socialist press, as science was traditionally considered by this ideology as a liberating tool and as antidote against material and intellectual oppression by capitalist system, the state and the church. Journals and leaflets of worker's movement informed of latest scientific developments, reflected about their social implications and served as an alternative to hegemonic worldviews. References to radioactivity abound in this literature, and also in works of prominent members of the socialist movement like Paul Lafarge, Karl Marx's son in law, who commented on radioactivity discovery in the following terms:

“recent discovery of radioactivity breaks down the fundamental laws of mathematical physics, destroys the atomic basis of chemical structure. We cannot found a better example to illustrate the sterility of spoken discourse and of the fertility of experience.”<sup>33</sup>

In Spain, a good example of these appropriations can be found in *La Revista Blanca. Sociología, ciencia y arte*.<sup>34</sup> Established in 1898, it can be considered as one of the most important socialist magazines because of its quality and widespread circulation. With a bi-weekly periodicity, it originally counted with sixteen pages without illustrations. Since 1902 onwards, its size increased to thirty-two



pages with illustrations and its readership peaked at 8000, which is a very important circulation for the period. *La Revista Blanca* defined itself as “positivist and anticlerical”, and was much inspired by its French counterpart *La Revue Blanche*, which regularly lent illustrations and articles. Divided in three sections (“Sociology”, “Science and Art” and a “General section”), scientific content was in charge of engineer Fernando Tárrida de Mármol, who elaborated periodical chronicles of current scientific and technological news.<sup>35</sup>

Tárrida de Mármol covered a great diversity of subjects, in the style of short notes, one or two pages long. He showed special interest for extraordinary scientific news and cosmological speculation, generally bringing into account thermodynamic scatology.<sup>36</sup> His section dealt several times with radioactivity, stressing that this phenomena shacked the traditional idea of the stability of atoms, and emphasizing its revolutionary character. According to Tárrida,

“atomic theory, which is still so young and successful, is also going to disappear, as it is energetically challenged by inflexible facts that doesn’t honour beliefs, traditions or theories.”<sup>37</sup>

Indeed, radioactivity could be the key for a new interpretation of nature:

“What a new field of study! It seems that we are in front of a very peculiar kind of matter. Are we leading towards recognising the unity of matter? This radioactive propriety, is only particular of a number of bodies? Is the living cell a result of these phenomena? [...] Does inert matter has a sort of life, sending to the space all kinds of emanations, some in form of light, other modifying the medium and operating on the living beings, causing sensations? Then, all psychical phenomena would be real, a purely dynamical phenomenon... How many ideas can suggest a few centigrams of matter”<sup>38</sup>

In his chronicles, Tárrida considered radioactivity as a crucial step towards the establishment of a truly materialistic theory of nature. This position, along the lines of Buchner’s materialism, appears explicitly in a later article, where he points out that radioactivity could explain phenomena like “mental suggestion” and “telepathy” from a physical, not spiritual basis:

“The day will arrive when we will see the fall of all these systems that attribute supernatural causes to the most important phenomena in nature, mostly those concerning human nature”<sup>39</sup>

Tárrida de Mármol was also interested in applications of the new science. His column included references to the application of radium in the production of light, radiography and the treatment of cancer.<sup>40</sup> However, the high price of radium



could limit the widespread use of this element in medicine, and gave him ground for social critique:

“It is evident that a so scarce pharmaceutical like radium is not going to be used by poor people who, after a life of hard work, could not afford a single atom of radium. This pharmaceutical would only cure millionaires or even richer people. The poor, in relation to these remedies, are only to be used like dogs and guinea pigs, that is, as a matter for experiments.”<sup>41</sup>

Subversive readings of radioactivity in the socialist press are the counterpoint of Echegaray's politically and scientifically conservative position. Accordingly, it is reasonable to think that reaction to new physical interpretations of radioactivity could be somewhat related to containment of its subversive readings by the Spanish scientific establishment. A way to evaluate this claim is by reading the first popularisation books on radioactivity published in Spain, in which we can appreciate a thorough limitation of radical interpretations of the new phenomena.

A good example of this is *El Radio y las nuevas radiaciones*, the Spanish translation of Berget's *Le Radium et les nouvelles radiations* by Eduardo Navarro Beltran del Río, civil engineer and Professor of Electrical Engineering at Bilbao Industrial Engineering School.<sup>42</sup> In the original edition, Berget compared the discovery of radioactivity with the discovery of America, and emphasised revolutionary aspects of radioactivity such as the endless emission of energy. This kind of readings were attenuated in the Spanish version, which included an appendix written by Navarro praising the laws of conservation of matter and energy. Navarro justified his interference by claiming that “we feel the philosophical necessity of these laws and, when considering that these hypotheses could disappear, the ghost of chaos would raise from the very deep of our body, which is matter and energy”. Indeed, Navarro adopted Echegaray's hypothesis to explain the origin of radioactivity, considering that radium was a condensation of helium and that emanation was a condensation of ozone, in the context of an evolutionist theory of elements.

Another example of cautious appropriation of radioactivity is another book published in 1904 by Ramón Pomés y Soler, *El Radium y la Radiografía*.<sup>43</sup> Pomés considered radium as a “matter of huge importance” and pointed out that his treatment of the issue would be restricted to explain the facts “experimented and described by Röntgen, Becquerel, the Curies, Rutherford, Geitel and other eminent scholars”, without adding anything himself. However, when he reached the controversial point of radioactivity origin, he omitted mention to Rutherford's and Curie's hypothesis, and simply took into account traditional chemical views of the atom.

## Conclusion

The images portrayed in the press and in the popularisation literature in Spain, together with the lack of alternative interpretations outside the radical press, suggests that the Spanish scientific community showed a cautious attitude in relation to the different interpretations about the origin of radioactivity. This attitude prevailed in the first decade after the discovery of radioactive phenomena, and can be related to a widespread conservative approach among Spanish scientists, who could have endangered their efforts to institutionalise science if they had embraced radical readings of radioactivity. This conservative attitude to the new physics in the Spanish academia was the context in which this new discipline was appropriated, and shaped the approach of its main representative in Spain, chemist José Muñoz del Castillo. His closeness to the Echegarían paradigm, together with other contingent personal and institutional factors, can explain the divergent path of Spanish radioactivity in relation to the practice of this science in other countries, both in the theoretical and in the practical aspects.

## Notes

<sup>1</sup> Pierre Curie to G. Gouy letter, January 22, 1904. Reproduced in Soraya Boudia, *Marie Curie et son laboratoire. sciences et industrie de la radioactivité en France* (Paris, Editions des archives contemporaines, 2001), 81-83.

<sup>2</sup> In this article the term “appropriation” is used instead of “reception” in dealing with the processes of translation and communication of science, as suggested by Kostas Gavroglu, *The Sciences in the European Periphery During the Enlightenment* (Dordrecht: Kluwer Academic Publishers, 1999).

<sup>3</sup> On the *fin de siècle* physics, see John L. Heilbron, “Fin de Siècle Physics”, in *Science, Technology and Society in the time of Alfred Nobel*, ed. Carl Gustaf Benhard et al. (Oxford, Pergamon Press, 1982), 51-73. The discovery of “new radiations” circa 1900, has been analysed in Helge Kragh, “The New Rays and the Failed Anti-Materialistic Revolution”, in *The Emergence of Modern Physics*, ed. Dieter Hoffmann et al. (Pavia, Università degli Studi di Pavia, 1995), 61-78.

<sup>4</sup> Nuanced reception of uranic rays is coherent with recent historical research, which plays down Becquerel’s role in the discovery of radioactivity. According to Boudia, *Marie Curie et son laboratoire*, 37-39, Becquerel’s main role fundamentally reflected his important position as prominent member of the French scientific establishment. Boudia claims that the discovery of radioactivity was not a punctual and isolated event, but we should alternatively consider the gradual emergence of a new area of study, with participation of different actors and closure of different controversies in several spaces. On the construction of Becquerel’s image as discoverer of radioactivity, see Paul Fournier & Josette Fournier, “Hasard ou mémoire dans la découverte de la radioactivité?”, *Revue d’histoire des sciences* 52 (1999): 51-79.

<sup>5</sup> The congress was held in August, 6-12, 1900 as part of Paris Universal Exhibition on radioactivity were read by Becquerel and the Curies.

<sup>6</sup> Xavier Roqué, “Marie Curie and the Radium Industry: a Preliminary Sketch”, *History and Technology* 13 (1997): 267-281; Boudia, *Marie Curie et son laboratoire*.

<sup>7</sup> It is interesting to trace similarities between the careers of Farjas and Jacques Danne, his successor in the journal direction. As well as Farjas, Danne was involved in radium production since

1904. In 1911, he established his own radium producing company, and also a laboratory for certifying radioactive products: the Laboratoire d'Essais des Substances Radioactives.

<sup>8</sup> Despite Marie Curie's dissertation was conceived as an academic work, it was soon translated and edited into English and German: Marie Curie, *Radio-Active Substances. Thesis presented to the Faculté des Sciences de Paris* (London: Chemical News Office, 1904); Marie Curie, *Untersuchungen ueber die Radioactiven Substanzen* (Braunschweig, Vieweg, 1904). Another example of early interest on radioactivity in the Anglo-Saxon world is the edition of Becquerel's works: Henri Becquerel, *On the Radio-Activity of Matter* (Washington, USGPO, 1903).

<sup>9</sup> Foveau de Courmelles, *Les applications médicales du radium* (Paris, 1904). Alphonse Berget, *Le radium et les nouvelles radiations: que faut-il en penser? Que faut-il en attendre?* (Paris, G. Charpentier, 1904).

<sup>10</sup> Emanation was the first name given to radon, a radioactive by-product of radium disintegration.

<sup>11</sup> Gustave LeBon, *La evolución de la materia* (Madrid, Librería Gutemberg de José Ruíz, 1911), p. 379.

<sup>12</sup> Lawrence Badash, *Radioactivity in America. Growth and Decay of a Science* (Baltimore, The John Hopkins University Press, 1979).

<sup>13</sup> William J. Hammer, *Radium and Other Radio-Active Substances: Polonium, Actinium and Thorium, with a Consideration of Phosphorescent and Fluorescent Substances, the Properties and Application of Selenium and the Treatment of Disease by the Ultra-Violet Rays* (New York, Van Nostrand, 1903).

<sup>14</sup> Keyword search has been performed using *The Times Digital Archive*.

<sup>15</sup> Ernest Rutherford, *Radio-Activity* (Cambridge, Cambridge University Press, 1905). In 1905, Rutherford published an enlarged edition of this book, which was translated into German in 1907: Ernest Rutherford, *Die Radioaktivitaet. Unter Mitwirkung des Verfassers ergaentzte autorisierte deutsche Ausgabe* (Berlin, Springer, 1907).

<sup>16</sup> Frederick Soddy, *Radio-Activity: An Elementary Treatise, from the Standpoint of the Disintegration Theory* (London, The Electrician, 1904). Nonetheless, this book was translated into German the same year: Frederick Soddy, *Die Radioaktivitaet vom Standpunkt der Desaggregationstheorie elementar dargestellt. Unter Mitwirkung von Dr. L.F. Guttman. Uebersetzt von Prof. G. Siebert* (Leipzig, Barth, 1904a). Soddy also published a short piece on radium with a more popular tone: *The Interpretation of Radium. Being the Substance of Six Free Popular Experimental Lectures Delivered at the University of Glasgow* (Soddy, 1909).

<sup>17</sup> Selimo Bottone, *Radium, and All About It* (London, New York, Whittaker & Co., 1904). W. Hampson, *Radium Explained. A Popular Account of the Relations of Radium to the Natural World, to Scientific Thought, and to Human Life*, (London, Jack, 1905).

<sup>18</sup> Friedrich O. Giesel, *Ueber radioaktive Substanzen und deren Strahlen* (Stuttgart, 1902). Paul Gruner, *Die Radioaktiven Substanzen und die Theorie des Atomzerfalles* (Bern, A. Francke, 1906). Hans Mayer, *Die Neuren Strahlungen. Kathoden-, Kanal, Roentgen-Strahlen und die radioaktive Selbststrahlung*. (Maehr.-Ostrau, R. Papauschek, 1904). Kristine Meyer, *Radium og Radioaktiver Stoffer samt nyere Opdagelser angaaende Straaler*. (Kobenhavn, Gyldendalske Boghandel, 1904).

<sup>19</sup> Lawrence Badash, *Radioactivity in America*.

<sup>20</sup> Xavier Roqué, "From Radiochemistry to Nuclear Chemistry and Cosmochemistry", in *Bridging Boundaries. Chemical Sciences in the Twentieth Century*, ed. Carsten Reinhardt (Weinheim, Wiley-VCH, 2000), 49-65.

<sup>21</sup> Néstor Herran, *Aguas, semillas y radiaciones. El laboratorio de radiactividad de la Universidad de Madrid, 1904-1929* (Madrid, CSIC, 2008). A summary of this book in English has been published as Néstor Herran, "Waters, Seeds and Radiation: Radioactivity Research in Early

Twentieth Century Spain”, in ed. Josep Simon et al., *Beyond Borders: Fresh Perspectives in History of Science* (Cambridge, Cambridge Scholars Publishing, 2008), 325-344.

<sup>22</sup> In order to perform these functions, Academies counted with journals. Reception ceremonies for new members also played an important role as political events.

<sup>23</sup> Echegaray was president of Madrid’s Academy in the period 1894-1896, and again between 1901-1916.

<sup>24</sup> On Echegaray’s role in the introduction of thermodynamics in Spain, see Stefan Pohl, *La “circulación” de la energía: Una historia cultural de la termodinámica en la España de la segunda mitad del siglo XIX* (Universitat Autònoma de Barcelona, Unpublished PhD dissertation, 2007). For a short review of Echegaray’s science, see José Manuel Sánchez Ron, ed., *José Echegaray* (Madrid, Fundación Banco Exterior, 1990).

<sup>25</sup> Echegaray political views are typical of the period of Spanish monarchic restoration: initially republican and liberal, he then evolved towards conservatism.

<sup>26</sup> Part of Echegaray’s popularisation literature was reprinted in books such as José Echegaray, *Ciencia Popular. Colección de artículos publicados en los periódicos El Imparcial y El Liberal* (Madrid, Hijos de J. A. García, 1905) and José Echegaray, *Vulgarización científica* (Madrid, Imprenta de Rafael Gutiérrez Jiménez, 1910).

<sup>27</sup> José Echegaray, *Vulgarización científica*, 79-90.

<sup>28</sup> José Echegaray, *Vulgarización científica*, 79-90.

<sup>29</sup> José Echegaray, “La evolución actual de las ciencias”, *Revista de Obras Públicas* 58(1910): 467-479.

<sup>30</sup> On Comas y Solà, see Xavier Barca, “Josep Comas i Solà, Barcelona, 1868, Barcelona, 1937. L’Astronomia de posició”, in ed. J. M. Camarasa & Antoni Roca Rosell, *Ciència i Tècnica als Països Catalans. Una aproximació biogràfica*. (Barcelona, Fundació Catalana per a la Recerca, 1995), 793-825. A selection of his Scientific Notes were published as a book: *José Comas y Solà, Astronomía y ciencia general. Colección de trabajos científicos de popularización referentes a la astronomía, a la sismología, a la historia de las ciencias en el siglo XIX, etc.* (Barcelona, F. Granada, 1907).

<sup>31</sup> José Comas y Solà, “La crisis de la materia”, *La Vanguardia*, March, 28th, 1907, 7-8. Comas y Solà note followed a previous article titled “Science in crisis”, where he tried to warn about “our most classical and well established principles in physical and mathematical sciences are in imminent danger of reaching the point of collapse”.

<sup>32</sup> José Comas y Solà, “La crisis de la materia”, *La Vanguardia*, March 28th, 1907, 8.

<sup>33</sup> Paul Lafargue, *Karl Marx’ Historischer Materialismus*, 1903.

<sup>34</sup> On *La Revista Blanca*, see María Cruz Seoane & María Dolores Sáiz, *Historia del periodismo español. Vol. 3. El siglo XX*. (Madrid, Alianza Universidad, 1997).

<sup>35</sup> Tárrida de Mármol (1861-1915) was son of a wealthy family of Cuban-born Catalan industrialists. He studied engineering in Barcelona, Toulouse and Madrid. In Barcelona, he worked as a teacher and director of the Polytechnic Academy of Barcelona until his incarceration after Montjuich trials in 1896 and his escape to France this same year.

<sup>36</sup> On the ideological and religious readings of thermodynamics, see Helge Kragh, *Matter and Spirit in the Universe. Scientific and Religious Preludes to Modern Cosmology* (London, Imperial College Press, 2004).

<sup>37</sup> Tárrida de Mármol, “Crónica Científica”, September 1st, 1901.

<sup>38</sup> Tárrida de Mármol, “Crónica Científica”, September 1st, 1901.

<sup>39</sup> Tárrida de Mármol, “Crónica Científica”, August 1st, 1902.

<sup>40</sup> Tárrida de Mármol, “Crónica Científica”, August 15th, 1903.

<sup>41</sup> Tárrida de Mármol, “Crónica Científica”, August 15th, 1903.

<sup>42</sup> Alphonse Berget, *El radio y las nuevas radiaciones* (Madrid, Bailly-Bailliere, 1904).

<sup>43</sup> Ramón Pomés y Soler, *El Radium y la Radiografía* (Barcelona, Rovira y Chiqués, 1904).

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# Deuterium as a Probe of the Boundaries between Physics, Chemistry and Biochemistry

Stephen J. Weininger\*

Radioisotopes were the first type of isotope to be found in the early 20<sup>th</sup> century, and ever since they have attracted the lion's share of attention from both scientists and lay people. This includes historians of science, who, in comparison, have had comparatively little to say about the significance of *stable* isotopes.<sup>1</sup> A notable exception has been Robert Kohler, who examined the impact of deuterium and other stable isotopes on the investigation of intermediary metabolism.<sup>2</sup> Kohler's narrative integrates the numerous institutional, social, economic and scientific factors that shaped this development, but doesn't discuss the actual techniques of isotope detection and measurement, a topic that has been neglected in the historical literature. The aim in this paper is to argue that the subject has something to tell us about disciplinary boundaries and about their permeability.

## The Importance of Deuterium

The study of experimental techniques is as important as the investigation of conceptual content for tracing the evolution of fault lines between disciplines.<sup>3</sup> The early history of deuterium provides an illustrative case study. Discovered in late 1931, this heavy isotope of hydrogen ( $^2\text{H}_1$  or D) was immediately an object of intense interest on the part of both chemists and physicists, and shortly thereafter it proved to be a uniquely powerful tool in biochemistry.<sup>4</sup> However, the experimental and theoretical techniques used to manipulate this nuclide, and the uses to which it was put, varied markedly in ways that initially reinforced but later undermined the distinctions between disciplines.

The detection of deuterium by H. C. Urey<sup>5</sup> and his colleagues<sup>6</sup> was received in the chemical community with great fanfare. *Industrial and Engineering Chemistry* asserted, in its lead editorial for 1934, "The importance of the discovery and pre-

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paration of the isotope of hydrogen, named ‘deuterium’, may be far greater than that of most elements. It seems certain that in years to come it will be ranked among the great discoveries in science. It is the starting point in developing a far-reaching new field in chemistry”.<sup>7</sup> It is not difficult to understand the enthusiasm that greeted this new arrival. Most of the then known isotopes belonged to elements that played little or no role in most chemists’ work, especially that of organic chemists and biochemists. Hydrogen, on the other hand, is ubiquitous in the compounds dealt with by these scientists. So the substitution of deuterium for the light hydrogen isotope, protium ( $^1\text{H}_1$  or H), in organic compounds was expected to open an entirely new window on chemical properties and processes. Indeed, the large relative mass difference between the hydrogen isotopes (100%) led G. N. Lewis to assert that “the isotope of hydrogen is, beyond all others, of interest to chemists”. He predicted that “it will be so different from common hydrogen that it will be regarded almost as a separate element”, and noted that “If this is true, the organic chemistry of compounds containing the heavy isotope of hydrogen will be a fascinating study”.<sup>8</sup> Lewis was not alone in placing the discovery of deuterium among the most significant contemporary achievements in science; the Nobel Committee awarded Urey the Chemistry Prize in 1934, less than three years after his first publication on deuterium.

### **The Detection and Determination of Deuterium**

Using emission spectroscopy, Urey had detected the heavy isotope in a sample of liquid hydrogen that had been concentrated by low temperature distillation. While this approach sufficed to confirm the presence of deuterium, it couldn’t give an accurate estimate of its concentration. For that, Urey measured the refractive index of a sample of water obtained by oxidising a portion of the deuterium-containing sample.<sup>9</sup> As will be seen, the chemists and physicists who scrambled to exploit Urey’s discovery divided according to their analytical techniques and objects of study; significantly, Urey himself published his spectroscopic results in the *Physical Review* and his refractive index work in the *Journal of the American Chemical Society*.

Urey’s refractive index article was followed closely by G. N. Lewis’s piece on the same topic.<sup>10</sup> Immediately after Urey’s discovery of deuterium, Lewis had initiated a vigorous research program centered on the isolation and study of this isotope.<sup>11</sup> Among chemists, the principal approach to quantifying the amount of deuterium present in a substance was to oxidise the sample completely, thereby converting all of the hydrogen and deuterium to isotopically enriched water, and then

measuring its density. Just a few months after Urey's first paper appeared, Polanyi and Gilfillan described a micropycnometer that could measure densities of samples as small as  $10^{-5}$  mg.<sup>12</sup> With larger samples, densities could be determined to one ppm or less using the falling drop method, in which a drop of the sample is allowed to fall through a liquid of nearly identical density, the rate of fall being proportional to the deuterium content. As noted, the alternative was to measure the liquid's refractive index; by combining the data from the two measurements it was possible to calculate the content of both  $^{18}\text{O}$  and D.<sup>13</sup> As far as the conversion of a sample's hydrogen quantitatively to water was concerned, chemists had over a century's experience with combustion analysis, while accurate measurements of densities and refractive indices were also well established procedures in the chemists' arsenal.<sup>14</sup> Thus, the incorporation of deuterium, with all its investigative potential, into chemical practice was rendered almost painless by the fact that it could be accomplished using methods so familiar that many were routinely taught to students. Furthermore, these methods had theoretical as well as practical significance. For example, by treating deuterium as a distinct chemical species, its behavior could be brought under the aegis of equilibrium thermodynamics, an area in which Lewis was an acknowledged master.<sup>15</sup>

Lewis was also working on the separation of lithium isotopes, and his former graduate student, Jacob Bigeleisen, said of this project that Lewis and his co-worker used "nothing but simple chemistry for the enrichment process, the determination of the relative atomic weights for analytical purposes, a mastery of chemistry, and hard work".<sup>16</sup> Much the same could have been said of his experiments with deuterium. Lewis's commitment to pursuing his deuterium work within a chemical framework is captured clearly in another observation by Bigeleisen:

"No molecular theory of the condensed phase isotope effect existed at that time [1934]. In fact, it was the absence of such a theory... that made these studies interesting to Lewis. In his Madrid lecture Lewis states, '...it is often these very cases which defy the analysis of mathematical physics that are of the most interest to chemists'".<sup>17</sup>

It is likely that Lewis's chemical contemporaries shared these sentiments as fully as they embraced his techniques.

The natural abundance of deuterium in hydrogen is only 0.2 atom-percent, insufficient for detection with the techniques available in the 1930s. Enrichment was therefore mandatory. Urey's coworkers initially accomplished this by distilling six liters of liquid hydrogen near its triple point ( $\sim 14$  K) and then examining the residual few mL, but this was not a very suitable process for isolating large quanti-



ties of the pure isotope. For this, Urey, Lewis and others chose electrolysis of water, another technique with a long history in chemistry.<sup>18</sup>

No matter what the origin of the deuterium, chemists overwhelmingly chose to monitor it via the density and/or refractive index of the derived water. What other parameters might they have availed themselves of? The measurement of nuclear magnetic moments was experimentally quite difficult and not then feasible for condensed phases.<sup>19</sup> Spectroscopic studies of deuterium were virtually all carried out in the gas phase and focused on the energy levels of atomic and molecular hydrogen (H, H<sub>2</sub>) and deuterium (D, D<sub>2</sub>).<sup>20</sup> That left only the mass to charge ratio as a characteristic that could be used for identifying and quantifying isotopes; in other words, mass spectrometry. Commercial mass spectrometers were not available and the home-made ones constructed by physicists required constant attention due to a host of problems unfamiliar to most chemists.<sup>21</sup> Moreover, these spectrometers were designed primarily to give accurate values of nuclear masses, not their abundances.<sup>22</sup> Finally, water ranks as one of the least amenable compounds for mass spectrometric analysis because of the great difficulty involved in removing residual water from the spectrometer. Thus, physicists and chemists were clearly divided not only by the purposes for which they used deuterium but as well by the experimental methods employed in its study. For physicists the analytical species of interest were atomic and molecular deuterium (D, D<sub>2</sub>), while for chemists it was partially deuterated water.

These two distinct analytical traditions eventually melded together in biochemistry. As Kohler has shown, Schoenheimer at Columbia became convinced of the revolutionary possibilities that deuterium held out in the study of intermediary metabolism. Before the advent of isotopic tracers, metabolic studies depended on adding some compound to an organism's diet, isolating the excreted products and concluding that the second had arisen somehow from the first. Schoenheimer likened this approach to putting a penny in a vending machine, taking out a piece of chocolate and assuming that the penny had been turned into chocolate.<sup>23</sup> In a less jocular assessment, he noted that:

"The study of the metabolism of substances which occur in nature in large amounts and are continually synthesized and destroyed in the animal body presents almost insuperable difficulties. If substances such as naturally fatty acids, amino acids, etc., are administered to an animal, we lose track of them the moment they enter the body, since they are mixed with the same substances already present... The difficulty in following physiological substances in the course of their transportation in the body, and their conversion into other substances, accounts for our ignorance with respect to many of the most fundamental ques-



tions concerning intermediate metabolism. The solution of these problems will be possible only when direct methods for tracing such substances are available".<sup>24</sup>

Overnight, the incorporation of isotopic tracers held out the promise of a degree of certainty in these studies that had been only dreamed of before. In Schoenheimer's words, the possibilities "[appear] to be almost unlimited".<sup>25</sup> In actuality, this new application of deuterium came to pass so soon only because David Rittenberg, one of Urey's PhDs, could not find employment as a physical chemist and went to work with Schoenheimer. It was his exploration and refinement of analytical techniques that allowed the promise of isotopic tracers in metabolism studies to be realised.<sup>26</sup>

Rittenberg initially used the same analytical techniques that all other chemists did, in which samples were burned and the deuterium content calculated from the density and/or refractive index of the resulting water. The results were amazingly accurate but the purification procedures, for both equipment and samples, bordered on the excruciating.<sup>27</sup> As a result, Rittenberg began to experiment with alternate analytical pathways because the nature of the metabolism studies imposed additional restrictions on the experimenters. In particular, sample sizes were often quite small and large numbers of analyses were needed. Rittenberg began to utilise a reaction that had been studied earlier by both Polanyi and Farkas, in which the partially deuterated water underwent catalysed exchange with molecular hydrogen, and the resulting partially deuterated hydrogen then became the analytical target.<sup>28</sup> At first, Rittenberg determined its deuterium content by microthermal conductivity, a procedure that had also been worked out by his predecessors. However, he had already had experience with mass spectrometry while working with Urey, and eventually built a mass spectrometer to analyse the hydrogen/deuterium gas mixture ( $H_2/HD/D_2$ ), which was a comparatively unproblematic procedure. As he noted in 1942, determining isotopic composition with a mass spectrometer was not as accurate as determining it from the density of the water. On the other hand, the mass spectrometric method was not affected by impurities, while with density measurements, "the limiting factor is not the determination of the density of water but its purification".<sup>29</sup> Furthermore, when fully refined, the combined combustion/mass spectrometric method could yield duplicate analyses on 3-5 mg samples of water in 40 minutes.

The push to develop mass spectrometric techniques was given additional impetus by the desire of biochemists to use doubly labeled substrates, such as  $^{15}N$ -enriched deuterated amino acids.<sup>30</sup> The nitrogen in the sample could be converted to nitrogen gas by known reactions and also analysed mass spectrometrically. Thus, a combination of the enormous potential of isotopic tracers for biochemistry and

the exigencies imposed by the nature of the biochemical experiments led to a fusion of the techniques and approaches of physicists and chemists.

After World War II, biochemists increasingly turned away from stable isotopic tracers because of the availability of artificial radioisotopes such as tritium ( $^3\text{H}_1$  or T) and  $^{14}\text{C}$ , which could be easily tracked by liquid scintillation counting.<sup>31</sup> Chemists' allegiance to deuterium and other stable isotopes, by contrast, remained firmly entrenched. The large mass ratio of deuterium to protium was successfully exploited for mechanistic studies in the form of equilibrium and kinetic isotope effects.<sup>32</sup> It also prompted some striking speculations from Urey, who noted in his 1934 Willard Gibbs Medal address that:

"The differences are so great that, if deuterium had been present in a larger proportion in natural hydrogen, its effects could not have been overlooked. Many of our fundamental laws of chemistry could not have been established. It is difficult to estimate what the effects on the history of chemistry would have been. The development of chemistry as an exact science might have been greatly retarded. The atomic weight of hydrogen would not have been constant and perhaps the general acceptance of the atomic theory would have been delayed. On the other hand, the discovery of the isotopes of hydrogen might have been made much sooner, though what we could have done with them in the middle of the nineteenth century, I do not know".<sup>33</sup>

When, in the 1950s, nuclear spin ceased to be the plaything of physicists alone and chemists embraced NMR, deuterium had a special role to play as a sort of virtual element, an absence rather than a presence, the antithesis of its shining career as a tracer.

## Notes

<sup>1</sup> A qualitative sense of historians' predilections can be gained by entering the Subject and Keyword search terms "radioactivity", "radioisotope", "stable isotope", "deuterium", etc in the History of Science, Technology and Medicine database.

<sup>2</sup> Robert E. Kohler, Jr., "Rudolf Schoenheimer, Isotopic Tracers, and Biochemistry in the 1930's", *HSPS* 8 (1977): 257-89.

<sup>3</sup> Valuable discussions of the relationship between experimental practices are found in Doris T. Zallen, "Redrawing the Boundaries of Molecular Biology", *J. Hist. Biol.* 26 (1993): 65-87, and Matthias Dörries, "Easy Transit: Crossing boundaries Between Physics and Chemistry in mid-Nineteenth Century France", in *Making Space for Science: Territorial Themes in the Shaping of Knowledge*, ed. Crosbie Smith and Jon Agar (New York: St. Martin's, 1998), 246-62. Dörries offers a trenchant critique of the spatial metaphor in discussions of scientific boundaries.

- <sup>4</sup>A bibliography published a little over a year after the first definitive identification of deuterium contained 109 entries: "Deuterium", *I&EC News Edition* 12 (1934): 11-12. The interest remained high, as shown by Alice H. Kimball, *Bibliography of Research on Heavy Hydrogen Compounds*, ed. Harold C. Urey and Isidor Kirschenbaum (New York: McGraw-Hill, 1949), which ran to 350 pages.
- <sup>5</sup>For Urey, see James R. Arnold, Jacob Bigeleisen, and Clyde A. Hutchinson, Jr., "Harold Clayton Urey: April 29, 1893-January 5, 1981", *Biog. Mem. Nat. Acad. Sci.* 68 (1995): 363-412.
- <sup>6</sup>Harold C. Urey, F. G. Brickwedde and G. M. Murphy, "A Hydrogen Isotope of Mass 2", *Physical Review* 39 (1932): 164-65; "A Hydrogen Isotope of Mass 2 and its Concentration", *Phys. Rev.* 40 (1932): 1-15.
- <sup>7</sup>Harrison E. Howe, "The Editor's Page", *I&EC* 26 (1934): 1-2, on 1.
- <sup>8</sup>Gilbert N. Lewis, "The Isotope of Hydrogen", *JACS* 55 (1933): 1297-98, on 1298.
- <sup>9</sup>R. H. Crist, G. M. Murphy and Harold C. Urey, "The Isotopic Analysis of Water", *JACS* 55 (1933): 5060-61.
- <sup>10</sup>Gilbert N. Lewis and Daniel B. Luten, Jr., "The Refractive Index of H<sub>2</sub>O<sup>18</sup>, and the Complete Isotopic Analysis of Water", *JACS* 55 (1933): 5061-62.
- <sup>11</sup>Jacob Bigeleisen, "Gilbert N. Lewis and the Beginnings of Isotope Chemistry", *J. Chem. Ed.* 61 (1984): 108-16.
- <sup>12</sup>E. S. Gilfillan and M. Polanyi, "Mikropyknometer zur Bestimmung von Verschiebungen im Isotopenverhältnis des Wassers", *Z. Phys. Chem. A* 166 (1933): 254-56.
- <sup>13</sup>Lewis and Luten, "Refractive Index, 5062."
- <sup>14</sup>Aaron J. Ihde, *The Development of Modern Chemistry* (New York: Dover, 1984 [1970]), 175-79, 393.
- <sup>15</sup>Gilbert Newton Lewis and Merle Randall, *Thermodynamics and the Free Energy of Chemical Substances* (New York: McGraw-Hill, 1923).
- <sup>16</sup>Bigeleisen, "Gilbert N. Lewis", 111.
- <sup>17</sup>Bigeleisen, "Gilbert N. Lewis", 112.
- <sup>18</sup>Edward W. Washburn and Harold C. Urey, "Concentration of the H<sup>2</sup> Isotope of Hydrogen by Fractional Electrolysis of Water", *PNAS* 18 (1932): 496-98; Henry Eyring and Albert Sherman, "Theoretical Considerations Concerning the Separation of Isotopes", *J. Chem. Phys.* 1 (1933): 345-49; I. Laird Newell and J. B. Ficklen, "Concentration of the Heavy Hydrogen Isotope", *JACS* 55 (1933): 2167.
- <sup>19</sup>Paul Forman, "Molecular Beam Measurements of Nuclear Moments before Magnetic Resonance. Part 1: I. I. Rabi and Deflecting Magnets to 1938", *Annals of Science* 55 (1998): 111-160.
- <sup>20</sup>Ref. 6; David H. Rank, "The Isotope of Hydrogen in the Atomic Spectrum", *Phys. Rev.* 42 (1932): 446; Muriel F. Ashley, "The Band Spectrum of H<sup>1</sup>H<sup>2</sup>", *Phys. Rev.* 43 (1933): 770; Gilbert N. Lewis and Muriel F. Ashley, "The Spin of the Hydrogen Isotope", *Phys. Rev.* 43 (1933): 837.
- <sup>21</sup>S. H. Bauer, "Mass Spectrometry in the Mid-1930's: Were Chemists Intrigued?" *J. Am. Soc. Mass Spect.* 12 (2001): 975-88.
- <sup>22</sup>Kenneth T. Bainbridge, "The Isotopic Weight of H<sup>2</sup>", *Phys. Rev.* 42 (1932): 1-10; H. Lukanow and W. Schütze, "Ein Massenspektrograph nach der Parabelmethode mit großer Linienschärfe", *Z. f. Physik* 82 (1933): 610-19. John A. Hipple noted that "Very early in the development of instruments for mass analysis of ions there occurred a natural diversion of the work into two channels. Those physicists interested in the accurate measurement of the masses of the elements developed instruments with very narrow slits and photographic recording .... [those] more interested in the accurate measurement of the relative amounts of the ions of different masses pre-

sent worked with instruments of fairly wide slits and measured the ion current electrically”: “Gas Analysis with the Mass Spectrometer”, *J. Appl. Phys.* 13 (1942): 551-59, on 551.

<sup>23</sup> Mildred Cohn, “Some Early Tracer Experiments with Stable Isotopes”, *Protein Science* 4 (1995): 2444-47, on 2444.

<sup>24</sup> Rudolf Schoenheimer and D. Rittenberg, “Deuterium as an Indicator in the Study of Intermediary Metabolism. I”, *J. Biol. Chem.* 111 (1935): 163-68, on 163.

<sup>25</sup> Rudolf Schoenheimer and David Rittenberg, “Deuterium as an Indicator in the Study of Intermediary Metabolism”, *Science* 82 (1935): 156-57, on 157.

<sup>26</sup> D. Rittenberg and Rudolf Schoenheimer, “Deuterium as an Indicator in the Study of Intermediary Metabolism. II. Methods”, *J. Biol. Chem.* 111 (1935): 169-74; Jack Graff and David Rittenberg, “Microdetermination of Deuterium in Organic Compounds”, *Anal. Chem.* 24 (1952): 878-81; David Shemin and Ronald Bentley, “David Rittenberg: November 11, 1906-January 24, 1970”, *Biog. Mem. Nat. Acad. Sci.* 80 (2001): 257-74. In my view, Kohler (“Rudolf Schonheimer”) does not give sufficient weight to the role played by Rittenberg.

<sup>27</sup> Albert S. Keston, D. Rittenberg, and Rudolf Schoenheimer, “Determination of Deuterium in Organic Compounds”, *J. Biol. Chem.* 122 (1937) 227-37.

<sup>28</sup> J. Horiuti and M. Polanyi, “A Catalyzed Reaction of Hydrogen with Water”, *Nature* 132 (1933): 819; Adalbert Farkas, “A Method of Analysis of Heavy Water”, *Trans. Farad. Soc.* 32 (1936): 413-15.

<sup>29</sup> D. Rittenberg, “Some Applications of Mass Spectrometric Analysis to Chemistry”, *J. Appl. Phys.* 13 (1942): 561-69, on 561.

<sup>30</sup> Rudolf Schoenheimer, D. Rittenberg, M. Fox, Albert S. Keston and Sarah Ratner, “The Nitrogen Isotope ( $N^{15}$ ) as a Tool in the Study of the Intermediary Metabolism of Nitrogenous Compounds”, *JACS* 59 (1937): 1768; D. Rittenberg, Albert S. Keston, F. Rosebury and Rudolf Schoenheimer, “Studies in Protein Metabolism. II. The Determination of Nitrogen Isotopes in Organic Compounds”, *J. Biol. Chem.* 127 (1939): 291-99.

<sup>31</sup> Cohen, “Early Tracer Experiments”, 2447.

<sup>32</sup> Kenneth B. Wiberg, “The Deuterium Isotope Effect”, *Chem. Rev.* 55 (1955): 713-43.

<sup>33</sup> Harold C. Urey, “Significance of the Hydrogen Isotopes”, *I&EC* 26 (1934): 803-06, on 805.

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## 4 THE FOUNDATION AND DEVELOPMENT OF MACROMOLECULAR CHEMISTRY

### *Introduction by W. Gerhard Pohl*

When I started in 1962 as a Ph.D. student of Werner Kuhn in Basel, I heard a lot about his contributions in the early years of polymer science. After his unexpected death in 1963 I became acquainted with the correspondence of Werner Kuhn with Hermann Mark. Recently I found unpublished correspondence of Kuhn with Hermann Staudinger and with Hermann Mark in the archives of the University of Basel. These letters throw a light on the development of polymer science, which is not well known even among historians of chemistry. So I thought it might be of interest to produce a paper on that topic. After reading the tribute to Hermann Staudinger's 50 years Nobel Prize anniversary in 2003, by Helmut Ringsdorf, I approached him suggesting to organise with me a session on the early history of polymer science at the Leuven conference. Ringsdorf was the last graduate student accepted by Staudinger and continued in polymer research later, whereas I moved into the field of biophysical chemistry after finishing my dissertation. Professor Ringsdorf accepted my suggestion and contributed knowledge and enthusiasm to organising this special session about the origin of the science of macromolecules and about polymer science. Four papers were accepted for the session and were arranged in historical sequence:

- Gary Patterson (Pittsburgh). *The Emergence of Macromolecular Paradigm in the World of Chemistry*.
- W. Gerhard Pohl (Linz). *Historical Notes from the Development of Macromolecular Chemistry between 1920 and 1940*.
- Johannes Feichtinger (Vienna). *Hermann Mark. The Viennese Born Ambassador of Macromolecular Research*.
- Marcel Van Beylen (Leuven). *Professor Georges Smets. The Development of Macromolecular Chemistry in Belgium*.

Because Gary Patterson could not attend the conference his paper was read by Peter J. T. Morris (London). The other papers were given by the authors.

Helmut Ringsdorf (Mainz) was the ideal chairman for this special session, because he had known Hermann Staudinger, Hermann Mark and Werner Kuhn personally. He added several high lights after the presentation of the papers and contributed significantly during the lively discussions. For instance he pointed out that Hermann Staudinger's dreams belonged to the unity of chemistry and biology. It was a coincidence that in 1953, when Staudinger received the Nobel Prize "for his discoveries in the field of macromolecular chemistry", the structure of DNA had been found by James Watson and Francis Crick. The chemical structure of that macromolecule catalysed the development of molecular biology and is the basis of gene technology.

Several chemists, mentioned that they had known Staudinger as the "father of macromolecular chemistry", but they did not know how difficult it had been for him to convince the chemical community that giant molecules can exist. They were also surprised that Staudinger for many years had not accepted the results from physical chemistry about the structure of macromolecules in solution.

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# The Emergence of the Macromolecular Paradigm In the World of Chemistry

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The community of scientists devoted to the study of macromolecules is now large and vibrant. However, before 1920, there was no such community. The present paper traces the ideas associated with matter from the early days of Greek natural philosophy through the ages of the mechanical philosophy, the electrochemical period, the rise of structural chemistry and the rise of physical chemistry. A detailed analysis of the work of Boscovich and of van't Hoff is presented. Reasons for the delay in the formation of a coherent community of polymer scientists are discussed.

## Introduction

The world in which we live is filled with macromolecules. However, the recognition that this is so is of fairly recent origin. A macromolecule is defined as a covalent assembly of atoms of high relative molecular mass with a definable composition and structure containing a large number of subunits of relative low molecular mass. In order for macromolecules to be recognized as a normal part of our world, it was necessary to have a compelling theory of molecules that was accepted by the worldwide community of chemists. In addition, the conditions under which such covalent assemblies could attain large sizes needed to be elucidated. The story of macromolecules started in antiquity, herein an account is given of the era prior to 1920.

The present community of scientists devoted to the study of macromolecules is very large. It includes chemists, physicists, biologists, mathematicians, chemical and mechanical engineers, materials scientists and engineers, physicians, dentists, and even people with formal degrees in polymer science and engineering. The present account considers the period before there were such communities, and concludes at a time when there were no compelling reasons to believe that such a community would soon form. However, the growth of natural philosophy that preceded and key moments in this history will be discussed in anticipation of an eventual ontology of matter that included macromolecules.

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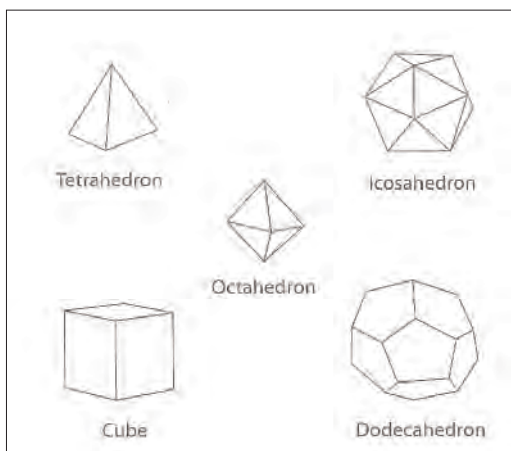
## Science in Antiquity

Communities of philosophers developed at several places and times in the ancient world, such as Croton, Elea, Miletus, Byblos and Athens. The world was observed and critical thought was brought to bear on the description and explanation of the natural environment. There was no lack of observations to feed this process and the terrestrial world was divided into animals, vegetables and minerals. Part of philosophy was learning to distinguish things that differed.<sup>1</sup> To a human observer, the air seemed continuous, as did the sea and the land. It was perfectly reasonable to conclude that everything on earth was a mixture of continuous elements. This continuum paradigm still survives as the best choice for many phenomena on earth.

Philosophical intuition is not often content to rest with the most obvious explanation of physical reality. Considerations of the ultimate divisibility of matter led to the notion of “indivisibles” or *ατομος*. But, what were the atoms like? Some thinkers reasoned that at some level, all matter must be identical, and hence the atoms were all the same. Thus if they were all the same they must be spherical, the perfect shape.<sup>2</sup> This material idealism is very attractive until one tries to explain the incredible heterogeneity of our actual world. The intellectual pressure to elaborate the fundamental particles of our world is and was then strong, and other noble shapes were invoked: the so-called Platonic solids (tetrahedron, cube, octahedron, dodecahedron and icosahedron). (See Figure 1).<sup>3</sup> The question of the interactions of the fundamental particles was much harder to envision. But, Greek thinkers like Democritus did suggest that there were many different intrinsic

shapes associated with the fundamental particles.<sup>4</sup> This crude model could have led to a science of macroparticles, but there was no evidence for it, and it languished in favor of the continuous model, favored by Aristotle.

The structure and dynamics of the particle world were also discussed. Since the earth was at the center of the universe, and all matter sought its natural place, the particles were assumed to be falling towards the earth. If the particles were not presently in contact, and if they did



**Figure 1. The Greek fundamental particles (From Scerri, *The Periodic Table*).**



not interact at a distance, then they would fall independently towards their ultimate destination. The raw determinism of this view offended philosophers like Epicurus, and he proposed that the particles occasionally underwent “swerves” in order to preserve free will. The ability to elaborate a fictional account of reality in order to satisfy philosophical preferences is one of the signs of creativity in humankind. But, these “solutions” did not compel adherence and are now largely, but not completely, forgotten.

### **The Age of Gassendi**

The corpuscular paradigm returned to “polite discourse” in the time of Gassendi, Descartes and Boyle. The “mechanical philosophy” demanded that the material universe be described entirely in terms of the structure and dynamics of the particles of which it was composed. Since the particles were viewed as hard spheres, this research program was developmentally challenged from the start. Even the ingenious invention of “vortices” as the fundamental structural and dynamic units of material reality by Descartes was largely stillborn.

Robert Boyle carried out quantitative measurements on gases and showed that they behaved as “elastic” bodies: they resisted compression with a predictable force. Isaac Newton explained this pressure in terms of the intrinsic repulsions between otherwise static gas particles.<sup>5</sup> This “triumph” is a good example of the underdetermination of science. Not all successful explanations are a faithful representation of the microscopic reality of matter. The invocation of repulsion, when the only widely accepted interaction between particles of matter was gravitational attraction, was brave on Newton’s part, but he was pilloried for asserting both attraction and repulsion by those who viewed such talk as superstitious. The “fundamentalism” of Newton’s day rejected both his science and his religion.

Apparently, there needed to be at least two kinds of matter: the kind that was attracted to itself and produced liquids and solids, and the kind that repelled itself and produced gases. Clearly, there could be no macroparticulate gases. And liquids and solids could not be represented in terms of well-defined compositions and structures. There could only be aggregations of matter in this conceptual world.

The Golden Age of “Atoms and Powers”<sup>6</sup> (Thackray, 1970) flowered especially well in the work of Roger Joseph Boscovich (1711-1787). He was a truly multidisciplinary thinker with an allegiance to the Society of Jesus and to Nature. He combined a rare excellence in many fields and was “at once philosopher, astronomer, physicist, mathematician, historian, engineer, architect and poet”.<sup>7</sup> He was an

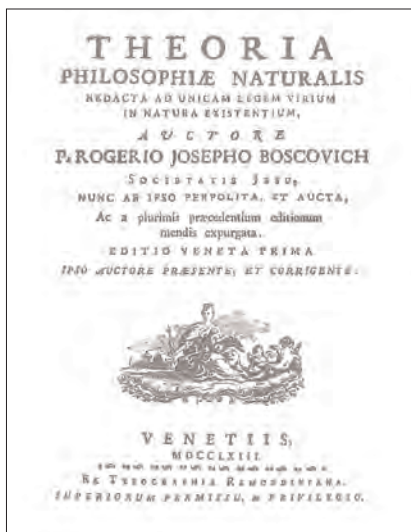


Figure 2. Title page from “Theoria Philosophiae Naturalis” R.J. Boscovich, 1763.

independent thinker who tried to take the best of 17<sup>th</sup> century thought, wherever he found it. He tried to mediate between the followers of Newton and Leibniz and produce a theory that would satisfy both. His system was presented in a magisterial work “Theoria Philosophiae Naturalis”. It is available in English translation.<sup>7</sup>

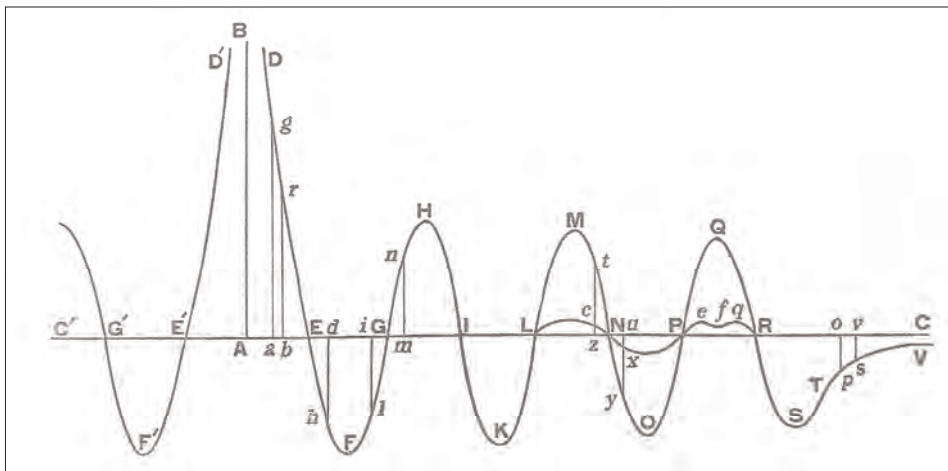
Boscovich’s approach was to combine all known empirical knowledge with a careful philosophical and mathematical analysis. On philosophical grounds, he believed, strongly in the “continuity of Nature.” While “hard spheres” might be useful for illustrative purposes, he believed correctly that all real materials must avoid “discontinuities.” He also believed in the “impenetrability” of the fundamental particles of matter.

Another philosophical preference he had was for the identical nature of the primary particles of matter. With these philosophical assumptions, how could he produce a theory that would explain the richness of the actual world of observation? His “solution” was to propose an interparticle force function that was sophisticated enough to yield a great variety of composite particles. The famous graph of this function is shown as Figure 3.

Boscovich reasoned that the fundamental particles had no intrinsic size and could best be represented as points. He followed Leibniz and the theory of “monads” in this regard. Lest we think this view is antiquated, modern electrons are often viewed as points with no intrinsic size. For example, the limiting de Broglie wavelength for an electron is,

$$\lambda = h / m_e c = 2.43 \times 10^{-12} \text{ m}$$

a very small size. As two of these points approached along their line of interaction, the force of repulsion increased continuously and without bound. However, the range of this ultimate repulsive potential is very small. At very large separations, two mass points interact by gravitational forces and the final portion of the curve reflects this attractive force. The intermediate oscillations are required to reproduce the properties of macroscopic matter. There must be both repulsions and attractions in order to allow the formation of composite particles with more complicated force curves.



**Figure 3. The force between two primary particles in the theory of Boscovich.**

Boscovich proposed that these primary particles combined to form “tenacious compound particles” (TCP). These TCP entities were then proposed to be the basis of the chemical elements. These compound particles could be described by a well-defined mass, a well-defined structure, and a well-defined force function with themselves and with other tenacious compound particles. In particular, he proposed that the surface of these TCP entities could have regions of attraction and repulsion with very regular patterns. His thinking was very geometrical and he reasoned from the observed shapes of single crystals to the shapes of the underlying TCPs and to the regular arrangements of the compound particles.

Such a model is complex enough to produce TCPs with multiple attractive sites that could lead to either linear or branched chains of TCPs. Unsaturated surface sites could be covered with univalent TCPs or be available for further interactions. Boscovich had a profoundly geometrical mind, and could have easily developed a theory of macroTCPs, but with so many other natural phenomena to explain, he focused his attention on materials that were important in his world, for example gel-like materials such as rennet. Most of the observable materials that were formed by aggregation of TCPs were also viewed as reversible. The theory of Boscovich looks similar to modern colloid science where one of the most celebrated interparticle potentials (DLVO) contains multiple repulsive and attractive regions.

The spirit of Boscovich is well-expressed in two of his comments from the conclusion of the “Theoria”:

“Although we cannot peer into the intrinsic nature of bodies, the endeavour to investigate Nature must not be abandoned. Many things can be detected daily from those external properties”.

“But what if, partly by observation and partly by using deduction, it should finally be established that matter is homogeneous, and that all distinctions between bodies comes from form, connection, forces and motions of the particles, such as may be the fundamental origin of all sensible properties? These escape our senses for no other reason than the exceedingly small volume of the particles; nor are they beyond the powers of our intelligence, except on account of their huge number, and the very complicated, though general, law of forces. Owing to these, we cannot hope to obtain an intimate knowledge of the composition of each species. I consider that the attainment of a knowledge of the structure of particular bodies in the future will be difficult; that it will be altogether impossible, I will not dare to assert.

### **The Age of Franklin**

The existence of gravity and the proposed existence of gaseous repulsion was not enough to produce a satisfactory natural philosophy of chemistry. The extensive studies of electrical phenomena carried out by Benjamin Franklin and others<sup>8</sup> and chronicled by Joseph Priestly<sup>9</sup> provided an opportunity to further elaborate the theoretical description of matter. Macroscopic particles displayed both attraction and repulsion under the right conditions, and the strength of this force was much larger than the gravitational attraction. Perhaps electricity was the key to the understanding of chemistry. Berzelius certainly thought so! He even coined the word “polymer” to describe the aggregation of smaller “mers” to form more extended objects.<sup>10</sup>

The physical description of electricity in the 18<sup>th</sup> century was as a fluid; it flowed from one place to another. Boscovich proposed that his compound particles could absorb many different fluids: light, heat (caloric) and electricity. After all, some materials absorb light, and some even give it off. The belief in the existence of “elements” in addition to the current chemical atoms is well-documented.<sup>11</sup> In particular, it was believed that each compound particle was filled with just the right amount of electricity under isolated conditions. Manipulations of these particles could lead to either an excess or a deficit of electric fluid. Particles with an excess of electricity attracted those with a deficit. In addition, “neutral” tenacious compound particles had different “affinities” for electricity, so that, when two par-

ticles were near one another, electricity could flow from one particle to another, leading to attraction. Identical particles would not interact in this way since there was no tendency to exchange electricity. The modern notion that electricity is exchanged one electron at a time was completely unknown. Dalton lived in this netherworld where even the boldest chemists believed in some continuous fluids. Phlogiston may have been banished, but other tenuous substances would continue to be important until the 20<sup>th</sup> century.

### **The Age of Dalton**

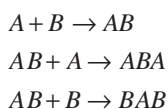
“The New System of Philosophical Chemistry” envisioned gaseous entities as composed of a small, unique number of chemical atoms. Exactly what these atoms were was unknown and exactly why these atoms stuck together was not understood, and it was usually asserted that homonuclear diatomic or polyatomic molecules were forbidden. Even though Dalton was lionized during his lifetime, his philosophy was often denigrated, even by those, such as Davy, who were in a position to award the medals and national honours! The Law of Definite and Multiple Proportions was a major advance in understanding, and the atomic paradigm is essential for the development of a science of macromolecules, but Dalton could never convince his contemporaries why elements needed to be discussed in terms of chemical atoms.

As the number of known chemical substances increased, it became more and more clear that stable polyatomic structures were the most useful way of representing many chemicals. Gerhardt and Laurent in France took the lead in promoting this perspective.<sup>12</sup> More detailed thinking about the proposed structures led to the concept of valency.<sup>13</sup> Some atoms appeared to be routinely bound to more than one other atom. The source of the chemical bond was vaguely described in electrostatic terms, but the utility of valency was becoming apparent to many chemists. Once Kekulé had established the tetravalency of carbon atoms, it appeared that the notion of macromolecules was inevitable. Kekulé even promoted a generic formula for the n-alkanes of the form:  $C_n H_{2n+2}$ , however he was uneasy with the notion that  $n$  could be very large.

Part of the genius of the paradigm of structural formulas was that the lines drawn on paper represented logical relationships between the atoms. The connectivity of each atom could be discussed without knowing exactly what the nature of the connection was. For the geometrically minded chemist, geometry was indeed the royal road to chemistry. Even in the 21<sup>st</sup> century, most chemists draw lines on

paper without any thought of quantum mechanics. With the concept of multivalency and the visual aid of atomic structural formulas, a science of macromolecules should have been inevitable, but there are many chemical reactions that are inevitable but rarely happen.

The reticence in the mind of Kekulé and many others at this point in history is quite understandable. In the absence of detailed knowledge about the nature of the chemical bond, it is most reasonable to assume that atoms bind according to some equilibrium expression. In the simplest case of an alternating binary chain polymer,  $(AB)_n$ , a chemical reaction scheme can be presented in the form:



If these chemical reactions are reversible and the equilibrium constant for all these reactions is the same, the probability distribution of chain lengths ( $n$ ) can be given as:

$$\wp(n) = p^{n-1}(1-p)$$

where  $p$  is the probability that an AB bond is formed. The number average degree of polymerization is then given by:

$$\langle n \rangle = 1/(1-p)$$

For a bond probability of 90%, the mean chain length is only 10. The notion that average degrees of polymerization in the thousands could be obtained was extremely implausible in the 19<sup>th</sup> century. Reversible polymerization is inherently inefficient if large molecular weights are desired. Every linear polymer can be decomposed by heating it to the range where the entropy gained by depolymerization drives the equilibrium back towards smaller units; the phenomenon known as the ceiling temperature. Since the principles of chemical equilibrium were only just beginning to be formulated in a precise way, and the principles of chemical kinetics were also in their infancy, most chemists continued to think in terms of reversible aggregation as the best paradigm for producing larger, but not too large, entities.

The strengths of chemical bonds were not known, nor was the actual basis for a chemical bond understood. Nevertheless, the geometry of structural formulas did provide a sound conceptual basis for the discussion of molecules. The pinnacle of clarity in structural chemistry was reached in the work of J.H. van't Hoff, "Chemistry in Space" (1891).<sup>14</sup> This English translation was also an updated and highly expanded version of the original French pamphlet, "La Chimie dans L'Espace" (1875).

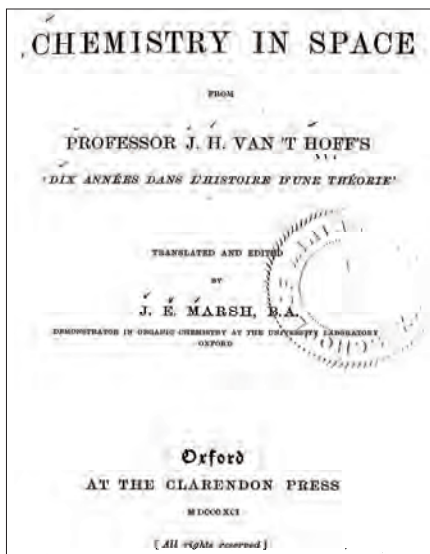


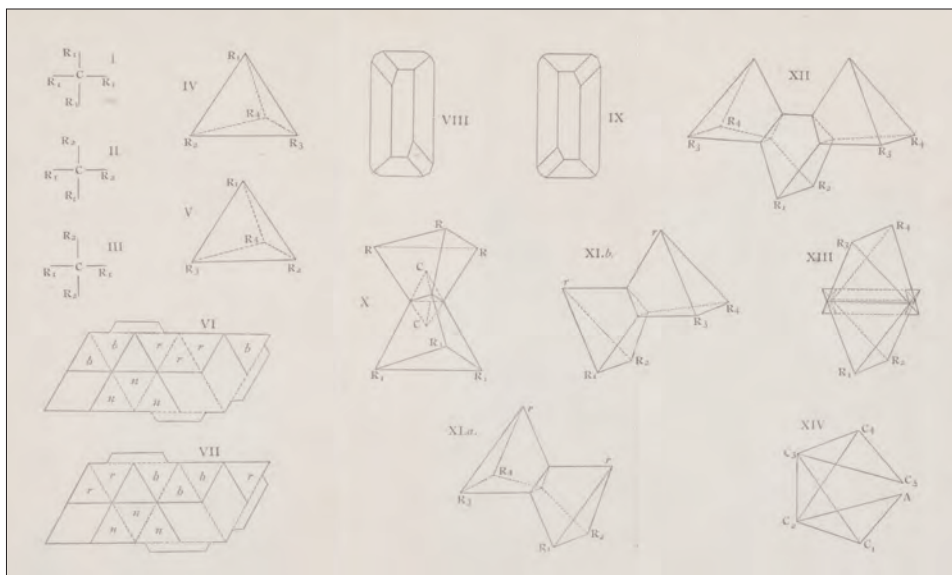
Figure 4. J. H. van't Hoff, *Chemistry in Space*, Oxford, Clarendon Press, 1891.

The tetravalency of carbon was further elaborated in terms of the tetrahedral geometry associated with carbon compounds. All of the known optically active organic compounds were explained in detail by the theory that the presence of an asymmetric carbon with four distinct substituents was necessary for the creation of optical rotatory power. Hundreds of specific compounds were discussed in detail. The field of Stereochemistry was completely consolidated and a vibrant research community was created that persists to the present. The book concluded with a folding sheet of line diagrams similar to that to be found in any modern organic chemistry textbook.

Among the many types of organic compounds containing asymmetric carbons, homologous series of chain molecules were discussed. Chemical formulas of the form:  $E_1-(CR_1R_2)_n-E_2$  occur frequently in the chapter on molecules with multiple asymmetric carbons. van't Hoff understood completely that each asymmetric carbon needed to be considered and the overall optical activity would depend on the actual sequence of centers. For example, a molecule with two asymmetric carbons would have three distinguishable optical isomers: Using *r* and *s* notation, there would be two optically active forms, *rr* and *ss*, with opposite rotation, and two indistinguishable forms, *rs* and *sr*, with zero optical rotation. No limit was placed on the value of the chain length, *n*, but only known compounds that had structures that were considered reliable were discussed in detail. These included the hexose sugars, so loved by Emil Fischer.

The progress of the field of stereochemistry was often obfuscated by the presence of impurities in commercial samples of either natural or synthetic products. An especially interesting case involving styrolene (styrene) was discussed in detail. The structural formula definitely ruled out optical activity, but commercial samples routinely yielded optical rotation. The degree of rotation could be varied by repeated distillation, but it was difficult to eliminate entirely. Of more interest in the present context, it was observed that freshly distilled styrene polymerized on its own, a fact that has been verified many times. The concept that unsaturated





**Figure 5. J.H. van't Hoff diagrams from "Chemistry in Space". Photos by Douglas A. Lockard. The Donald F. and Mildred Topp Othmer Library of Chemical History, Chemical Heritage Foundation, Philadelphia, PA.**

compounds were capable of polymerization was treated as perfectly natural by van't Hoff. The stereochemical consequences of polymerization of unsymmetrical-substituted alkenes were discussed. In view of the clarity of this exposition, it is astonishing that the existence of macromolecules was often denied by leading organic chemists. But leading lights such as Berthelot even denied the existence of chemical atoms. Ultimate progress in the science of macromolecules needed to wait for the death of a generation of anti-atomists and anti-structuralists such as Berthelot and Kolbe.

### The Rise of Physical Chemistry

The first Nobel Prize in Chemistry was awarded to van't Hoff (1901). If his masterpiece, "Chemistry in Space", was not enough, he also contributed major work to the theory of chemical thermodynamics, chemical kinetics and solutions. He is the father of the field of physical organic chemistry. To the lifeless structural formulas, he added a dynamic view of molecules; they vibrated, rotated and even underwent internal conformational changes. He discussed chemical reactions in



terms of the mechanism of the transformation. He considered the breaking and making of chemical bonds to be the key to understanding reactions.

One of the most important steps in the establishment of the molecular paradigm in the 19<sup>th</sup> century was the measurement of accurate molecular weights. Application of the gas theory of Avogadro (and later by Ampere) to measurements of gas density by Dumas led to accurate molecular weights for many molecules. Since most macromolecules are either nonvolatile or have very low vapor pressures below their ceiling temperature, another way of measuring molecular weight was necessary. Major advances in the understanding of solutions were made by Raoult and van't Hoff. van't Hoff reasoned that if it was the kinetic motion of gas molecules that gave rise to gas pressure, the kinetic motion of solute molecules would give rise to osmotic pressure (Jones, 1899).<sup>15</sup> The importance of the kinetic theory of matter for the rise of modern chemistry must be stressed. However, the self-proclaimed father of physical chemistry, Ostwald, was unwilling to consider solutions of large particles of matter to be "true" solutions, and proposed that they were merely physical mixtures with zero osmotic pressure, like glass marbles in a fish-bowl! Even the authority of van't Hoff was not enough; it took the kinetic theory of solutions by Einstein<sup>16</sup> and the Brownian motion measurements of Perrin<sup>17</sup> to clarify the matter. The van't Hoff Law of Osmotic Pressure can be stated as:

$$\Pi = (N/V)k_bT$$

where N is the number of solute molecules in volume V, and the kinetic theory term includes the Boltzmann constant and the absolute temperature. A knowledge of the mass concentration of the solution, c, then leads directly to the molecular weight, M, since  $N/V = cN_A/M$ . Perrin is famous for his dogged pursuit of an accurate value for the Avogadro number (Perrin, 1913).<sup>18</sup> Actual measurements of many particles in solution yielded values in excess of 10,000 for the molar mass, either by osmotic pressure or by melting point depression, another equivalent colligative technique discussed by Raoult and van't Hoff. Mere measurements were insufficient to convince scientists who were committed to a paradigm of physical aggregation as the source of the large masses for the particles. The science of colloidal particles was already a coherent research community in the late 19<sup>th</sup> century, but its leading lights were committed to a stance that promoted qualitatively different principles for colloids than for other forms of matter: a "new" form of matter, a "new physics." The community of colloid scientists in the 21<sup>st</sup> century is large and vibrant and the unity of physical chemistry has been restored.

Since a truly atomic level method for studying the chemical structure of molecules was still many years away, more mesoscopic techniques were developed to gain new insights. The transport properties of solutions were considered and Einstein

developed a kinetic theory of solutions that included particle diffusion and viscosity. While measurement of the diffusion coefficients of solute particles is now routine, it was hard work in the 19<sup>th</sup> century. Measurement of the viscosity of liquids and solutions was much easier, and the Ostwald viscometer was a precision device. The standard procedure consists of measuring the viscosity of the solution as a function of concentration and then calculating the limiting quantity known as the intrinsic viscosity:

$$[\eta] = \lim_{c \rightarrow 0} ((\eta - \eta_0) / c \eta_0)$$

Einstein showed that the intrinsic viscosity could be related to the hydrodynamic volume as:<sup>19</sup>

$$[\eta] = (((5/2)V_h N_A) / M)$$

It was observed that colloidal particles in the size range 1-1000 nm were common. Just the evidence of large size did not convince many colloid scientists that these particles were macromolecules. Even the observation that very large sizes could be obtained for very small values of  $M$  did not compel adherence to the macromolecular paradigm. After all, reversible aggregation can lead to fractal particles with a low internal density.<sup>20</sup>

Classical physical chemistry was a very successful research community and the three famous “Ionists” (van’t Hoff, Arrhenius, and Ostwald) all received the Nobel Prize during the first decade of the 20<sup>th</sup> century.<sup>21</sup> A flood of disciples followed in their footsteps. The “hot” areas of research included chemical kinetics and colloid science. Macromolecules had not yet attracted enough interest to produce a community devoted to the study of their properties as a full time effort.

## Notes

<sup>1</sup> A good source for the ancient period is Marshall Clagett, *Greek Science in Antiquity* (Salem, Ayer Company, 1985).

<sup>2</sup> Another good philosophically oriented treatment is Ernan McMullin, *The Concept of Matter in Greek and Medieval Philosophy* (Notre Dame, University of Notre Dame Press, 1965).

<sup>3</sup> Eric R. Scerri, *The Periodic Table: Its Story and Its Significance* (New York, Oxford University Press, 2006).

<sup>4</sup> The essential reference for the history of atomism. Bernard Pullman, *The Atom in the History of Human Thought* (Oxford, Oxford University Press, 1998).

<sup>5</sup> Isaac Newton, *Principia Philosophiae Mathematica*, (London, Royal Society, 1686).

<sup>6</sup> Arnold Thackray, *Atoms and Powers: An Essay on Newtonian Matter-Theory and the Development of Chemistry* (Cambridge, Harvard University Press, 1970).

- <sup>7</sup> Branislav Petronievic, "Life of Roger Joseph Boscovich" in Roger Joseph Boscovich, *A Theory of Natural Philosophy*, (Cambridge, MIT Press, 1966).
- <sup>8</sup> I. Bernard Cohen, *Franklin and Newton* (Philadelphia, The American Philosophical Society, 1956).
- <sup>9</sup> Joseph Priestley, *A History and Present State of Electricity, with Original Experiments*. (London, J. Dodsley, J. Johnson and B. Davenport, 1767).
- <sup>10</sup> Herbert Morawetz, *Polymers: The Origins and Growth of a Science* (New York, John Wiley and Sons, 1985).
- <sup>11</sup> Scerri, *The Periodic Table*.
- <sup>12</sup> Alan J. Rocke, *Nationalizing Science: Adolphe Wurtz and the Battle for French Chemistry* (Cambridge, The MIT Press, 2001).
- <sup>13</sup> Alan J. Rocke, *Nationalizing Science*.
- <sup>14</sup> Johannes H. van't Hoff, *Chemistry in Space*. (Oxford, Clarendon Press, 1891).
- <sup>15</sup> Harry Clary Jones, Ed. *The Modern Theory of Solutions* (New York, Harper and Brothers, 1899).
- <sup>16</sup> Albert Einstein, *Investigations on the Theory of the Brownian Movement* (London, Methuen and Co., 1926).
- <sup>17</sup> Jean Perrin, *Brownian Movement and Molecular Reality*. (London, Taylor and Francis, 1910).
- <sup>18</sup> Jean Perrin, *Les Atomes*. (Paris, Alcan, 1913).
- <sup>19</sup> Gary Patterson, *Physical Chemistry of Macromolecules* (Boca Raton, CRC Press, 2007).
- <sup>20</sup> Zhenyu Gu, *et al.* "Self-assembled supramolecular microgels: fractal structure and aggregation mechanism". *Journal of Polymer Science, Part B: Polymer Physics* 41 (2003): 3.037-3.046.
- <sup>21</sup> L.K. James, Ed. *Nobel Laureates in Chemistry 1901-1992* (American Chemical Society and Chemical Heritage Foundation. 1993).

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## Staudinger - Mark - Kuhn: Historical Notes from the Development of Macromolecular Chemistry between 1920 and 1940

W. Gerhard Pohl\*

The author's interest in history of science began when working as a student in the 1960s in the Institute of Physical Chemistry of the University Basel. There I was engaged in a doctoral thesis about polyelectrolyte gels<sup>1</sup> under the supervision of Werner Kuhn (1899-1963). After the sudden death of Professor Kuhn in 1963 I studied his biography and discovered that he had worked in many different fields of science, starting as a physicist in Copenhagen where he became a friend of Niels Bohr (1885-1962) and working in the laboratory of Ernest Rutherford (1871-1937) for some time. During this period he was looking for an effect which later became known as "Mößbauer effect" which he predicted theoretically but could not demonstrate by experiment. Thereafter, he joined the organic chemist Karl Freudenberg (1886-1983) in Heidelberg. There he developed a theory of optical activity and began his studies in the new field of macromolecules. His work attracted the interest of Hermann Staudinger (1881-1965) and Herman F. Mark (1895-1992), with whom he exchanged scientific correspondence for many years. This paper focuses on aspects of the early development of macromolecular chemistry by the contributions of Staudinger, Mark and Kuhn.

### Early synthetic polymers

In the 19<sup>th</sup> century chemists found substances which behaved differently than normal organic substances. They were by-products of certain syntheses, unwanted because they were sticky, insoluble and could not be studied by the usual methods. For these materials the words "polymer" and "colloid" were coined. Adolf von Baeyer (1835-1917) for instance observed the formation of such a polymer by the reaction of phenol with formaldehyde. Apparently he was thinking of the possibility to synthesise very large molecules and exchanged this idea with friends. One of them the well-known organic chemist Victor Meyer (1848-1897) wrote in a letter to Baeyer in 1885: *"I let build (my co-workers) giant molecules. I want to see,*

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*how far this goes....It would be nice to get hydrocarbons of known constitution built from 1000 atoms. You once calculated that a molecule with one million atoms should be visible."* In 1907 the Belgian-born American chemist Leo Hendrik Baekeland (1863-1944) used Baeyer's reaction to form Bakelite, the first wholly synthetic resin. He built his first factory near Berlin. Later his company spread over the whole world and he became a wealthy man. In 1924 he was elected president of the American Chemical Society.



**Portrait of Hermann Staudinger.**  
Reproduced by Courtesy of Deutsches Museum, Munich.

Now we turn to Hermann Staudinger. Born in 1881 he received his Ph.D. with Daniel Vorländer (1867-1941) in Halle/Saale. He moved to Straßburg and in 1905 discovered a new class of substances –the Ketenes. He became Professor at Karlsruhe. Staudinger also studied aliphatic diazo-compounds, organic phosphorus compounds and other groups of substances. In 1912 he became successor of Richard Willstätter (1872-1942) at the ETH, Zürich. There he studied explosions when halogen-compounds reacted with alkali-metals. He also investigated isoprene, insecticides and flavour components and was engaged in introducing new chemistry curricula at the ETH. His best student in Karlsruhe, Leopold Ruzicka (1887-1976) moved with him to Zürich. He had received his Ph.D. in 1910 for studies on ketenes. In Zürich he was assigned to work on insecticides. When Ruzicka told Staudinger about his desire to pursue his own interests in the terpenes, he lost his assistantship and *"found his research facilities to be severely curtailed"*, an action that understandably disappointed Ruzicka. Staudinger was an inspiring teacher, but also a harsh taskmaster for the young chemist. One of Staudinger's best students in Zürich was Thadeus Reichstein (1897-1996), who had to work on flavour components of coffee. When later I interviewed Reichstein, who had been one of my teachers in Basel, he told me, smilingly about Staudinger, that he was always trying to make a lot of money with his chemistry. But Staudinger was not as successful in that respect as Reichstein, who later became *"stein-reich"* through his synthesis of ascorbic acid. It may be hypothesised that it was this interest in substances with a commercial potential, which to some extent motivated Staudinger to turn to polymers, having seen the success of Baekeland. But this view is only my suspicion which cannot be proved by documents! Staudinger's polymer-period began in 1920 with his paper *"Über Polymerisation"*. At this time he had little experimental evidence, but defined the name *"macromol-*

ecule” in 1924 for particles with many atoms linked together by normal valencies in a long chain. He suggested that several natural substances such as cellulose, rubber and proteins belonged to this class of substances as well as several synthetic molecules. Among these polymers he suggested polyoxymethylene as a model of cellulose, polystyrene as a model of rubber and polyacrylic acid as a model of proteins. Most prominent chemists believed at that time that polymers were merely aggregates of small molecules –the so called micellar theory. This can be documented by the opinions of leading researchers of the time. For example, Jean Perrin (1870-1942) said in 1913: *“We would expect, moreover, that very complicated molecules would be more fragile than molecules composed of a few atoms and they would therefore have fewer chances in coming under observation”*. In 1907 Emil Fischer (1852-1919) said about molecular weights (quote): *“In my opinion these numbers (12000 – 15000) are based on very uncertain assumptions since we do not have any guarantee that the natural proteins are homogenous substances*. Emil Fischer reported in 1913 on the synthesis of a derivative of maltose with a molecular weight of 4021, a figure that he suggested stood at the extreme end of true molecular weights of organic compounds. Fischer regarded the then-accepted figure of 16700 for the molecular mass of haemoglobin –determined from its content of iron– as much too high. In 1925 Paul Niggli (1888-1953) said at a meeting of the Zürich Chemical Society about macromolecules: *“Such a thing does not exist”*. Staudinger at the end of that stormy meeting exclaimed (like Luther): *“Here I stand, I cannot do otherwise”*. In 1926 Heinrich Wieland (1877-1957) said to Staudinger, when he became his successor in Freiburg: *“Dear colleague, drop the idea of large molecules; organic molecules with a molecular weight higher than 5000 do not exist. Purify your products, such as rubber, then they will crystallize and prove to be low molecular weight compounds.”* In the same year Max Bergmann said: *“The assumption that high molecular organic natural compounds form large molecules or some special polymerization processes must be connected with their structure, has been perfectly defeated.”*

When Staudinger went from Zürich to Freiburg in 1926, Reginald O. Herzog (1878-1935) was head of the Kaiser-Wilhelm Institute for Fibre Research in Berlin. He worked together with Kurt H. Meyer (1883-1952) and Hermann Mark. In 1925 Reginald O. Herzog stated that X-ray crystallographic data collected by his group at the Kaiser-Wilhelm Institute supported the micellar structure of cellulose. Because Herzog, Meyer and Mark supported the micellar theory, Staudinger felt that they were his enemies. Staudinger was a pacifist and had been outspoken in his opposition to the First World War. In contrast, Meyer was very patriotic and had worked in the poison gas department of Fritz Haber during the war. So he did not like Staudinger for other reasons than his stubborn





**Portrait of Hermann Mark. Reproduced by Courtesy of Deutsches Museum, Munich.**

thinking in science alone. Meyer and Staudinger were not willing to make compromises. Mark, besides his scientific brilliance, had been a decorated soldier in the war which was attractive for Meyer. However the Austrian Mark tried to avoid aggressive controversies with Staudinger. Meyer and Mark worked together for several years in the BASF Company. They both were in part Jewish and later emigrated from Germany. In 1926 a key confrontation between Staudinger and the supporters of the micellar theory took place at the conference of the Association of German Natural Scientists and Physicians at Düsseldorf. Hermann Mark's presentation helped to undermine the apparent support given to the micellar theory by X-ray crystallography. Some leading scientists like Richard Willstätter were now inclined toward the

macromolecular concept. He said *"For me, as an organic chemist, the concept that a molecule can have a molecular weight of 100000 is somewhat terrifying, but, on the basis of what we have heard today, it seems that I shall have to slowly adjust to this thought."* According to Rudolf Signer Willstätter added: *"Staudinger opens to chemistry a field which is much bigger than all organic chemistry"*.

Today it is hard to understand why the scientific controversies between Staudinger, Mark and Meyer continued for a long time. In 1928 Karl Freudenberg a former student of Emil Fischer suggested a structure of cellulose as a chain of small molecules as envisioned by Staudinger. In his laboratory the young Swiss physical chemist Werner Kuhn started to work on the optical activity of polysaccharides. The work of Kuhn will be discussed later. In 1930 at a meeting of the German Colloid Society at Frankfurt K. H. Meyer was elected president of the Society, previously dominated by Wolfgang Ostwald (1883-1943) and other micellarists. The meeting was generally considered a victory for the macromolecular school of thought, although the micellarists refused to accept their



**Portrait of Werner Kuhn. Reproduced by Courtesy of the University of Basel.**

total defeat. Also in 1930 Staudinger developed a new method to determine the molecular weight of macromolecules, because the classical methods did not work well. He found a correlation between the viscosity of a solution and the molecular weight of the dissolved polymers. He called it the viscosity “law”. He used wooden sticks as a model of macromolecules. Staudinger was an excellent organic chemist but his abilities in physical chemistry were quite limited. His interpretation of the viscosity “law” was short-sighted. Hermann Mark and Werner Kuhn on the other hand were excellent physical chemists. Kuhn did not believe in Staudinger’s sticks-model and his viscosity law. In 1932 Kuhn began to develop the model of the statistical coil and a different formula for the viscosity of polymer solutions. Staudinger insisted on his views. I found several letters between Staudinger and Kuhn in the archives of the University of Basel<sup>2</sup> which were written in 1932 and 1933.

Here, I translate some sentences from these letters of Staudinger. He wrote: *“In my opinion, from the standpoint of organic chemistry, it is impossible to assume such a coiled form of molecules, as you did it. The molecules must be visualised as long stiff rods, which of course, because the double bonds are free to rotate (sic!), can perform certain swinging motions; on the average though the molecules have a straight, elongated form, otherwise the viscosity laws could not be understood”*. Staudinger was always very concerned with questions of priority. This is shown by the following sentence: *“...to support this research area I must insist today on clarity and I must contradict a treatment of this area, like it is done e. g. by H. Mark, indicating that the relations between viscosity and molecular weight have been known since long.”* Werner Kuhn did not agree with Staudinger. In his answer he wrote: *“In any case at present the conclusion can not be denied that my ideas about the development of viscosity are in no way compatible with the hypothesis of binding of solvent to rod-like particles. Also your result of a specific*



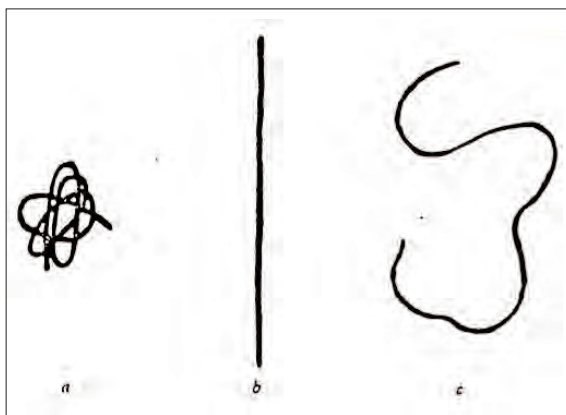
**Model of a statistical coil in Werner Kuhn’s Institute about 1963. By Courtesy of W. G. Pohl.**



viscosity being proportional to the length of the chain cannot be explained by the assumption of nearly straight rigid chains.” There is no answer of Staudinger to this letter in the Basel archives. But in a meeting of the German Bunsen-Society in 1934 Staudinger again used his model of long straight chains. He supported his view by definitely wrong arguments: “*The stiffness of organic molecules finally can be explained by the fact – contradicting usual reasoning – that carbon-atoms with a single bond cannot rotate freely (sic!), but are fixed like carbon double-bonds.*” Another sentence reads: “*Also in solution molecules are on the average rodlike as in a crystal. They are not coiled or forming a helix as has been assumed recently several times.*” Staudinger adhered stubbornly to his old view. What had been very positive for introducing his idea of chainlike macromolecules into classical organic chemistry against the resistance of the best organic chemists was now a senseless fight against new results of physical chemists. In 1935, Staudinger and Wallace H. Carothers (1896-1937) presented overviews of their seminal works made in the previous decade at the Faraday Society Symposium at Cambridge UK. This conference sponsored by the British society for physical chemistry, was the first international conference at which macromolecules were taken for granted and can be regarded as the end of the macromolecular debate.

Staudinger refused to accept the picture of coiled macromolecules until 1951. At a party in his institute one of his co-workers wrote a verse saying: “*Die Kuhn’schen Knäuel sind uns ein Gräuel*” (*Kuhn’s coils are a nightmare for us*).”

Werner Kuhn and others over the years developed a more general viscosity formula, which includes different conformations of dissolved chain-molecules from collapsed little spheres over expanded spheres to straight rod-like molecules. In the latter case a quadratic dependence of viscosity on



Mark's sketch of polymer molecules in solution. From: *Trans. Faraday Soc.* 32 (1936), Pt. 1, p. 312.

1930 Staudingers viscosity “law”  $[\eta] = K \cdot m$   
This would only apply for molecules structured like c (In the sketch).

K ..... Constant

M ..... Molar Mass of Polymer Molecules

1940s improved formula developed by Werner Kuhn, Mark, Houwink and Sakurada  $[\eta] = K \cdot m^a$

Molecules structured like

a are collapsed coils ..... exponent a = 0

b are rigid rods ..... exponent a = 2

c are expanded coils ..... exponent a > 0,5

molecular weight is observed in contrast to Staudinger's initial linear relationship.

When Staudinger received the Nobel Prize in 1953 he "jumped over his shadow" and mentioned Kuhn's coils in his Nobel-Lecture as an accepted fact.

## Conclusions

A new discipline of chemistry, called macromolecular or polymer chemistry was initiated by the efforts of Hermann Staudinger between the two world wars. Following the initial years dominated by research at universities, polymer chemistry has become one of the leading branches of chemical industry. This paper tells the story of how Staudinger, after the First World War, persuaded committed supporters of the micellar theory to change their views and how he won over the young chemists of the coming generation. It is significant that two key converts to the macromolecular concept in this period were Mark and Kuhn, both still in college in 1920. Herman Mark had no connections with Staudinger during that period and only became his friend in the 1950's. After the debate over macromolecules had been resolved in the early 1930s, attention turned to physical chemistry of polymers, e. g. the shape of polymers in solution and to the elasticity of rubber. A pioneering figure in this field was the Swiss physical chemist Werner Kuhn, who fought against the wrong ideas of Staudinger during the 1930s. Paul Flory (1910-1985) paid tribute to Kuhn's work in his Nobel lecture in 1974. In the preface of his book, "Statistical Mechanics of Chain Molecules", published in 1969 he had written: *"The late Werner Kuhn directed attention as early as 1934 to the correspondence between a real polymer chain of sufficient length and its mathematically tractable analog...this fact pays tribute to Kuhn's insight at a time when the prevalence of linear chain structures... was only beginning to be perceived."*

## Epilogue

In addition to his pioneering work on polymers, Werner Kuhn produced significant results in several other branches of science. As mentioned earlier he predicted the existence of the Mößbauer-effect in 1927, thirty five years before it could be shown experimentally. This was mentioned 1961 by Rudolf Mößbauer (b. 1929) in his Nobel Lecture. In 1933 Kuhn carried out the first photochemical separation of isotopes. After going to Basel in 1939, he studied the theory of distillation and constructed a new type of distillation column that could be used to obtain heavy

water, a technique which has been used industrially. Kuhn's theory of multiplication of small separation steps was further used to explain the process of urea concentration in the kidney and the creation of high pressures in the swimbladder of fishes. He even worked out, together with a geologist, a new theory of the build-up of the earth's interior.

## Notes

<sup>1</sup> Über die direkte Umwandlung von chemischer Energie in mechanische Arbeit mit Hilfe von Polyelektrolytgelen, Auszug aus der von der Philosophisch-Naturwissenschaftlichen Fakultät der Universität Basel genehmigten Dissertation. W. Gerhard Pohl, Hans J. Kuhn, Werner Kuhn, *Z. Naturforschung* 21a, (1966): 756-763.

<sup>2</sup> Nachlass W. Kuhn bei UB Basel, A II 116, 4 (5. August 1932), A II 116, 6 (12. August 1932), A II 116, 8 (18. 3.1933). I want to thank the University Library in Basel for supplying these references.

## References

- Flory, Paul, *Statistical Mechanics of Chain Molecules* (New York, Interscience, 1969).
- Furukawa, Yasu. *Inventing Polymer Science, Staudinger, Carothers and the Emergence of Macromolecular Chemistry* (Philadelphia, University of Pennsylvania Press, 1998).
- Guth, Eugen und Mark, Hermann. Zur innermolekularen Statistik, insbesondere bei Kettenmolekülen I, *Monatshefte f. Chemie* 65 (1934): 93-121.
- Kuhn, Hans, Werner Kuhn (1899-1963). Das Portrait, *Chem. unserer Zeit* 19 (1985): 86-94.
- Kuhn, Hans Jakob. Werner Kuhn 1899-1963 in memoriam, *Verhandl. Naturf. Ges. Basel* 74 (1963): 239-258.
- Kuhn, Werner. Über die Gestalt fadenförmiger Moleküle in Lösung, *Kolloid-Z.* 68 (1934): 2-15.
- Mark, Herman F. *Autobiography in McGraw-Hill Modern Scientists and Engineers*, Vol. II (New York, McGraw-Hill, 1980).
- Mark, Herman F. *Autobiography*, in *Contemporary Topics in Polymer Science. Vol. I: Macromolecular Science: Retrospect and Prospect*, ed. R. D. Ulrich (New York, Plenum Press, 1978), 123-131.
- Morris, Peter J. T. *Polymer Pioneers, A Popular History of the Science and Technology of Large Molecules* (Philadelphia, University of Pennsylvania Press, 1989).
- Priesner, Claus. *H. Staudinger, H. Mark und K. H. Meyer: Thesen zur Größe und Struktur der Makromoleküle* (Weinheim, Verlag Chemie, 1980).
- Ringsdorf, Helmut. Hermann Staudinger und die Zukunft der Polymerforschung, Jubiläumsfeiern – selbstbestimmter Anlass kultureller Frömmigkeit, *Angew. Chem.* 116 (2004): 1.082-1.095.
- Staudinger, Hermann. Über Polymerisation *Ber. dtsh. chem. Ges.* 53 (1920): 1.073-1.085.
- Staudinger, Hermann. *Arbeitserinnerungen* (Heidelberg, Hüthig Verlag, 1961).
- Stuart, H. A. *Die Physik der Hochpolymeren* (Berlin-Göttingen-Heidelberg, Springer 1952).

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## Herman F. Mark (1895–1992): Viennese Born ‘Ambassador’ of Macromolecular Research

*Johannes Feichtinger\**

Many different aspects can be told about the life of Herman Francis Mark: The story of Austria’s dealing with the legacy of “the most distinguished chemist”<sup>1</sup> of pre-war Austria, today; the story of not re-employing Mark in Austria or Germany after World War II, but of granting him awards for his academic achievements; or the story of the making of Mark’s biography.

Studying the available biographical sources, memoirs and autobiographical notes, it is not easy to escape the narrative of tremendous success told of him by former students, colleagues and by himself. In honour of his birthdays late in life many highly sponsored international symposia were held. For his 85<sup>th</sup> birthday, for instance, seven major celebrations were held to commemorate Marks scientific achievements: one each in New York, Philadelphia, Chicago, Moscow and Germany, and two in Israel. Once, Mark said: “My birthday is like the Salzburg Festival”.<sup>2</sup>

Many scientists, who later became world-famous, have been students of Mark, for instance: Edward Teller, Leo Szilard and Marx Perutz. The double Nobel Prize winner Linus Pauling declared himself repeatedly ‘deeply indebted’ to Mark,<sup>3</sup> “I think of him, with affection and admiration, as a pioneer in modern structural chemistry and an important early contributor to its development”.<sup>4</sup> After Mark’s death at the age of 97 his autobiography was published in the series “Profiles, Pathways and Dreams: Autobiographies of Eminent Chemists”, under the (sub-) title *A Century of Progress*.<sup>5</sup> This book rounded off the many publications on his splendid biography. A memorial conference was organised that paid tribute ‘to the greatness’ of the celebrated researcher and academic teacher.<sup>6</sup> In fact, he had received more than a dozen honorary doctoral decrees. He was also an elected member of more than two dozen Academies of Sciences and he had received the highest awards world wide, with one exception, the Nobel Prize.

Decades ago, Mark –the chemist, physicist and inventor– was also honoured, but not celebrated, in Austria. However, compared to other Austrian refugee scholars

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(for example, Karl Popper, Erwin Schrödinger, and Sigmund Freud) the name of Austria's once most distinguished chemist is not now in the public eye of Vienna.

The undivided international acknowledgement in the world of science rests on Mark's important academic work and achievements. To put it briefly: Firstly, on his advancement of a new academic discipline and a new field of research. He was acknowledged as the 'father of Polymer Science'. However, Mark's approach to the study of polymers had much wider implications. The study of macromolecules made him not only a pioneer in polymer science, but also an initiator of the beginnings of structural chemistry. Max F. Perutz, who had been placed as a student with Desmond Bernal in Cambridge in 1936, would later use the method of his teacher Hermann Mark, x-ray crystallography –to solve the alpha helix structure of haemoglobin. Deciphering the alpha helix paved the way to solve the structure of more complex proteomic systems. Students of Perutz, Watson and Crick, elucidated the double helix of DNA. Thus, the methodological approach, the Viennese chemist Mark had applied to polymer science, paved the way to the study of proteins and to other advances in molecular biology.

Secondly, Mark was acknowledged both as an industrial chemist and as well as an academic. He had realised very early the importance and advancement of polymer chemistry as a new branch of the subject. In the interwar period, the research on synthetic polymers had not created much impression among the advocates of traditional organic and physical chemistry in the universities, although cellulose had been utilised on an industrial scale as early as 1900. However, Mark had recognised the mood of the times. Ambitious to solve relevant problems, he had started to investigate the structure of natural polymers, in order to elucidate its influence upon their technical properties, such as strength, elasticity, softening point etc. These investigations were crucial for the further development of synthetic fibres. Industry and non-university research institutions were the first to recognise the importance of fundamental and applied polymer research. The industrial research done during the war was primarily empirical. Thus it remained for Hermann Mark at IG-Farben-Industry in Ludwigshafen, and Wallace H. Carothers at DuPont Company in Wilmington, Delaware to take a step forward. The work associated with Mark and Carothers signalled a breakthrough. Mark's research advanced the change in the approach to polymers, that is to say, from an empirical practise to theoretical, science based approach.

In the 1930's the research group directed by Carothers was to announce the synthesis of a synthetic polymer which, for the first time in history, had properties superior to natural fibres. The polymer was called 'Nylon'. Just when the true magnitude of the discovery of nylon was becoming apparent in 1937, "worsening

bouts of depression” prompted Carothers’ suicide<sup>7</sup>. However, within academic circles, Carothers’ work was not considered as very serious science, as his position was that of an industrial chemist, in contrast to Mark, who had gained experience in both industrial and in academic research.

Thirdly, Mark’s greatest accomplishment was, as his son Hans put it, as a pioneer in applying modern physics to chemistry.<sup>8</sup> Hermann Mark became one of the leading investigators in the field of the use of an infant analytical technique, x-ray diffraction, which provided irrefutable proof for the macromolecular structure of colloidal substances, like cellulose, rubber and silk. Mark’s experience in x-ray work deepened the understanding of the polymer structure, which had been crucial for the further development of synthetic fibres, such as nylon, acrylic and polyester.

However, it remained for Mark to finally contribute decisively to the acceptance of polymer chemistry as a truly scientific discipline, which deserved to be promoted and incorporated in the academic curricula. Mark, who had been forced to escape the Nazi terror in Austria, became an “agent of change”, as Norbert M. Bikales of the National Science Foundation (Washington) put it in a lecture at the Memorial Symposium for Herman Mark in 1992: “He was future oriented until the end. More than any other person he was responsible for spreading the gospel of macromolecules” in the United States, where serious academic research in polymers had not existed before he arrived, and beyond.<sup>9</sup> The only polymer scientist working in an academic position was Carl S. Marvel at Illinois University. Marvel had done work in synthesizing polymers but had not been interested in how they behaved. It was Mark who became the “ambassador” for polymer chemistry and education.<sup>10</sup>

The achievements ascribed to Mark are impressive. They cannot be understood fully without considering the historical circumstances of the advancement of Mark’s scientific career. In 1932, when Hermann Mark was appointed professor and director of the so called “First [I.] Chemical Laboratory” of the University of Vienna<sup>11</sup>, he brought a formidable scientific career in industry to a satisfactory but temporary end. After having finished studies in organic chemistry in Vienna in 1921, he changed to the University of Berlin as a post-doctoral instructor. Soon Fritz Haber recommended him to move to the Kaiser Wilhelm Institut für Faserstoffchemie, the newly established fibre Research Institute. This institute had been the first to study the structure of high-molecular-weight natural substances by the application of x-ray techniques. The ultimate goal of studying “Faserstoffe” was the establishment of quantitative values in the structure of organic molecules. At the Institute for Chemistry of Fibrous Material Mark esta-

blished himself as an expert crystallographer, who managed to decipher structures of inorganic and of organic substances.

In 1927 Mark accepted the position of a research manager in the laboratories of I.G. Farben Industries, Germany's largest chemical corporation at that time. There, work was conducted by a group of physicists, chemists, and engineers. The x-ray studies led to the determination of the structure of cellulose, silk, rubber and other natural high polymers. Their work concentrated on the investigation of organic structures for correlation with their technical properties, such as strength, elasticity, water retention, etc. The purpose of research at Ludwigshafen was clear, the goal of developing synthetic substitutes for natural fibres. For this purpose, Mark coordinated and organised research on macromolecules in three sections, synthesis, characterisation and application. Since this model had proved to be of value, he would later organise research on this manner in Vienna and New York. The collaborative research was published in the famous book *Der Aufbau der hochpolymeren organischen Naturstoffe*,<sup>12</sup> edited by Mark and by the head of the research laboratory, Kurt H. Meyer.

When the political climate worsened in Germany in 1932, the son of a Protestant father with Jewish descent was warned not to continue to hold the position at Ludwigshafen. Mark was prompted to accept a chair at Vienna University, where he was appointed Professor of Chemistry in 1932. Mark succeeded Professor Rudolf Wegscheider, and he re-organized the "First [I.] Chemical Laboratory". At that time structural chemistry was an unusual, one might say, a marginal field of academic research. Structural chemistry was at the time an industrial domain; academically it was not yet considered a serious discipline. Nevertheless, Mark introduced x-ray analysis, and concentrated on the design of the first academic curriculum for polymer technology. Thus he founded the first comprehensive polymer research and teaching institute.<sup>13</sup>

In the 1930s the Viennese academic system was in a bad condition. Anti-Semitism was at least as strong as in Germany. Young Jewish scholars and scientists were prevented from following academic careers. Thus, much of the academic discourse took place outside of the university. In interwar Austria, many new approaches to scholarship and science were developed at a non-university level. Thus, most of today's renowned intellectuals were not part of the academic system rather they belonged to private research groups and academic circles. In fact, third party encouraged or financed research projects turned out to be most innovative. Some of the then marginalised Viennese scientists and scholars went on to significantly transform science and scholarship.<sup>14</sup>



The Viennese School of high-polymer research, developed on both a university and on a non-university level: The “First [I.] Chemical Laboratory”, which was directed by Hermann Mark, was part of the Vienna University. The “Second [II.] Chemical Laboratory” was headed by Professor Dr. Ernst Späth until his death in 1946. As a Professor, Mark was supposed to educate people. However, apart from producing chemists, Mark carried out research that was financially supported by IG Farben. Doing research in the new field of polymers seemed to be attractive for young scientists. The younger generation was attracted by the ongoing controversies in the age of the foundation of polymer science. Thus Mark’s research programme attracted a number of scientifically ambitious doctoral students in chemistry, physics and mathematics, who were considered to be Jews. After World War I they rarely had a chance to obtain a post in Austria because of Anti-Semitic discrimination.

Today, one knows of many young Jewish scientists, excluded from academic careers, who have moved into cognitively underdeveloped academic fields, so-called ‘border sciences.’ In Germany, they were not allowed to compete scientifically with the Professors, who occupied the centre of the discipline; thus, they moved to peripheral universities.<sup>15</sup> In Austria, marginalised scholars remained located in a geographical sense in the centre, in Vienna. However, they organised themselves informally outside of the universities, setting up, as already mentioned, a second “scholarly culture”.<sup>16</sup>

When Mark was appointed professor, he switched over to basic research, since the synthesis of new monomers and polymers could be better performed in Ludwigshafen. He concentrated upon the mechanisms of polymerisation. Hermann Mark hired a number of doctoral students and young assistants, among them were Johann Wolfgang Breitenbach, Eugene Guth, Friedrich Eirich, Robert Simha, Engelbert Broda and Max Perutz. They were set to determine the relationship between molecular weight and viscosity. Mark initiated the statistical treatment of macromolecular substances. The most important achievement was the formulation of a statistical theory of the elasticity of rubber-like molecules and the mechanical properties of rubber. Many of Mark’s collaborators were Jewish by origin, as Mark would soon become himself, according to the Nuremberg laws.

In 1937, when the Nazis were ready to take over Austria, the manager of the large Canadian International Paper Company, Dr. C. B. Thorne, submitted an offer to Mark to take over the position of a research manager in the central laboratory in Hawkesbury, Ontario, Canada. In April 1938, after having been dismissed and arrested for interrogation, Mark accepted the Canadian offer. The visa-problem was solved by the Canadian ambassador. Mark handled the money-transfer pro-

blem scientifically: He bought platinum/iridium wire from a number of Viennese suppliers, and bent the wire into coat hangers. Hermann Mark knew he had to leave Austria, and he took his wife, their children and a Jewish niece in an adventurous trip by car. They mounted a Nazi flag on the radiator of their car, strapped ski equipment to the roof, and drove from Vienna into Switzerland on May 10<sup>th</sup>, 1938.<sup>17</sup> As Mark himself, most of his Jewish scientific collaborators also had to leave Austria. Though with one consequence, on February 7<sup>th</sup>, 1939 the Manchester Guardian had to report of the “break up of the School of Chemistry which Professor Mark had organised in Vienna”.

Concerning the careers of Austrian academic refugees one important fact may be noticed, namely, the significant correlation between, what might be called, the ‘paradigmatic youth’ of a field of scientific research and the proportion of refugee scholars from it. In other words, the more the scientists shifted away from the dominant paradigm, the higher the probability of their becoming émigrés during the rule of National Socialism, and also the higher the probability of their fitting in culturally more successfully overseas and of their continuing academic careers in the Anglo-American world. To put it briefly, there was an obvious relation between social marginality, innovation and the continuation of academic careers in the countries of refuge. This fact can be confirmed in many disciplines, from which scholars and scientists emigrated after 1933 and 1938 respectively; among them those in the fields of economics, the social sciences, humanities and the natural sciences<sup>18</sup>, in chemistry in particular. As to the members of the research group of Hermann Mark, their expulsion turned out to be the springboard for successful academic and professional careers in the Anglo-American world.

By the time Mark had to leave Austria in 1938, he had made important contributions to the field of polymer science. First, he had initiated a theory of the mechanisms of polymerisation processes. Second, he had experimentally proofed the influence of the structure of natural and synthetic polymers upon properties, and third, his systematic investigations on the relationship between structure and properties of macromolecular systems provided the knowledge for the design of new polymers with certain desired and required properties.

In 1936, the Rockefeller Foundation complained about the relative weakness of those fields of chemistry in the U.S., which were meant to contribute most directly to organic structural chemistry, a field of research notably developed in Europe. Thus, Mark’s qualifications gradually gained in importance. He was already then recognised as one of the world’s most important high polymer researchers. Since there had not yet been any organised academic research in high polymer chemistry in North America, Mark soon acted as intermediary in this new discipline,

transferring the European style of physically based research in polymer chemistry to Canada and the United States of America.

In the autumn of 1938, after his arrival in Canada, Herman Francis Mark, as he now called himself, was appointed research manager for the Paper Company in Hawkesbury. He immediately set about modernising the world’s biggest paper plant and supplier of cellulose, introducing new theories and methods, for instance, in the areas of the influence of molecular weights on the mechanical properties of organic and synthetic fibres. The Canadian International Paper Company was the main supplier of cellulose for the DuPont Company. At that time the DuPont Company was the largest producer of rayon in America. They were interested in the characterisation of cellulose. Since none of the American universities had departments of polymer science at that time, DuPont placed Mark as an Adjunct Professor at the Polytechnic Institute of Brooklyn (New York) in 1940. After the war he was appointed to the first created Professorship in Polymer Science and established polymer chemistry as a part of the general chemistry curriculum.

The archival sources do not verify Mark’s official story of straight away emigration from Austria to North America. After Hitler’s occupation of Austria, the ‘Anschluss’, the London Society for Academic Refugees tried to rescue Mark and seven other eminent Austrian scientists for British industry or universities. Among them, were three Austrian Nobel Prize winners (Franz Viktor Hess, Otto Loewi and Erwin Schrödinger). Outstanding British scientists (Joseph Needham, Eric K. Rideal and Robert Robinson) and industrial tycoons had described Mark as “one of the best researchers living in the field of x-ray analysis of solid bodies, such as biological structures and polymers”.<sup>19</sup> They organised a research fellowship at Balliol College, Oxford and assured Mark, that he and his family “would not starve”, if he decided for England. Mark declined the offer which had been delivered by the British intelligence service in Vienna. England seemed to him not to be the right place to go. However, after having spend one year in Hawkesbury, Mark felt isolated there. He was then even ready to come to England as a ‘voluntary’ and to put his abilities at the disposal of war chemistry. Mark’s unique knowledge was much appreciated by his British colleagues. W. Lawrence Bragg offered him a fellowship at Cavendish laboratory, and other academic and industrial offers were made. However, the permission to go was rendered valueless as the academic appointment in New York prevented Mark from leaving America. Soon, Mark was to become involved in a number of military projects but only a few of them had to do with polymers.

The so-called Shellac Bureau, whose function was the testing and chemical characterisation of shellac, was based in Brooklyn. Brooklyn was the major port of entry for shellac. Shellac, a natural polymer, was used in the optical industry, dental technology, and for phonograph records, pressed from shellac compounds. Since shellac needed to be imported from Asia, the war stimulated a search for a synthetic substitute. In respect to this, Mark's research and consulting experience at I.G. Farben was most valuable. He was well informed about synthetic resins with properties even superior to those of shellac. Mark was assigned to the Shellac Bureau, and in 1941 he was named director.

During the war, the United States lost access to natural rubber. The impact of the developing rubber shortage stimulated the government to sponsor a synthetic rubber research program. Soon about 2500 to 3000 chemists and engineers were occupied with research into synthetic rubber. Mark was familiar with the know-how, since he had worked in a synthetic rubber plant. Buna S and Buna N had been developed at I.G. Farben laboratories. Mark was assigned to a committee that tried to help the industry develop synthetic rubber technology. With Mark's collaboration, the American rubber industry was able to develop synthetic rubber and to produce the necessary quantities of rubber needed during World War II.

Simultaneously with his war work engagements, Mark was working to strengthen polymer research and education at 'Brooklyn Poly.' When Mark had joined the school, Polymer science was not as yet very prestigious. However, during the war it became a new interdisciplinary and academic branch of chemistry. Industry had sensed its novelty and utility. In effect, the Polymer Research Institute was established and Brooklyn Polytechnic became the first institute in the United States dedicated to polymers. Mark used the experiences he had collected in Berlin, Ludwigshafen and Vienna. He designed the first post-graduate program of polymer studies in America in a similar way to that which he had done at the University of Vienna a decade earlier, though on a much larger scale.<sup>20</sup>

After World War II Mark became one of the most important exponents of principles of polymer science: firstly, in regard to interdisciplinarity. Mark created the Polymer Research Institute in Brooklyn based on his experience-based conviction that systematic progress could only be expected, if organic chemists, physical chemists and at least one physicist were assembled into a team to conduct research. According to this he organised the Institute in three sections following the Viennese model, synthesis, characterisation and application of high polymers. Secondly, by establishing co-operations between industry and fundamental academic research. Thirdly, as the interest increased rapidly, Mark initiated the publication of a series of monographs on 'High Polymers'. In 1946 he started publishing

the Journal of Polymer Sciences; and finally, in assembling a number of qualified co-researchers.

Mark also played the role of a mediator and recruiter for some of his former Viennese collaborators. As polymer research was still in its infancy in America, over the course of the 1940s Mark brought over some former Viennese colleagues to join him, the physicist Robert Simha, the organic chemist Robert Raff and the physical chemist Frederic R. Eirich, all of whom had left Austria after the ‘Anschluss’ (1938) and were scattered across the British Empire. Other former students did not comply with Mark’s request, for example, the Austrian nuclear physicist Engelbert Broda decided to stay at the Cavendish Laboratory in Cambridge, where he was working on nuclear chain reaction, and Max Perutz was already too involved in preparing his masterpiece –the analysis of the structure of haemoglobin, for which he earned the Nobel Prize in 1962.

After the war Mark’s former students dispersed and founded new small research groups on polymer chemistry at many universities. Chemical companies also established research laboratories that absorbed the growing number of academic graduates.

It is noteworthy, that the Polymer Research Institute, nowadays located at the renamed Polytechnic New York University, still adheres to the research design introduced by Hermann F. Mark, with sections devoted to synthesis, characterisation, and application. However, what was new and barely recognised in the scientific field in the 1940s, is today called traditional chemistry. As the public web page<sup>21</sup> tells us, the Polymer Research Institute has recently expanded its interests “in addition to the traditional chemistry related areas in macromolecular technology to health related areas.” No doubt, the striking of this new path would have been in the spirit of Herman Francis Mark, whose academic achievements deserve a more detailed historical study.<sup>22</sup>

## Notes

<sup>1</sup> Walter Adams (Society for the Protection of Science and Learning, London) to The Under-Secretary of State, May 19<sup>th</sup>, 1938. Personal file of Hermann Mark 219/9; 437/1 at the Archive of the Society for the Protection of Science and Learning (today CARA), Bodleian Library, Oxford.

<sup>2</sup> Jeffrey I. Seeman, “Editor’s Note”, in Herman F. Mark, *From Small Organic Molecules to Large. A Century of Progress* (Washington: American Chemical Society, 1993), XI-XXV, on XXIII.

<sup>3</sup> Linus Pauling, “My Indebtedness to Herman Mark”, in *Polymer Science in the Next Decade. Trends, Opportunities, Promises. An International Symposium Honoring Herman F. Mark on His 90<sup>th</sup> Birthday*, ed. Otto Vogl, Edmund H. Immergut (New York: John Wiley & Sons, 1987), 19-22.

- <sup>4</sup> Linus Pauling, "Pauling on Mark" in *Pioneers in Polymer Science*, ed. Raymond B. Seymour (Dordrecht/Boston/London: Kluwer Academic Publishers, 1989), 153-163, on 163.
- <sup>5</sup> Herman F. Mark, *From Small Organic Molecules to Large. A Century of Progress* (Washington: American Chemical Society, 1993).
- <sup>6</sup> *Polymers to the Year 2000 and Beyond: A Memorial Symposium for Herman F. Mark*, ed. Sheldon M. Atlas, Eli M. Pearce, F. R. Eirich (New York: John Wiley & Sons, 1993) (*Journal of Polymer Science: Polymer Symposia* 75).
- <sup>7</sup> See *Chemical Achievers. The Human Face of the Chemical Sciences*. Cf. <<http://www.chemheritage.org/classroom/chemach/plastics/carothers.html>>.
- <sup>8</sup> Hans Mark, "Herman F. Mark. The Man", in *Polymer Science Overview. A Tribute to Herman F. Mark*, ed. G. Allan Stahl (Washington: American Chemical Society 1981) (ACS Symposium Series 175), 89-92.
- <sup>9</sup> Norbert M. Bikales, "Herman Mark's Children", in *Polymers to the Year 2000 and Beyond: A Memorial Symposium for Herman F. Mark*, ed. Sheldon M. Atlas, Eli M. Pearce, F. R. Eirich (New York: John Wiley & Sons 1993) (*Journal of Polymer Science: Polymer Symposia* 75): 143-150, on 143.
- <sup>10</sup> F. R. Eirich, "Remembering Herman F. Mark", in *Polymers for Advanced Technologies* 3 (1992): 102-104. See National Historic Chemical Landmarks: <[http://acswebcontent.acs.org/landmarks/landmarks/polymer/pol\\_1.html](http://acswebcontent.acs.org/landmarks/landmarks/polymer/pol_1.html)>.
- <sup>11</sup> Herman F. Marks scientific course of life is documented in an overview on the Internet page of the Chemical Heritage Foundation (<<http://www.chemheritage.org>>) Further material is available on the Internet page of the American Chemical Society (<<http://portal.acs.org/portal/acs/corg/content>>) resp. *National Historic Chemical Landmarks* (<<http://acswebcontent.acs.org/landmarks>>).
- <sup>12</sup> Kurt H. Meyer, Hermann Mark, *Der Aufbau der hochpolymeren organischen Naturstoffe auf Grund molekular-morphologischer Betrachtungen* (Leipzig: Akad. Verl.-Ges., 1930).
- <sup>13</sup> Herman Mark, "Polymer Chemistry in Europe and America – How It All Began", *Journal of Chemical Education*, 58 (1981): 527-534, on 531-532. See also Ute Deichmann, *Flüchten, Mitmachen, Vergessen. Chemiker und Biochemiker in der NS-Zeit* (Weinheim: Wiley-VCH, 2001), 181-187.
- <sup>14</sup> Johannes Feichtinger, *Wissenschaft zwischen den Kulturen. Österreichische Hochschullehrer in der Emigration 1933-1945* (Frankfurt am Main: Campus, 2001).
- <sup>15</sup> Shulamit Volkov, "Soziale Ursachen des Erfolgs in der Wissenschaft – Juden im Kaiserreich", *Historische Zeitschrift* 245 (1987): 315-342.
- <sup>16</sup> Johannes Feichtinger, "Kulturelle Marginalität und wissenschaftliche Kreativität. Jüdische Intellektuelle im Österreich der Zwischenkriegszeit", in *Das Gewebe der Kultur. Kulturwissenschaftliche Analysen zur Geschichte und Identität Österreichs in der Moderne*, ed. Johannes Feichtinger, Peter Stachel (Innsbruck/Wien/München: Studienverlag, 2001), 311-333. See also Johannes Feichtinger, "Zur Migration, Akkulturation und Identität österreichischer Intellektueller und Wissenschaftler 1900-1945", in *Österreichische Satire (1933-2000). Exil – Remigration – Assimilation*, ed. Jeanne Benay, Alfred Pfabigan, Anne Saint Sauveur, Bern: Lang, 2003 (*Convergences* 29), 15-38.
- <sup>17</sup> Mark, *From Small Organic Molecules to Large*, 85. See also personal file of Hermann Mark 219/9; 437/1 (Society for the Protection of Science and Learning, Bodleian Library).
- <sup>18</sup> Feichtinger, *Wissenschaft zwischen den Kulturen*, 31-54. Johannes Feichtinger, "Migration – Cultural Transfer – Scientific Change. Austrian Scholarly Traditions and their Impact on

Scholarship and Science in the Americas 1933–1945”, in *Reverberations. Representations of Modernity, Tradition and Cultural Value in-between Central Europe and North America*, ed. Susan Ingram, Markus Reisenleitner, Cornelia Szabo-Knotik (Frankfurt am Main: Lang, 2002), 223-239.

<sup>19</sup> Joseph Needham to Walter Adams, June, 14<sup>th</sup>, 1938. Personal file of Hermann Mark 219/9 at the Archive of the Society for the Protection of Science and Learning, Bodleian Library, Oxford.

<sup>20</sup> Mark, “Polymer Chemistry in Europe and America – How It All Began”, 532-534. Herman Francis Mark. Transcript of a Tape-recorded Interview conducted by R.S. Marvin, March 19th 1979, archived by the American Institute of Physics, Center for History of Physics, College Park, MD.

<sup>21</sup> Polymer Research Institute. <<http://www.poly.edu/researchcenters/pri/index.php>>.

<sup>22</sup> See the first draft of a planned study on the ‘Wiener Schule der Hochpolymerforschung in England und Amerika. Emigration – Wissenschaftswandel – Innovation’, manuscript available in German: <[http://www.oew.ac.at/kkt/mitarbeit/fei/fei\\_v99\\_e.html](http://www.oew.ac.at/kkt/mitarbeit/fei/fei_v99_e.html)>.



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## **Reminiscences of Professor Georges Smets (1915-1991). On the development of macromolecular chemistry in Belgium and his contributions to it in the international polymer chemistry community**

*Marcel Van Beylen\**

On January 28, 1991 about 15 minutes before I was to deliver my weekly lecture on macromolecular chemistry, which Professor Smets had given for 40 years, the secretary told me with tears in her eyes that Professor Smets had died. He died at the age of 75 about 7 years after retiring and becoming an Emeritus Professor, after 40 years of a very productive career.

Polymer chemistry in general, and particularly in Belgium where he was the first to start with it, is inextricably connected with him in person.

After brilliant studies at the Catholic University of Louvain (bilingual at the time) he obtained a PhD in Chemistry but also the degree of Pharmacist. After a short period as a researcher at the Society GEVAERT (later Agfa Gevaert) he was appointed at the University of Louvain in 1944, where he created the laboratory of macromolecular chemistry.

Few of those who had the privilege to know him as a teacher, as their thesis supervisor, as a colleague, as a friend are able to imagine polymer chemistry in Belgium and elsewhere in the polymer world, without Professor Smets' contributions. Indeed, Professor Smets was involved in the early stages of polymer chemistry and over the years, he has not only enthusiastically shared his knowledge with his students, but exported it all over the world.

Numerous people, both in the scientific and the industrial world, have sought his advice over the years, very understandably so, because Professor Smets has made many fundamental contributions to nearly all fields of macromolecular chemistry. He was one of the first to show successfully the possibilities of block- and graft copolymerisation by the radical transfer reaction between a growing chain and a pre-existing polymer. By incorporating peroxide groups into polymers or by put-

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ting them at their ends, several original methods of graft and block-copolymerisation by radical mechanisms were established years before anionic polymerisation later became the customary way to produce them. Later, photochemical initiation procedures based on the photolysis of photosensitive side groups or groups built into the polymer chain were also used.

Professor Smets' background in organic chemistry led him to extensive studies of organic reactions on high polymers and intramolecular functional interactions, in the course of which he was one of the first to draw the attention to the influence of the intramolecular structure and stereospecificity of the polymers formed during the reactions and on the nature of group interaction.

Knowledge of organic chemistry and its recent progresses were at the origin of Professor Smets' skills in the synthesis of new polymers using recent methods of organic chemistry such as cyclodimerisation, dipolar cycloadditions, carbene and azide reactions, either for the preparation of new monomers followed by their polymerisation or for direct polyaddition reactions.

Another of his many fields of interest was the synthesis of properties of photochromic polymers and copolymers. Incorporation of photochromic groups into a polymer molecule yielded deep insights in the internal structure and the physical properties of polymers such as chain segment mobility and transition phenomena. He also observed photomechanical phenomena in the case of photochromic networks, which showed photocontractile behavior.

These phenomena led him to study several photochemical and thermal reactions, e.g. isomerisation, dissociation, and recombination in solid polymer matrices, stressing the importance of the physical properties of the polymer medium on the course of these reactions. The above is not an exhaustive survey, as Professor Smets also treated problems like anionic polymerisation and copolymerisation and the synthesis of polyampholites, their complexation behavior towards metal salts and their significance for selective membrane synthesis.

In view of the main research areas of Professor Smets it is not surprising that the IUPAC Polymer Chemistry Meeting held in his honour in the autumn of 1984, in Leuven, was mainly devoted to "Block and Graft Copolymerisation and Mechanisms, Polymer Transformation Reactions and Polymer Photochemistry".

Well aware also of the importance of the physical properties and the potential applications of the polymers synthesised, Prof. Smets also created a section of physical chemistry and physics of polymers and took in some more physically oriented people in his laboratory.

At this time the synthetic polymer chemistry is still flourishing in combination with the laboratory of electronics and photonics to produce, under the guidance of a young research associate,

present-day polymers with special optical and magneto-optical properties. He is keeping up the former level of the laboratory.

The originality and significance of the work of Professor Smets found broad international recognition. Indeed, not only have a great many scientists from abroad visited the Laboratory of Macromolecular and Organic Chemistry of Leuven over the years and have several post-doctoral researches spent one or two years to work under Professor Smets' guidance, but also over the years, Professor Smets' honours have accumulated to an impressive list of awards, honorary degrees and nominations as a member of different organisations and societies including that of being President of IUPAC, 1977-1979.

Besides his activities as the leader of a large research group he has been an inspiring and appreciated teacher since 1944. Many of his former students are now distinguished by their own work initiated under his guidance in Leuven. Many of them hold now Professorial positions in various Belgian as well as foreign universities.

So far we only the professional activities of Professor Smets have been mentioned. However, despite his many trips abroad, on which he was often been accompanied by the equally well known Mrs. Smets, he was a family man, father of five children, also the spiritual father of a countless number of students who for a long time have been able to count on him for advice and support.

After Professor Smets death a Georges Smets Chair was created jointly by his colleagues of the Katholieke Universiteit Leuven and the Université Catholique de Louvain. Each year a lecturer of world renown is appointed to give lectures in both universities.

## **Bibliography**

- G. Gruenwald, *Plastics. How Structure Determines Properties* Hanser Publishers, Munich, 1993.
- Wikipedia: Matière Plastique <[http://fr.wikipedia.org/wiki/Mati%C3%A8re\\_plastique](http://fr.wikipedia.org/wiki/Mati%C3%A8re_plastique)>

### **III**

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## **Papers and Posters**

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## Did Lucretius' Atomism Play any Role in Early Modern Chemistry?

Marco Beretta\*

Recent historiography has pointed out the influence on early modern chemistry of different classical theories of matter. Among these the reading and interpretation of the writings of Lucretius is a particularly interesting example. While the *De rerum natura* has been regarded by religious authorities as a dangerous heterodox work, chemists throughout Europe between 1500 and 1800 became progressively interested in adopting Lucretius' qualitative atomism. It is argued that such atomism played an important role not only in building an alternative philosophy of matter to that of Aristotle, but also in developing more concrete and operative options, such as the definition of chemical reaction.

“So different forms come together into one mass and things are made of mixed seeds [*permixto semine*]. Nay more, everywhere in these very verses of mine you see many letters common to many words, and yet you must needs grant that verses and words are formed of different letters, one from another; not that but a few letter run through them in common, or that no two of them are made of letters all the same, but that they are all alike the same one with another. So in other things likewise, since there are atoms common to many things, yet notwithstanding they can exist with sums different from one another; so that the human race and corn and glad trees are rightly said to be created of different particles”. *DRN*, II, 686-699.<sup>1</sup>

By taking the fortunate analogy between atoms and letters, already used by Democritus, Lucretius wished to push it further and aimed at bringing the tenet of classical atomism from the relevance attributed by Democritus to individual atoms to the central importance he attributed to aggregates and combinations. Lucretius thought that the macroscopic bodies were the results of the combination of molecules (which he called *concilia*) constituted of different kinds of atoms. While the number of atoms existing in nature was infinite, their forms, just like the letters of the alphabet, were limited. These very forms were at the end the explanation of the macroscopic differences between different observable bodies:

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“Why? Unless it be that those bodies of light are smaller than those of which the quickening liquid of water is made. And we see wine flow through the strainer as swiftly as you will; but, on the other hand, the sluggish olive-oil hangs back, because, we may be sure, it is composed of particles either larger or more hooked [*magis hamatis*] and entangled [*plicatis*] one with the other...

So that you may easily know that those things which can touch the senses pleasantly are made of smooth and round bodies [*levibus atque rotundis*] but that on the other hand all things which seem to be bitter and harsh, these are bound together with particles more hooked...”. *DRN*, II, 389-394 and 402-407.

Lucretius defined in further details the relations between the shapes of the atoms and their physical-chemical effect:

“Other particles there are, moreover, which cannot rightly be thought to be smooth not altogether hooked with bent points, but rather with tiny angles standing out a little, such as they can tickle the senses rather than hurt them; and of this kind is lees of wine and the taste of endive. Or again, that hot fire and cold frost have particles fanged [*dentata*] in different ways to prick the senses of the body is proved to us by the touch of each...”. *DRN*, II 426-434.

As their shapes were limited, the same kinds of atoms could enter into the composition of completely different bodies, like letters in words. The macroscopic changes were due to the different dispositions of the same atom, just like the letters which compose the word *AMOR* might be rearranged and result into the word *ROMA*. However, the analogy stopped here, because the atoms could be rearranged not only horizontally but in all three dimensions. Lucretius’ description of this kind of arrangement is worth quoting:

“In truth when you have tried all those parts of one body in every way, shifting top and bottom, changing [*transmutans*] right with left, to see what outline of form in that whole body each arrangement gives...”. *DRN*, II, 487-90.

It is interesting to note that in this important quotation Lucretius introduced the term transmutation, for the first time, into the Latin language. However, while later alchemists understood under this concept the possibility to obtain gold from lead through a complex experimental procedure, Lucretius understood it as the effect of the molecular structure of bodies which, under certain circumstances, suffered changes into the disposition of their atoms without their quantity or quality being minimally altered.

Equally original is the use that Lucretius makes of the term mixt (*permixtum*). In contrast to the reductionistic philosophies of matters set forth by the pre-Socratic

philosophers and by Aristotle, Lucretius thought that matter was mostly constituted by molecular aggregates and that the motion of isolated atoms was only a temporary state which inevitably led to some kind of combinations with other atoms:

“There is not one of all the things, whose nature is seen before our face, which is built of one kind of atoms, nor anything which is not created of well mixed seeds [*permixto semine constet*]; and whatever possesses within it more forces and powers, it thus shows that there are in it most kinds of atoms and diverse shapes.” *DRN*, II,583-588.

This is a crucial statement, because Lucretius while recognising the intrinsic complexity of matter, could explain phenomena such as the passage from the inorganic to the organic and *vice versa* from life to death, without having to resort to metaphysical or occult causes. When the molecule of a body dissolved the atoms did not disappear but moved to form other molecules, not necessarily the same, and thus continue the eternal cycle of the transformation of matter.

It is now important to underline the difference of Lucretius' concept of a mixed body with that supported by Aristotle in *De generatione et corruptione* (327a-b) where he excluded, in the following passage, the possibility of the ingredients to maintain their original identity:

“According to some thinkers, It is impossible for one thing to be combined with another. They argue that if both the combined constituents persists unaltered, they are no more combined now than they were before, but are in the same condition: while if one has been destroyed, the constituents have not been combined –on the contrary, one constituent is and the other is not, whereas combination demand uniformity of condition in them both: and on the same principle even if both the combining constituents have been destroyed as the result of their coalescence, they cannot have been combined since they have no being at all.”<sup>2</sup>

Aristotle, it is clear from this passage, preferred to study the qualitative transformation of matter only from its observable features. Lucretius, on the contrary, is interested more in the chemical mechanism of combination than in its effects. The weaknesses of this approach laid in the impossibility of empirical verification. However, while for the Aristotelians mixts were essentially different from the sum of their constituents, for Lucretius the atomic composition of matter allowed the reversibility of molecular combinations and the whole universe was nothing else than a perennial flow of dissolutions and combinations of atoms the sum of which remained constantly and eternally the same. From this followed the basic principles stated in the first book of the *DRN* which say that “nothing is ever



begotten of nothing" (I, 149) and that "nature breaks up each thing again into its own atoms, nor does she destroy ought into nothing" (I, 215-216).

From this succinct exposé it is hopefully clear how fecund the Lucretian view of matter could have been if applied to the solution of chemical issues. It is therefore difficult to agree with the conclusion of the recent book on atomism by William Newman<sup>3</sup> where the success of corpuscularism is traced back to the fourth book of Aristotle's *Meteorologica* rather than to classical atomism. But this is another story.

Having briefly exposed the basic principles of Lucretius' philosophy of matter, their influence on early modern chemistry will now be highlighted.

The first commented edition of Lucretius' poem appeared in 1511 (Bologna)<sup>4</sup> but prior to that, Leonardo da Vinci had showed, in a chemical context, a confident knowledge of its contents and used the term *semenze delle cose* (seeds of things), *attomi* (atomi), *particule* (particles) which he took from both Lucretius' *DRN* and the Latin translation of Diogenes Laertius *Vitae philosophorum*. An acquaintance of him, the artillery officer from Siena Vannoccio Biringuccio, author of the *De la pirotechnia* the first systematic survey of the mineral world (published in Venice in 1540) also showed some acquaintance with the poem when, in order to explain the different texture of silver mines, resorted to the hypothesis of the different shape of the atoms and *particelle*.

It was during the seventeenth century that Lucretius was discovered by naturalists in general and chemists in particular as an effective source in the battle against Aristotle's theory of matter.

In 1620 in the second part of the *Novum Organum* Francis Bacon while describing the shape of the particles which constitute the texture of matter wrote the following:

"Thus let the nature in question be the Expansion or Coition of Matter in bodies compared one with the other; viz. how much matter occupies how much space in each. For there is nothing more true in nature than the twin propositions, that "nothing is produced from nothing" [*ex nihilo nihil fieri*]<sup>5</sup> and "nothing is reduced to nothing" [*neque quicquam in nihilum redigi*]<sup>6</sup>, but the absolute quantum or sum total of matter remains unchanged, without increase or diminution."<sup>7</sup>

Here Bacon quotes nearly verbatim Lucretius' principle of conservation of matter and from it he deduces some important consequences also for chemistry:

"this greater or less quantity of matter in this or that body is capable of being reduced by comparison to calculation and to exact or nearly exact proportions.

Thus one would be justified in asserting that in any given volume of gold there is such an accumulation of matter, that spirit of wine, to make up an equal quantity of matter, would require twenty-one times the space occupied by the gold.

Now the accumulation of matter and its proportions are made manifest to the sense by means of weight.”<sup>8</sup>

Epicurus and Lucretius were the first atomists to attribute a specific weight to atoms, a feature which was destined to be of crucial importance for chemists.

Bacon, as it is well known, favoured the circulation of Lucretius' atomism within English scientific circles and its success among the future founders of the Royal Society of London is testified to by the first English translation of the first book of the *DRN* which was carried out in 1656 by John Evelyn.<sup>9</sup> In addition to the influence of Bacon, the edition of the *DRN* published by Gassendi in 1649 and then, less complete, in 1658 (see notes 11 and 12), played a central role in the appreciation of scientists in general and chemists in particular of the contents of the poem. In fact was Gassendi who tried the first serious attempt to combine chemical conceptions with atomism<sup>10</sup> and Lucretius' qualitative approach to the substratum of matter seemed indeed particularly fitted to bring such an attempt to a successful conclusion. The first reason that made atomism attractive was the patent observation made by seventeenth century chemists that the four elements of Aristotle and the three principles by Paracelsus were no longer sufficient to explain the complex texture of matter and the innumerable variety of macroscopic bodies. As long as the chemical analysis of such bodies progressed, Gassendi pointed out, the clearer became that the ingredients of these same bodies went beyond the elements. The atoms of Lucretius were useful for another reason: in addition to their mechanical qualities (movement, size and gravity) they possessed particular shapes which, as we have seen, were the true causes of the peculiar molecular texture of bodies. It is interesting to note in passing that Gassendi introduced the term *moleculae* as the discriminatory element of the intrinsic complexity of matter, and that he does so while commenting a passage of the second book (135-141) of the *DRN*.<sup>11</sup> Within this framework the shapes of the atoms were the causes of heat, cold, light, sound etc. as well as of the chemical combinations of mixts. The importance of Gassendi for early modern chemistry has been already examined in many studies and this is not the occasion to insist further on this topic, rather I would like point out an aspect of both the *Animadversiones*<sup>12</sup> and the *Syntagma* which has been so far neglected: both works are editions of Lucretius' *DRN*. It is true that the *DRN* is not published in the original consequence but in his aim to revive its contents, Gassendi decided to scatter the verses according to a logic which followed the distinction made by Epicurus of philos-

ophy, into canonic, physic and moral, taking physics and the most important part of the three.<sup>13</sup> Gassendi mixed his prose with descriptions of the experiments made by him and his contemporaries with commentaries of Lucretius' passages related to the topic he treated and Lucretius' atomism served him to provide a consistent theoretical basis to his chemistry.

Gassendi's works were highly successful and as early as 1654 William Charleton, a physician with a keen interest in Helmontian iatrochemistry, presented a synthesis of Gassendi's work into English.<sup>14</sup> But it was within the Fellows of the Royal Society of London that Gassendi and Lucretius found an attentive audience.

In this favourable context, it is not surprising that Robert Boyle adopted corpuscularism as one of the keys he used to interpret chemical combinations. He was also very positive towards Lucretius:

"By granting Epicurus his principles that the atoms or particles of bodies have an innate motions, and granting our supposition of the determinate motion and figure of the aerial particles, all the phenomena of rarefaction and condensation, of light, sound, heat etc., will naturally and necessarily follow."<sup>15</sup>

Lacking empirical ground, however, Lucretian atomism had to be mingled with Paracelsism and other sources with an approach more suited to the experimental outlook privileged by Boyle. On the other hand we should not underestimate the fact that due to his straightforward criticism against religion, his belief in the materiality and mortality of the soul, his cosmological thinking on infinite worlds, Lucretius was a highly controversial author whom, in order to avoid the accusation of atheism, could be cited only with cautious circumspection. This is certainly the reason why Boyle, in *The Sceptical Chymist* (1661) after having set his famous definition of the elements as "certain primitive and simple, perfectly unmingled bodies; which not being made of any other bodies, or of one another, are the ingredients of which all those called perfectly mixt bodies are immediately compounded, and into which they are ultimately resolved"<sup>16</sup>, tried to prove that his adherence to atomism was a stranger to Lucretius. On this matter he in fact declared:

"If I were fully to clear to you my apprehensions concerning this matter, I should perhaps be obliged to acquaint you with diver of the conjectures (for I muse yet call them no more) I have had concerning the principles of things purely corporeal: for though because I seem not satisfied with the vulgar doctrines, either of the peripatetick or Paracelsian schooles, many of those that know me [...] have though me wedded to the Epicurean *Hypothesis*, (as others have mistaken me for

an *Helmontian*) yet if you knew how little conversant I have been with *Epicurean* authors, and how great a part of *Lucretius* himself I never yet had the curiosity to read, you would perchance be of another mind.”<sup>17</sup>

Naturally, Boyle’s statement was guided by his firm aim at distinguishing his corpuscularism from that of Thomas Hobbes which he regarded as dangerously heterodox and which was explicitly, though not entirely, based on Lucretius. In fact, other members of the Royal Society did not find Lucretius to be so dangerous and, as perceptibly noticed by Henry Guerlac, Isaac Newton was among the most enthusiastic supporter of Lucretian atomism, especially when its attention was focussed to explain the nature of the microscopic world. In the famous Query 31 of the *Opticks* devoted to the explanation of chemical reactions, Newton in fact wrote:

“it seems probable to me, that God in the Beginning form’d Matter in solid, massy, hard, impenetrable, moveable Particles, of such Sizes and Figures, and with such other Properties, and in such Proportion to Space, as most conduced to the End for which he form’d them”<sup>18</sup> [i.e the composition of natural bodies].

The followers of Newton brought this idea further and applied it to iatrochemistry. This is the case of James Keill who, in order to explain the composition of blood set forth the following hypothesis:

“A few different sorts of particles variously combined, will produce great variety of fluids, some may have only one sort, some three, ore more ... If we suppose only five different sorts of particles in the blood, and call them *a, b, c, d, e*, their sever-al combinations, without varying the proportions, in which they are mixt will be these following:

*ab: ac: ad: ae:*  
*bc: bd: be: cd:*  
*ce: de: abc: adc:*  
*bdc: bde: bec: dec:*  
*abcd: abc: acde: abd:*  
*bcde: abcde:*

But whether there are more or fewer in the blood, I shall not determine.”<sup>19</sup>Arnold Thackray has seen in this explanation the first attempt to apply an algebraic method to chemistry and has not seen that Keill has used Lucretius’ analogy between atoms and letters and that, exactly like the Latin poet, conceived no more than 5 difference atomic shapes, here represented with the initial letters abcde.

Following Boyle's application of the corpuscular hypothesis to chemistry, a typical reaction which was explained in Lucretian terms were those involving salts.

The French apothecary Nicolas Lemery, in his famous *Cours de Chymie* (Paris, 1675), conceived these specific chemical reactions in terms of particle shape and movement. While acids had a pointed shape which explained their sharp taste and their tendency to solidify by forming pointed crystals, alkalis were composed of a porous texture so shaped as to admit entry of the spike particles of acid.<sup>20</sup>

This result was to have crucial consequences.

During the early 1660s a pupil of Giovanni Alfonso Borelli and successor of Galileo in the chair of mathematics of the University of Pisa, Alessandro Marchetti, translated Lucretius into Italian and although the book was not printed until 1717,<sup>21</sup> manuscript copies of the translation widely circulated and not only in Italy within scientific circles. Domenico Guglielmini was among those who was particularly influenced by Marchetti's translation and he was the one who introduced for the first time the term *molecole* in the Italian language.<sup>22</sup> Guglielmini took from Lucretius the idea that atoms were constituted by *minimae partae* (minimal parts) which could be considered as geometrical figures and which were actually the cause of the different forms of atoms. These minimal parts, however, did not have an independent existence and the materiality was a quality which belonged exclusively to the atoms they belonged. It seemed therefore that the minimal part served Epicurus and Lucretius to build an atomic geometry which was alternative to that of Euclid (which as it is well known is based on the axiom that points have neither parts nor extensions) and at the same time to keep the physical materiality of the atom. Crystals observed with the microscope in salts seemed to confirm that elementary particles had specific geometrical forms! Following his observations with the microscope, Guglielmini developed the idea that the shape of the atoms constituting salts could be reduced to homogeneous geometric forms deduced by the crystals configurations.

A salt reaction was visualized for the first time in the work *Conjectures physiques* by the Dutch natural philosopher Nicolas Hartsoeker.<sup>23</sup> He thus represented, for the first time in a scientific treatise, the shapes of the particles of an alkali as cylinders porous to the extremities. This particular shape made it natural to the pointed particles of acids, represented as nails, to fill the pores and to enter in combination with the alkali. Hartsoeker however went even further in his explanation of the reaction by showing how the round particles of water, represented as in Lucretius like small spheres, separated the alkali from the acids.

In 1768 d'Holbach promoted a French translation of Lucretius' *DRN*.<sup>24</sup> The author of the translation was La Grange, a materialistic philosopher tutor of d'Holbach sons, but the commentaries which accompanied the translation were the works of scientists, some of whom like the chemist Jean Darcet, were distinguished members of the Académie Royale des Sciences in Paris.

Against this background, it is not surprising that d'Holbach was one of the most important protagonist in the revival of Lucretius' doctrine of organic matter. In his *Système de la nature*, anonymously published in 1770, he summarised the contents of his work, and more generally of his philosophical doctrine, as follows:

"It will not then be inconsistent with observation, repugnant to reason, contrary to good sense, to acknowledge that matter is self-existent; that it acts by an energy peculiar to itself; that it will never be annihilated. Let us then say, that matter is eternal, that nature has been, is, and ever will be occupied with producing and destroying; with doing and undoing, with combining and separating; in short, with following a system of laws resulting from its necessary existence. For every thing that she doth, she need only to combine the elements of matter; these, essentially diverse, necessarily either attract or repel each other; come into collision, from whence results either their union or dissolution."<sup>25</sup>

As emphasised by this passage, d'Holbach attributed to matter an inner energy, to nature the necessity of laws regulating occurrence of natural phenomena, and to sensuous experience the possibility of knowing and manipulating them by following the paths indicated by nature herself. D'Holbach had been the translator of Stahl's chemistry into French and his idea on matter heavily relied upon the conviction that atoms had an inner force which enabled them a self organization. His doctrine, shared by Rouelle, Roux and Darcet, was mixture of materialistic and vitalistic concept which on the one hand enhanced the epistemological value of qualitative chemistry and on the other undermined mathematical authority.

From an entirely different perspective Lavoisier developed a corpuscular view of matter which also relied on the reading of Lucretius. While Lavoisier shared d'Holbach's views that chemistry could not be reduced to mathematics and that the appreciation of the individual qualities of the ingredients of a reaction was its distinctive feature, he thought that quantification had a central role in the exact identification of these same ingredients. To this aim, between 1766 and 1767, he proposed to measure the quantity of salts dissolved in water by determining their specific gravities and by preparing comparative tables of the results. What he expected from these results was to gain different quantitative numerical data for each and every salt he had analysed. Lucretius also thought that each solid body

had a different specific gravity and that this was due to the fact that atoms themselves had different specific weight. Lavoisier owned two copies of the Latin poem, one in Latin and the other in French in the d'Holbach edition.<sup>26</sup> True, Lucretius was never cited in Lavoisier's works. However, there are several instances in his work of analytical chemistry which clearly show a close reading of the Latin poem. Space limitation does not permit to go through them now. Here it suffices to mention the most evident, and perhaps most important, of them. In 1789 Antoine Laurent Lavoisier published a *Traité élémentaire de chimie* a work which, according to his contemporaries introduced the quantitative method of analysis of reaction as the sole viable investigative path for chemistry. In the central part of the *Traité* the French chemist established that

"in all the operations of art and nature, nothing is created; an equal quantity of matter exists both before and after the experiment; the quality of the elements remains perfectly the same; and nothing takes place beyond changes and modifications of these elements. Upon this principles the whole art of performing chemical experiments depends: we must always suppose an exact equality between the elements of the body examined and those of the products of its analysis."<sup>27</sup>

The identity of this principle, eventually regarded as one of the most important laws of chemistry, with Lucretius's doctrine of conservation of matter is remarkable. The Latin poet in fact stated in the first book of his poem that "nothing is ever produced from nothing" and that "no single thing returns to nothing, but all by disruption return to the elements of matter". (*DRN*, I, v. 150 e 248-50).

After Lavoisier chemical atomism gained momentum and with the works by Berthollet, Dalton, Berzelius and Avogadro a new story began which finally left Lucretius behind and in which no traces of the classical atomism were left. This shift became possible because the systematisation Lavoisier gave to chemistry made it useless to resort to the history of atomism and gave a modern basis in which chemistry could progress with its own autonomous theoretical means.

## Notes

<sup>1</sup> All the citations from Lucretius' *De rerum natura* (hereafter abbreviated as *DRN*) have been taken, with a few modifications, from volume 1 of *Titi Lucreti Cari De rerum natura libri sex*. Edited with Prolegomena, Critical Apparatus, Translation, and Commentary by Cyril Bailey (Oxford: The Clarendon Press, 1947), 3 vols. A different version of this paper has been published with the title "Lucrezio e la chimica", *Automata*, 2 (2007), pp. 41-57

<sup>2</sup> *The Works of Aristotle translated into English under the editorship of W. D. Ross* (Oxford: At the Clarendon Press, 1947) vol. 2.



<sup>3</sup> William R. Newman, *Atoms and Alchemy: Chymistry and the experimental Origins of the Scientific Revolution* (Chicago: The University of Chicago press, 2006).

<sup>4</sup> *In Carum Lucretium poetam commentarij a Ioanne Baptista Pio editi, codice Lucretiano diligenter emendato, nodis omnibus et difficultatibus apertis, obiter ex diuersis auctoribus tum Grecis tum Latinis multa leges enucleata, que superior etas aut tacuit aut ignorauit* (Bononiae : typis excussoriis editum in ergasterio Hieronymi Baptistae de Benedictis Platonici Bononiensis, 1511 kal. Maii).

<sup>5</sup> *DRN*, I, 149-150.

<sup>6</sup> *DRN*, I, 215-216.

<sup>7</sup> Francis Bacon, *The Works ... Collected and Edited by James Spedding, Robert Leslie Ellis and Douglas Denon Heath*, vol. 4 (London: Longman & Co., 1858), p. 197. The Latin version of the same text is quoted in the first volume of the same edition on p. 311.

<sup>8</sup> Francis Bacon, *The Works*, *Op. cit.*, vol. 4, p. 197

<sup>9</sup> *An essay on the first book of T. Lucretius Carus De rerum natura interpreted and made English verse by J. Evelyn* (London: printed for Gabriel Bedle and Thomas Collins, 1656).

<sup>10</sup> Olivier René Bloch, *La philosophie de Gassendi. Nominalisme, Matérialisme et Métaphysique* (La Haye : Nijhoff, 1971), p. 241.

<sup>11</sup> Pierre Gassendi, *Syntagma Philosophicum* in Id., *Opera Omnia*, (Lugduni: sumptibus Laurentii Anisson & Ioan. Bapt. Devenet, 1658), 6 vols. on vol. 1, p. 282b.

<sup>12</sup> Pierre Gassendi, *Animadversiones in decimum librum Diogenis Laertii, qui est de vita, moribus, placitisque Epicuri: continent autem Placita, quas ille treis statuit philosophiae parteis: I, Canonicam nempe, habitam Dialecticae loco; II, Physicam, ac imprimis nobilem illius partem meteorologiam; III, Ethicam, cuius gratiâ ille excoluit caeteras* (Lvgdvni: apud Gvillelmvm Barbier, 1649), 3 vols.

<sup>13</sup> In a letter addressed to Gabriel Naudé, by referring to his own method of using Lucretius, Gassendi wrote: "Sed erit forte, quod uterque in eodem campo decurramus, cum ille Lucretium ex serie contextus interpretatus sit, ipse methodo paullo immutata Lucretium producturus sim, ad explicationem, confirmationemque placitorum Epicureorum hinc *totus quidem Lucretius in opellam mean transferetur*; sed carminum ordo mihi perturbatus, planeque varius futurus est» (my Italics). Gassendi, *Opera*, vol. 6, pp. 49-50.

<sup>14</sup> Walter Charleton, *Physiologia Epicuro-Gassendo-Charltoniana: or a fabrick of science natural upon the hypotesis of atoms with indexes and a new introduction by Robert Hugh Kargon*, Repr. from the London ed. 1654 (New York: Johnson reprint corporation, 1966).

<sup>15</sup> Robert Boyle, *Works* (London 1772), vol. 1, p.180.

<sup>16</sup> *The Works of Robert Boyle. Edited by Michael Hunter and Edward B. Davis* (London: Pickering & Chatto, 1999), vol. 2, p. 345.

<sup>17</sup> *The Works of Robert Boyle. Edited by Michael Hunter and Edward B. Davis* (London: Pickering & Chatto, 1999), vol. 2, p. 354.

<sup>18</sup> Isaac Newton, *Opticks*, Ed. by I. B. Cohen, New York, Dover Publications, 1979, p. 400.

<sup>19</sup> James Keill, *An Account of animal secretion, the quantity of blood in the human body, and muscular motion* (London: 1708), pp. 61-61, quoted in Arnold Thackray, *Atoms and Powers. An Essay on Newtonian Matter-Theory and the Development of Chemistry* (Cambridge Mass.: Harvard University Press, 1970) p. 70.

<sup>20</sup> "Comme one ne peut pas mieux expliquer la nature d'une chose aussi cachée que l'est celle d'un sel, qu'en attribuant aux parties qui le composent des figures qui répondent à tous les effets qu'il produit, je dirai que l'acidité d'une liqueur consiste dans des particules de sel pointues, lesquelles



sont en agitation ; & je ne crois pas qu'on me conteste que l'acide n'ait des pointes, puisque toutes les expériences le montrent." Lémery, *Cours de Chymie* (Paris: d'Houry, 1757), p.17.

<sup>21</sup> *Di Tito Lucrezio Caro Della natura delle cose libri sei. Tradotti da Alessandro Marchetti lettore di filosofia e matematiche nell'universita di Pisa et accademico della Crusca* (London: John Pickard, 1717). On this and all the editions of Lucretius published with the contribution of natural scientists see my "Gli scienziati e le edizioni del *De rerum natura*" in, Marco Beretta and Franco Citti (eds.), *Lucrezio, la natura e la scienza* (Firenze: Leo S. Olschki) forthcoming.

<sup>22</sup> Domenico Guglielmini, *Riflessioni filosofiche dedotte dalle figure de' sali dal dottore Domenico Guglielmini espresse in un discorso recitato nell'Accademia filosofica sperimentale di monsig. arcidiacono Marsigli la sera delli 21. marzo 1688* (Bologna : per gli eredi d'Antonio Pisarri, 1688) Chapter 79.

<sup>23</sup> Nicolas Hartsoeker, *Conjectures physiques* (Amsterdam, 1706).

<sup>24</sup> *Lucrece. Traducion nouvelle avec des notes* (Paris: Bleuet, 1768) 2 vols.

<sup>25</sup> "Reconnoissons donc que la matière existe par elle-même, qu'elle agit par sa propre énergie et qu'elle ne s'anéantira jamais. Disons que la matière est éternelle, et que la nature a été, est et sera toujours occupée à produire, à détruire, à faire, et à défaire, à suivre des loix résultantes de son existence nécessaire. Pour tout ce qu'elle fait elle n'a besoin que de combiner des élémens et des matières essentiellement diverses qui s'attirent et se repoussent, se choquent ou s'unissent, s'éloignent ou se rapprochent, se tiennent assemblées ou se séparent. C'est ainsi qu'elle fait éclore des plantes, des animaux, des hommes ; des êtres organisés, sensibles et pensants, ainsi que des êtres dépourvus de sentiment et de pensée. Tous ces êtres agissent pendant le tems de leur durée respective suivant des loix invariables, déterminées par leurs propriétés, leurs configurations, leurs masses, leurs poids, etc. » d'Holbach, *Système de la nature* (1770) (Paris: Fayard, 1990), t. 2, p. 171.

<sup>26</sup> *Titi Lucretii Cari De rerum natura libri sex, ad postremam Oberti Gifanii Ic. emendationem accuratissimè restituti. Quae praeterea in hoc opere sint praestita, pagina post dedicationem indicabit* (Lugduni Batavorum: ex officina Plantiniana, apud Franciscum Raphelengium, 1595); *Lucrece, traduction nouvelle, avec des notes, par M. L\* G\*\** (Paris: Bleuet, 1768) 2 voll.

<sup>27</sup> "On voit que, pour arriver à la solution de ces deux questions, il fallait d'abord bien connaître l'analyse et la nature du corps susceptible de fermenter, et les produits de la fermentation ; car rien ne se crée, ni dans les opérations de l'art, ni dans celles de la nature, et l'on peut poser en principe que, dans toute opération, il y a une égale quantité de matière avant et après l'opération; que la qualité et la quantité des principes est la même, et qu'il n'y a que des changements, des modifications.

C'est sur ce principe qu'est fondé tout l'art de faire des expériences en chimie: on est obligé de supposer dans toutes une véritable égalité ou équation entre les principes du corps qu'on examine et ceux qu'on en retire par l'analyse." Lavoisier, *Traité élémentaire de chimie* (Paris : Cuchet, 1789), vol. 1, pp. 140-141.

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## George Ripley and Alchemical Consensus

Jennifer M. Rampling\*

Alchemical practitioners frequently sought to demonstrate the credibility of their writings by appropriating the wisdom of their authoritative predecessors, while condemning the practices of “false” alchemists. Sometimes, however, respected authorities appear to disagree, both with one another and with other texts attributed to them. The English alchemist George Ripley (c. 1415–1490) was particularly deft at reconciling potential conflicts between his sources, to the extent that later, pseudo-epigraphic works also imitated his placatory approach. This paper represents a summary of the argument presented at the Sixth International Conference on the History of Chemistry.

### Introduction

In 1317 Pope John XXII issued his famous decretal against the practice of alchemy. One of the charges laid against the alchemists was the inconsistency of their texts, which, in undermining the unity of alchemy as a discipline, also raised questions over its right to be regarded as a science. Disputing over the precise ingredients and processes to be used was a well-established alchemical tradition. For instance, in a tenth-century treatise, the *Mâ’ al-Waraqî*, Ibn Umail rebuked foolish practitioners who, by ignorantly misreading their authorities, attempted to use human hair in their works – a barb probably aimed at the *Shawâhid* of Muhammad bin Zakarîyâ ar-Râzî, which explicitly recommended such animal products as hair and eggs.<sup>1</sup> Yet many medieval Latin texts sought to reconcile contradictory authorities, where necessary reinterpreting their words. For instance, the philosophers’ frequent references to hair, blood and eggs are explained in the fourteenth-century *De Secretis naturae* of pseudo-Arnald of Villanova as metaphorical code names for the elements. The philosophers deliberately obscured their true meaning with cover names, or *decknamen*, in order to protect their knowledge from the unworthy.<sup>2</sup>

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## George Ripley and the pseudo-Lullian corpus

This paper considers how potential conflicts between authorities are handled in some of the texts attributed to one fifteenth-century English alchemist, George Ripley (c.1415–1490). While condemning “false books” and warning of obscure language intended to deceive “fools,” Ripley’s work is generally respectful towards the great alchemical authorities. Indeed, by showing how apparent differences could be reconciled, Ripley and those who later wrote under his name were able to confirm their own status as adepts, and to site themselves firmly on the side of the philosophers, rather than with the fools who misunderstood and misapplied alchemical secrets.

Little is known of the life of George Ripley, a Canon Regular of the priory at Bridlington in Yorkshire, except that he obtained a papal dispensation to study at university for seven years, with the option of studying abroad.<sup>3</sup> Later, he was to enjoy one of the most successful posthumous careers of any English alchemist, and enjoyed that highest of chymical compliments – the attribution of a large number of pseudo-epigraphic works. For the purposes herein, only the two texts most reliably attributed to Ripley: his famous poem, the *Compound of Alchemy*, or *Twelve Gates*, and the Latin prose work, the *Medulla Alchymiae*, are regarded as original works.

In both of these works, the influence of pseudo-Lullian alchemical texts is overwhelming. Although the historical Ramon Lull denied the possibility of alchemy, over one hundred works are pseudonymously ascribed to him, espousing a variety of alchemical doctrines. As Michela Pereira has noted, some compilers attempted to organize the diverse and often contradictory Lullian works into a unified corpus.<sup>4</sup> Ripley’s later reputation appears to stem from success in just such an endeavour, a fact recognised in the 1649 edition of his *Opera omnia*, featuring twelve of his attributed works, published in Cassel. The editor, Ludwig Combach, devoted the larger part of his introduction not to Ripley, but to Lull, stressing Ripley’s primary value as an expositor of Lull’s works.<sup>5</sup> In Elias Ashmole’s paraphrase, Ripley “has great Affinity with the Writings of *Lully*, insomuch that the one explaineth the other”.<sup>6</sup>

When considering Lull in relation to other authorities, however, Ripley was similarly concerned to present both opinions as potentially valid. We see this both in the *Compound of Alchemy*, where Ripley presents the alternative recipes of Lull and Roger Bacon as equally valid, and also in the *Medulla*, where a ‘water’ described by Guido de Montanor is suggested as a more readily available alternative

to the Lullian recipe<sup>7</sup>. In both cases, Ripley presents alternatives to strict Lullian practice. The *Medulla* also provides interesting evidence of Ripley's attempts to reconcile conflicts between different Lullian texts, particularly between those arguing for the inclusion of organic ingredients (such as 'quintessence of wine') and those forbidding non-metalline materials. Ripley's ingenious solution is to gloss a passage from the Lullian *Epistola accurtationis*, which uses quintessence of wine, using terms taken from another core Lullian text, the *Testamentum*, which refer to non-organic ingredients.<sup>8</sup> This decision appears to have its origin from Ripley's own observation that distilled alcohol is too weak to affect the changes described in his source, and hence must represent a deliberate obscuration of the true ingredient. For Ripley, the authority of his source was thus preserved, as was the "metal-only" composition of the philosophers' stone.

The linkage of Ripley's name with attempts to attain consensus between his sources is also apparent in other, less reliably attributed items in the 'Ripley corpus.' One particularly intriguing example is the *Concordantia Raymundi Lullii & Guidonis*, a short text aimed at reconciling a potential conflict between Ripley's two favourite authorities, Lull and Guido, over whether the ferment to be used in the work should be common gold, or gold which has first been decocted.<sup>9</sup> This text, the earliest copy of which dates to the mid-sixteenth century, bears striking similarities with Ripley's handling of the same problem in his Compound and *Medulla*. Less convincingly 'Ripleian' is a commentary on Aristotle and Hermes, circulating in the last quarter of the sixteenth century, which attempts to reconcile the two ancient authorities. This text appears to have been adapted from an earlier, fifteenth-century work attributed to one Richard of Salopia, to which Ripleian characteristics (including references to Lull and Guido) have been appended by a later compiler.<sup>10</sup>

## Conclusion

Taken together, these texts chart a fascinating change of emphasis in Ripley's reputation. The Canon's original works expounded and popularized Lullian doctrines, helping embed them in existing alchemical traditions while advertising Ripley's own mastery of his sources. A century later, Ripley had become an authority in his own right, his own name used to lend authority to alchemical texts.

## References

- <sup>1</sup> H. E. Stapleton, *Three Arabic Treatises on Alchemy by Muhammad bin Umail (10th Century A.D.)* (Calcutta: Memoirs of the Asiatic Society of Bengal, XII, 1933), 142.
- <sup>2</sup> “Fatui autem intelligentes litteram operati sunt de ovis, de sanguine, de capillis, de rebis, de aluminibus, de salibus, et nihil invenerunt quia non bene intellexerunt dicta philosophorum obscure loquentium.” Antoine Calvet, “Le *De secretis naturæ* du pseudo-Arnaud-de Villeneuve,” *Chrysopoeia: Cinq traités alchimiques médiévaux* VI (1997-9): 155-206, on 180.
- <sup>3</sup> *Calendar of entries in the Papal registers relating to Great Britain and Ireland: Papal letters, Vol. XI: 1455-1464*, prepared by J.A. Twenlow (London: H.M.S.O., 1893), 530-1.
- <sup>4</sup> Michela Pereira, *The Alchemical Corpus Attributed to Raymond Lull* (London: Warburg Institute Surveys and Texts, 1989).
- <sup>5</sup> Ludwig Combach, ‘Praefatio ad lectorem,’ in George Ripley, *Opera omnia chemica, quotquot hactenus visa sunt, quorum aliqua jam primum in lucem prodeunt, aliqua MS. exemplarium collatione à mendis & lacunis repurgata, atque integrati restitua sunt*, ed. Ludwig Combach (Cassell: typis Jakob Gentschii, impensis Sebald Köhlers, 1649), ff. 2r-7r.
- <sup>6</sup> Elias Ashmole, *Theatrum Chemicum Britannicum* (London, 1652; repr. New York and London: Johnson Reprint Corporation, 1967), 458.
- <sup>7</sup> “Caeterum quia haec res rarò reperitur in partibus nostris, aliisque quibusdam regionibus, ideò Guido de Montanor Graecus Philosophus invenit aliam humiditatem unctuosam, quae natat super alios liquores.” Ripley, *Opera omnia chemica*, 167-8.
- <sup>8</sup> “[I]deò loquemur aliquid ampliùs de hoc igne contra naturam: & est aqua mineralis valdè fortis & mortalis, quae pertinet ad Elixir illud. Et haec aqua extrahitur cum igne elementalì de quodam menstruo foetente, quod fit ex rebus quatuor, ut inquit Raymundus in epistola accurtat. & est aqua fortissima mundi” (Ripley, *Opera omnia chemica*, 167-8). The *Epistola accurtationes* omits mention of the fire against nature: “Tota enim via mineralium consistit in duabus aquis, quarum una facit lapidem volatilem sine labore & periculo: alia autem eum fixat, & secum fixatur cum periculo: quia haec aqua extrahitur, ut nostri ex quodam menstrualì foetenti immassati ex rebus quatuor, & fortior est aqua mundi & mortalis.” Jean-Jacques Manget, *Bibliotheca chemica curiosa, seu rerum ad alchemiam pertinentium thesaurus instructissimus* (Geneva: 1702), vol. 2, 864. The concept of the fire against nature is expounded in the Lullian *Testamentum*; a modern edition of which is provided by Michela Pereira and Barbara Spaggiari, *Il Testamentum alchemico attribuito a Raimondo Lullo: Edizione del testo latino e catalano dal manoscritto Oxford, Corpus Christi College, 255* (Florence: Sismel, 1999).
- <sup>9</sup> Ripley, *Opera omnia chemica*, 323-36.
- <sup>10</sup> The “Expositio textuum Aristotelis et Hermetis, secundum Ricardum de Salopia” (London, British Library MS. Sloane 3744, ff. 54r-60r) contains none of the “Ripleian” additions found in the “Tractatus expositivus Hermetis et Aristotelis,” attributed to Ripley in a sixteenth-century version (London, British Library MS. Harley 2411, ff. 7r-24v).

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## Helmont's Mechanical Experiments

Steffen Ducheyne\*

In this essay, I will discuss four significant experiments from Van Helmont's work in full detail: (1) the thermoscope experiment, (2) the transmutation experiment, (3) the ice-experiment, and (4) the willow experiment. I will draw the main material from both *Ortus Medicinae* and *Dageraad*. These experiments have been selected on the basis of their being methodologically relevant and sufficiently detailed. For the English translation of *Ortus Medicinae*, I have relied on the English version of 1664 *Oratrike or Physick Refined* (which is, by the way, not an excellent translation) and compared it to the Latin edition – I refer to the latter in footnotes.<sup>1</sup> I will focus on and discuss what Van Helmont calls *mechanical experiments*. It should be stressed, as Newman and Principe have noticed before me, that the term “mechanical” is somewhat misleading here. The Low-German equivalent “*handtdadelijcke mechanijcke bewesen*”, i.e. “hand-on” or “handicraft”, better illustrates Van Helmont's notion of a mechanical experiment: *generally, it referred to natural processes which were deliberately manipulated at the hand of the investigator of nature and is not directly connected to simple machines*. I will use my analysis of these experiments as a basis for a general discussion of the characteristics of experimentation in Van Helmont's work in the following section.

Let us first of all look at Van Helmont's thermoscope experiment.<sup>2</sup> According to Van Helmont, the demonstration was essentially based on mathematics (he calls it a “*demonstratio mathematica*”<sup>3</sup>). It sets out to falsify the thesis according to which water and air can be transformed into one another: Van Helmont rejected both that air can be transformed into water by heating and that water can be transformed into air by heating. (Van Helmont accepted that water can be produced by the condensation of air (and hence, by cold).) Now for the experiment itself. Two spheres A and D are connected to each other by BCE. Both spheres are filled with air. The pipe BC is filled with vitriol which was coloured red by the steeping of roses. It is essential that the two spheres are perfectly closed “*perfec-*

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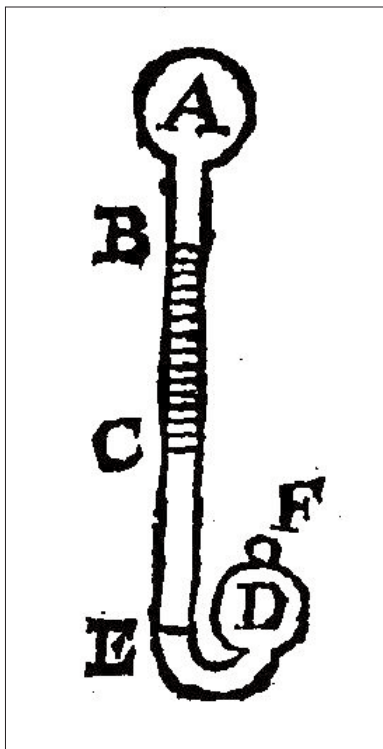


Figure 1. *Van Helmont's thermoscope experiment.* From Van Helmont, *Works*, 60.

*tissime clausa*".<sup>4</sup> Van Helmont established by observation that without the opening in F, the liquor in BC cannot be moved from its place by heating the air in A (see Figure 1). Van Helmont points to the great practical difficulty of the experiment:

The preparation of the demonstration. It is very great, because the air suffers enlarging, and the heaping together or straightning, according to the qualities of the heat and cold, and because the just extension of quantity is not had in the air, unless when it is temperate.<sup>5</sup>

When heating the air in A no extra water was produced. Van Helmont explained this by assuming that the air in the upper part of the vessel thickened as it tried to expand (*"Aër (...) accrescit per augmentum dimensionum, & ideo occupat plus loci, quam antea"*<sup>6</sup>). The amount of fluid remains the same, contrary to the opinion of Van Helmont's opponent, Henricus van Heers, a physician of Liège, according to whom the compressing of air by heat produces water (*"quod aer compressus, conversatur in aquam"*<sup>7</sup>).

Van Helmont stressed that van Heers faulty interpretation was due to his ignorance of mathematics:

But Heer boasted amongst *Idiots*, that he had sometimes been a Professour (*sic*) of the *Mathematicks* at *Padua*. Wherefore I would demonstrate in paper, his every way ignorance of Mathematics.<sup>8</sup>

Next, Van Helmont proceeded to show that the water cannot dry up (*"exsiccare"*) or be exhaled (*"exhalare"*) by heating, if A and D are kept carefully shut.<sup>9</sup> Since no extra water was produced when heating the air contained in A, the thesis that air can be transformed into water is untenable, according to Van Helmont. Similarly, since no water disappeared when heating the vessel, the thesis that water can be transformed into air (*"quod liquor sit mutatus in aëris"*) is untenable. The above experiment further exhibits the following features:

1. The potential movement of the water is visualised by colouration – note that there are only four figures in *Ortus* (which are absent in *Dageraad*).

2. By using a sphere (*sphaera* or *globus*) all disturbing factors (e.g., extra air or fluid) are screened off. The amount of air and water is kept fixed.
3. By using the sphere we establish a relatively isolated physical system.

Van Helmont claimed to have rebutted Aristotle's doctrine of the four elements and to have proven by "*handtdadelijcke mechanijcke bewesen*" and "*mathese*" that all matter originates from water.<sup>10</sup> I refer to this experiment as the transmutation experiment. These proofs consisted in showing that all material can be reduced "by art" to a salt which has an *identical* weight to that of the original material. When this salt is mixed with a corrosive it turns into "vivid water".<sup>11</sup> Once the corrosive is again separated from the "vivid water", an *identical* amount of corrosive is separated from an amount of clear water. Hence, Van Helmont is able to conclude that the original material should consist of water in the first place (reference to the constancy of matter is crucial in his argumentation). As I would interpret it, Van Helmont's reference to *mathese*, precisely lies in his reference to the conservation of matter. Van Helmont's reasoning process<sup>12</sup> goes as follows:

1. all material  $\Rightarrow_{(\text{by fire})}$  salt (*where the initial matter weighs as much as the obtained salt*)
2. [salt + corrosive]  $\Rightarrow_{(\text{mixing})}$  vivid water
3. vivid water  $\Rightarrow_{(\text{filtering})}$  [corrosive + clear water] (*where the corrosive weighs as much as the corrosive used in (2)*)
4. all material  $\Rightarrow_{(\text{by fire, mixing and filtering})}$  water (by steps (1)-(3) and the conditions in (1) and (3))

Bear in mind that by steps (2) and (3) Van Helmont is able to show that

$$[\text{salt} + \text{corrosive}] \Rightarrow_{(\text{mixing and filtering})} [\text{corrosive} + \text{clear water}]$$

Since the corrosive is identical, we have:

$$\text{salt} \Rightarrow_{(\text{mixing and filtering})} \text{clear water}$$

Note that, next to these "mechanical" proofs, Van Helmont also stressed a biblical reason not to accept Aristotle's doctrine: in *Genesis* there is no mentioning of the creation of the four elements.<sup>13</sup>



The next experiment I shall discuss is the ice-experiment. The aim of the experiment is (again) to show that air cannot be turned into water. It proceeds as follows:

Fill a glassen and great Bottle, with pieces of Ice, but let the neck be shut with a Hermes Seal, by the melting of the glasse in the same place. Then let this Bottle be put in a balance, the weight thereof being laid in the contrary Scale; and thou shalt see that the water, after the Ice is melted, shall be weightier by almost an eight part than it self being Ice. Which thing, since it may be a thousand times done by the same water reserving always the same weight, it cannot be said, that any part thereof has been turned into air.<sup>14</sup>

One thing we should keep in mind, as T.S. Patterson has argued, Van Helmont refers to an increase of the *specific weight* of the water, i.e. the weight per fixed unit of volume, obviously not of its *absolute weight*.<sup>15</sup> Newman & Principe stress that Van Helmont had no distinct terminology for absolute and specific weight. We notice that Van Helmont used the sphere as a means to isolate the volume of water and air. No water or air can escape, nor enter the vessel. Since the absolute weight of the water remains identical, the variations in its specific weight cannot be attributed to the fact that some amount of air is changed into water (this would result in a change in the absolute weight of the water). The changes in specific weight can thus only be explained by the expansion of the water when freezing. This converges with what Van Helmont wrote in his letter to Père Marin Mersenne on the 30<sup>th</sup> of January, 1631: “*glaciari ipsum est actus effectivus et primarius aquae*”.<sup>16</sup> According to Van Helmont, this is a “mechanical” demonstration: *probatur per mechanicam*.

Hereafter, follows a description of Van Helmont’s famous tree-experiment,<sup>17</sup> which Van Helmont also considered to be a mechanical demonstration (“*ostendi in mechanica*”):

But I have learned by this handicraft-operation, that all Vegetables do immediately, and materially proceed out of the Element of Water only. For I took an Earthen Vessel [*vas*], in which I put 200 pounds of Earth that had been dried in a Furnace, weighing five pounds; and at length, five years being finished, the Tree sprung from thence, did weigh 169 pounds, and about three ounces: But I moistened the Earthen Vessel with Rain-water, or distilled water (always when there was need) and it was large, and implanted into the Earth and least the dust that flew about should be co-mingled with the Earth, I covered the lip of the mouth of the Vessel, with an Iron-plate with Tin, and easily passable with many wholes. At length, I again dried the Earth of the Vessel, and there were found the same 200

pounds, wanting about two ounces. Therefore 164 pounds of Wood, Barks, and Roots, arose out of water onely.<sup>18</sup>

Newman & Principe note that this experiment “gives a clear example of his quantitative technique”.<sup>19</sup> The *explanandum* here is the weight and growth of the tree. First of all, the weight of the earth is measured. That the earth has been dried on a fire and is isolated from the external world by means of a plate is significant here, since these conditions guarantee that no other elements than earth could reside in the pot. That the water is distilled (or is rainwater) equally guarantees that no other elements than water reside in the pot. This assumption was later challenged by James Woodward in 1700. In contemporary parlance, we would say that these variables (earth and water) are controlled.<sup>20</sup> Then, the gained weight of the tree is measured (*ca.* 164 pounds). Note however that after five years Van Helmont weighed the “Wood, Barks, and Roots”. Apparently, Van Helmont did not include the weight of the leaves for whatever reason. Notice further that Van Helmont is not worried at all by difference of two ounces. Given that there did not reside any other elements than earth and water in the pot, and that the earth did not diminish significantly, Van Helmont (wrongly) concluded that *only* the water *produces* the growth of the tree.

One remark should be added here. Van Helmont sometimes used the term “mechanical experiment” in a very loose sense. A mechanical experiment does not always refer to an experiment made at the hand of the natural philosopher. For instance, from the fact that flowers follow the motion of the Sun (even when the Sun does not shine), Van Helmont concluded that flowers have some kind of *instinctum*.<sup>21</sup> In this case, no direct intervention or isolation of variables is presupposed. This example shows that Van Helmont’s idea of mechanical experiment is not limited to experiments as “*experimenta*”, that is purposely performed tests of naturalistic theses, but also contained a broader spectrum of rather loose evidence. As I have stressed in the introduction, Van Helmont did not have the same notion of experiment as we do. Van Helmont’s loose usage of the term “mechanical experiment” shows that Halleux’s reduction of it to “proofs taken from the laboratory” is too narrow: for Van Helmont it referred to more than that. In addition to that, Van Helmont allowed for anecdotes (*een geschiedenis*) and loose observations. For instance, the constant dripping of saltpetre in caves is an indication (*een teken*) that stone is transformed again into its primary principle: water.<sup>22</sup>

Although, modern quantitative-like aspects play a role in Van Helmont’s experimental procedures, and although he often stressed the mathematical component in his arguments,<sup>23</sup> it would be clearly wrong to call Van Helmont’s experimental procedures equally quantified as our contemporary ones, in which both the level

of accuracy has become more important (since our means of measurement have expanded drastically) and the mathematics involved has become more complex (e.g. the usage of statistics and formulae).<sup>24</sup> The importance of mathematical arguments in Van Helmont's work is mainly restricted to determination of weights and density-ratios. However, it should be granted that Van Helmont's ordering of the density-ratio's of tin (which he used as his standard unit), iron, copper, silver, lead, mercury, and gold differs from the modern ones by only an average of less than 2 percent.<sup>25</sup> It should be kept in mind that these were *proportions* between the specific weights of these materials, not absolute values. The exact values are mostly presented roughly and full details are in most cases not treated (at least in the published versions). The prominence of the mathematics involved in weighing procedures derived from Van Helmont's thesis that the quantity of matter remains constant during chemical reactions.

In Van Helmont's work we clearly see an *interventionist approach* towards scientific inquiry. According to such an approach, causal relations can be discovered by actively manipulating natural processes. *Generally*: If we wish to establish whether *A* causes *B*, we will need to establish whether deliberate and purposive variations in *A* result in variations in *B* – *while keeping fixed all other factors*. If *A* produces the expected changes in *B*, the causal relation is established. That other factors are kept fixed is essential here: it allows us to reason that the variations in *B* can *only* be explained by referring to the variations in *A*. A “relatively closed system” (see *infra*) precisely serves as a *locus* in which the keeping fixed of factors is facilitated. I will begin by clarifying my terminology; then I will show how it is embodied in Van Helmont's experimental practice.

Let me first of all clarify what I mean by the term “closed physical system”.<sup>26</sup> A closed physical system is hermetically isolated and independent from its environment: there are no interactions between components of the system and the surrounding environment. Such a system has a constant number of particles, energy, or volume, etc. Such a system is literally “cut loose” from its environment. A closed system is especially useful to isolate the relevant properties we are interested in. Such a system guarantees us that no other influences are active (and hence, that no external influences need to be adduced for the effects we observe in the system under consideration). In explaining G. H. Von Wright's intuition of closed systems,<sup>27</sup> which allows screening-off causal influences from outside the system, Hans Radder supposes the following definition of physical closedness:

Suppose we have a system *S* localized in space and time with initial and final states *a* and *b*. We now examine the role of state  $a_0$ , which immediately precedes *a* and is therefore outside *S* [note that  $a_0$  is produced only by active and intention-

al interference]. If system  $S$  is to be closed in the above sense, then firstly  $a_0$  must not be sufficient for  $a$ , and secondly, not sufficient for all next stages of  $S$  up to and including final state  $b$ . Thus for closedness a first condition is that the system will not ‘by itself’ move from state  $a_0$  to  $a$ . Furthermore  $a_0$  must not ‘influence’  $S$  through one of the intermediate states or the final state, i.e.  $a_0$  must not be sufficient for one or more of these states.<sup>28</sup>

The idea is that by purposive intervention we produce the required initial state in a closed system where – by definition – no other causal variables are active or interfere with the internal processes. The causal influence of  $a_0$  is strictly restricted to producing  $a$  and it has no effect on what happens further in the closed system. Of course, in practice we do not have closed physical systems at our disposal. The best we can do is to try to create “relatively closed physical systems”.<sup>29</sup> Creating relatively closed systems is a way of controlling variables – of course, Van Helmont did not himself use terminology like this. However, his practice is embodied by this procedure. Van Helmont frequently used the sphere as a relatively closed physical system. This interventionist approach which is especially striking in the works of Van Helmont is a particularization of *scientia operativa*.

In the thermoscope experiment we discussed, the vessel is used to keep the amount of air and fluid fixed. Hence, we are able to screen off the external addition of air or water as being causally relevant for the observed process. In other words, the putative increase of water *could*, assuming this set-up, only be produced by the air contained in the vessel. Now we are in a position to properly test whether the heating of the air (our active intervention  $a_0$ ) in  $A$  produces the fluid in  $BC$  to move or creates an increase in the amount of fluid. This turns out not to be the cause. The ice-experiment takes place in an isolated vessel, wherein the total amount of water is kept fixed. Our intervention is to freeze the amount of water which we have weighed on beforehand and then to let it melt again. Van Helmont established that the variations in the specific weight of the water cannot be caused by the fact that some amount of air is transformed into water (since the absolute weight of the water remains the same). The variations of the specific weight of water are caused by the expansion of the water itself. Studying the behaviour of a growing tree is not possible in a closed system – for the obvious practical reason that the tree would simply cease to exist without water and oxygen. What we can do is try to control as many variables as possible. This is what is attempted in Van Helmont’s tree-experiment: the earth is kept constant and the water is purified. According to Van Helmont, only the addition of the water can explain the growth of the plant. In many of Van Helmont’s experiments, pro-

cedures of keeping variables fixed – as well as reference to relatively closed physical systems, in which all external variables are screened off – frequently occur. Van Helmont had a particular and profound insight in the idea that knowledge of nature is produced by isolating certain natural processes or creating – or at least, trying to create as good as possible – relatively closed physical systems. The sphere is paradigmatic for this practice.

## Notes

<sup>1</sup> Van Helmont's works were also translated in French by Jean Le Conte: *Les Oeuvres de Jean Baptiste Van Helmont traitant des principes de médecine et physique* (Lyon: 1670) and in German by Christian Knorr von Rosenroth (who was assisted by Van Helmont junior): *Aufgang der Artzney-Kunst* (Sulzbach: 1683). For a thorough study of the dissemination of Van Helmont's work in the seventeenth century see A. Clericuzio, "From Van Helmont to Boyle. A Study of the Transmission of Helmontian Chemical and Medical Theories in Seventeenth Century England," *BJHS* 26(3) (1993): 303-334.

<sup>2</sup> In contrast to a thermometer, a thermoscope does not have a scale. A careful reading of the 1648 edition is advisable here. This experiment is absent in *Dageraad*. Strunz is one of the few authors who briefly discusses the thermoscope experiment (F. Strunz, *Johann Baptist Van Helmont (1577-1644)* (Leipzig/Vienna, Franz Deuticke: 1907), 40-42).

<sup>3</sup> J. B. Van Helmont, *Ortus Medicinae, id est initia physicae inaudita. Progressus medicinae novus, in morborum ultionem ad vitam longam* (Amsterdam, Elsevir: 1648), 60.

<sup>4</sup> Van Helmont, *Ortus Medicinae*, 62.

<sup>5</sup> J. B. Van Helmont, *WORKS, Containing his most Excellent Philosophy, Chirgury, Physick, Anatomy. Wherein The Philosophy of Schools is Examined, their Errors Refuted and the Whole Body of Physick REFORMED and RECTIFIED. Being a new rise and progresse for PHILOSOPHY and MEDICINE, for the Cure of Diseases and the Lengthening of Life*, transl. by J. Chandler (London: 1664), 61. Translation of: "Praeparatio demonstrationis. Est maxima, quod aer patiatu dilationem, & constructionem juxta qualitates caloris, & frigoris, & quod justa extensio quantitatis in aëre non habeantur, nisi cum est temperatus." (Van Helmont, *Ortus Medicinae*, 62).

<sup>6</sup> Van Helmont, *Ortus Medicinae*, 62.

<sup>7</sup> Van Helmont, *Ortus Medicinae*, 65.

<sup>8</sup> Van Helmont, *WORKS*, 60. Translation of: "Heer autem apud Idiotas ostentabat, se quandoque Patavii suis Matheseos Professore. Quare volui in charta demonstrare, ipsius omnimodam ignorantiam Matheseos." (Van Helmont, *Ortus Medicinae*, 64).

<sup>9</sup> Van Helmont, *WORKS*, 60. Translation of: "Itaque juxta hypothesin Heer ( quod aer compressus, conversatur in aquam ) liquor nunquam defuisset in vase. (...) Non potest autem siccitatem admittere, in vitro exquisite clauso nisi sua hypothesis destruat, ( nimirum quod aer compressus, mutetur in aquam ) nec iterum ista hypothesis subsistere potest, nisi admiserit continuationem liquoris." (Van Helmont, *Ortus Medicinae*, 65).

<sup>10</sup> J. B. Van Helmont, *Dageraad ofte Nieuwe Opkomst der Geneeskunst, in verborgen grond-regulen der Nature* (Oud Hollandsch van Gelder en Zonen: 1944 [1644]), 61, 64, cf. 114. Van Helmont argued that fat and oils can be transformed again into water by distilling the soap he obtained

from mixing fat with alkalis (Van Helmont, *Dageraad*, 109; W. R. Newman and L. M. Principe, *Alchemy Tried in The Fire, Starkey, Boyle and the Fate of Helmontian Chymistry* (Chicago/London, Chicago University Press: 2002), 79).

<sup>11</sup> Van Helmont noted that: “Voorts bewijst oock onze ervarentheyt dat alle vast lichaam, des hout, gewas, visch, vleesch, alle sout, swavel, keye, marchasite, aerde, sandt, steen, metael en bergwerck, wordt by konst [i.e. by “*vuer-konst*”, “by art”] verkeert tot een daedelijck sout, *bestaende in het selve zijn voorigh gewichte*, en dat van dit sout, wordende daer nae dickwils geprobeert met het *specificum corrosivum* van Paracelsus, verandert gansch en geheel in een vluchtig water ’t welck ten lesten soet wordt als regen-waeter, mits dat het voorsegde corrosijf daer van wordt gescheyden *sonder verlies van het gewichte* [...]” (Van Helmont, *Dageraad*, 51 [emphasis added]).

<sup>12</sup> The mercurial alum example Newman & Principe discuss can be cast in similar terms (Newman and Principe, *Alchemy Tried in The Fire*, 80-83).

<sup>13</sup> Van Helmont, *WORKS*, 64.

<sup>14</sup> Van Helmont, *WORKS*, 75. Translation of: “Probatum per mechanicam. Imple lagenam vitream & magnam, fructis glaciei, collum vero claudatur sigillo Hermetis, id est, per vitri ibidem liquationem. Ponatur haec tum lagena, in bilance, adjecto pondere, in oppositum, & videbis quod propemodum octava sui parte, aqua, post resolutam glaciem, erit ponderosior seipsa glacie. Quod cum millesies ex eadem aqua fieri potest, reservante semper idem pondus, dici non potest, quod ejus pars aliqua in aerem sit versa.” (Van Helmont, *Ortus Medicinae*, 79).

<sup>15</sup> T. S. Patterson, “Van Helmont’s Ice and Water Experiments,” *Annals of Science* 1 (1936): 463-464; Patterson’s interpretation is also followed in Newman and Principe, *Alchemy Tried in The Fire*, 72-74.

<sup>16</sup> M. Mersenne, *Correspondance du P. Marin Mersenne Religieux Minime* (17 vol.), P. Tannery, C. de Waard & R. Pintard (eds.) (Paris, Presses Universitaire de France: 1932-1988), vol. 3, 61.

<sup>17</sup> As is widely known, Robert Boyle (1627-1691) accepted the experiment’s validity and noted that Van Helmont is “an Author more considerable for his experiments than many Learned men are pleas’d to think him” (R. Boyle, *The Sceptical Chymist or Chymico-Physical Doubts and Paradoxes, Touching the Spagyrist’s Principles Commonly called Hypostatical* (London, F. Cadwell for F. Crooke: 1661), 111-115). Boyle was not alone in his praise for Van Helmont: Antoine Laurent Lavoisier (1743-1794) also praised Van Helmont (Newman and Principe, *Alchemy Tried in The Fire*, 297-303).

<sup>18</sup> Van Helmont, *WORKS*, 109. Translation of: “Omnia verro vegetabilia immediatè, & materialiter, ex solo aquae elemento prodire hac mechanica didici. Caepi enim vas terreum in quo posui terrae in clibano arefactae **lb** 200, quam made feci aqua pluvia, illique implantavi truncum salicis, ponderantem **lb** 5. ac tandem exacto quinquennio, arbor inde prognata pendebat **lb** 169, & circiter unas tres. Vas autem terreum, sola aqua pluvial, vel distillata, semper (ubi opus erat) maduit, eratque amplum, & terrae implantatum, & ne pulvis obvolitans terrae commisceretur, lamina ferrea, stanno obducta, multoque foramina pervia, labrum vas tegebat. Non computavi pondus soliorum quaterno autumno deciduorum. Tandem iterum siccavi terram vasis, & repertae sunt eadem librae 200 duabus circiter unciis minus. Librae ergo 164 ligni, corticum, & radicum, ex sola aqua surrexerant.” (Van Helmont, *Ortus Medicinae*, 108-109).

<sup>19</sup> Newman and Principe, *Alchemy Tried in The Fire*, 79; see D. Hershey, “Misconceptions about Helmont’s Willow Experiment,” *Plant Science Bulletin* (49)3 (2003): 78-84 for an good account of the successfulness of Van Helmont’s tree-experiment.

<sup>20</sup> Note, however, that in Nicholas of Cusa's presentation of the "experiment" in his *Idiota de staticis experimentis* (Codex Cusanus Latinus 218, folia 132<sup>r</sup>-137<sup>v</sup>) such screening-off procedures are accentuated less (H. M. Howe, "A Root of van Helmont's Tree", *Isis* 56(4) (1965): 408-419, 408).

<sup>21</sup> Van Helmont, *Dageraad*, 333). I have run through Van Helmont's collected work in search for relevant fragments containing reference to mechanical experiments. The example with the flow-ers was one of the few examples I found.

<sup>22</sup> Van Helmont, *Dageraad*, p. 202.

<sup>23</sup> See for instance, Van Helmont, *Works*, 60, 82, 326. Van Helmont criticised his adversaries for not paying attention to the mathematical details of experiments: "*Quare volui in charta demonstrare, ipsius omnimodam ignorantiam Matheseos.*" (Van Helmont, *Ortus Medicinea*, 64).

<sup>24</sup> Newman & Principe conclude their study by claiming that "Van Helmont used no less mathematics than most modern-day chemists" (Newman and Principe, *Alchemy Tried in The Fire*, 319).

<sup>25</sup> Newman and Principe, *Alchemy Tried in The Fire*, 74-75)

<sup>26</sup> See A. Pickering, "The Hunting of the Quark," *Isis* 72(2) (1981): 216-236 and H. Radder, *The Material Realization of Science, A philosophical View on the Experimental Natural Sciences, Developed in Discussion with Habermas* (Assen/Maastricht, Van Gorcum: 1988).

<sup>27</sup> G.H. Von Wright, *Explanation and Understanding* (London, Routledge and Kegan Paul: 1971).

<sup>28</sup> Radder, *The Material Realization of Science*, 63-64 [subscripts and italics added]).

<sup>29</sup> Pickering, "The Hunting of the Quark," 218.



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# Alchemical *Versus* Chemical Use of Distillation Techniques And Materials: Their Mutual Influences And Divergent Developments

*Joaquín Pérez-Pariente\**

## Introduction

Recent scholarship on the alchemical experimental practices during the Renaissance to the Early Modern Period in Europe has revealed that some unexpected quantitative approaches were followed by contemporary alchemists in their laboratory work (1, 2). However, alchemy is still reported as being more connected to religion and occult practices than with true science (3). This is in spite of the well documented “relationship of alchemy to the pursuit of natural knowledge”, given by B. T. Moran (4). Prior to Moran’s view, it was generally acknowledged that only laboratory techniques and equipment invented by the alchemists were worth of considering in the development of chemistry, and that this distinction applied particularly to distillation procedures and glassware. However, and surprisingly, these technical innovations are frequently considered by scholars (even those possessing some technical background) with no reference to the alchemical context in which they were developed, and hence they are generally, without reservation attached to incipient chemical knowledge. On the other hand, and opposite to this view, it is not uncommon to find in the literature uncritical attributions of a supposedly alchemical meaning to any prior 17<sup>th</sup> century work that deals with distillation, particularly if iconographic material is included. In both cases, the purposes for which the distillation procedures were originally used remain in the shade, thus excluding a proper evaluation of either their alchemical or their chemical relevance. Hence, both approaches have distorted the proper appreciation of many documents that are relevant for the history of chemistry.

Placing distillation techniques (5) in the right context is a powerful tool, a sort of Ariadna’s thread, to distinguish between chemical and alchemical practices before the 18<sup>th</sup> century, and in this way to critically evaluate the alchemical heritage of modern chemistry.

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## Alchemical Distillations

Herein several examples will be provided to show the specificity of the alchemical use of distillation techniques and how these techniques experienced a progressive transference process to the metallurgical and to the pharmaceutical fields; interestingly, the theoretical approaches borrowed from alchemy were only to survive in pharmacy. Following this pattern the 16<sup>th</sup> century distillation of strong acids for metallurgical operations are seen as no more than a technical operation, whilst the same equipment, furnaces, glassware and materials were used for the preparation of chemical remedies in a philosophical context still impregnated with an alchemical legacy. There is a general agreement that distillation and the laboratory material associated with this technical practice have played a fundamental role in the historical process of making chemistry an independent science. In this paper “distillation” will be used in a sense broader than that attached to this word in modern science, and will also include sublimation.

Descriptions of distillation techniques and the corresponding glassware and laboratory equipment appeared for the first time in Graeco-Egyptian alchemy (6), while they are not mentioned at all in the contemporaneous technical Leyden Papyrus X and Stockholm Papyrus, which collect artisan’s recipes for dyeing, jewellery making, metal alloys and some other crafts (7). According to the historical records, available so far, we have to conclude that distillation was deliberately invented by Graeco-Egyptian alchemists for alchemical purposes. This poses the question, what might these purposes have been? This question is a reformulation of that which Arthur John Hopkins made in the preface to his book (8), “What was the central penetrating thought [of the alchemists] and whence did it come?”. Although “a comprehensive explanation [of alchemy] is still lacking”, as S. J. Linden quotes again from Hopkins (9), something about these practices has been unveiled. Hopkins offered a coherent picture of the alchemical procedures of the Egyptian alchemy in what he called “the standard method”, a four step process aiming to impose, in a progressive way, on a chaotic matter of black colour, the so-called *prima materia*, the necessary “water”, “air” and “fire” to promote its evolution. A process which imitated the natural evolution process of the minerals they believed took place inside the mines, the “womb” of the Mother Earth (10). Distillation was conceived in this context to obtain a variety of substances rich in these elements. The abundant references to sulphur and sulphur-containing compounds in Graeco-Egyptian alchemy (11) suggests that they were seen as particularly suitable for attracting *pneuma*. This was a concept adapted from Stoic philosophy, the “vital spirit” that pervades the entire universe and made possible the existence of life, associated indeed to “fire” and “air”. It is not surprising nor arbi-

trary the choice of sulphur-rich compounds as preferred “pneumatic” materials, we only need to recall the predominance of metal sulphides among the common metal ores, which were seen, according to extremely old metallurgical traditions, as mineral *embryos*, entities alive and maturing in the natural world.

While distillation techniques were designed for recovering and collecting *pneuma*-rich compounds, other apparatus were invented for submitting a variety of materials to the action of these “spirits”, such as the *kerotakis*, which was inspired by the instrument used by Greek painters for gently heating their wax-based colours (12). Volatile solid substances like sulphur or arsenic compounds placed in the lower part of a closed long-necked vessel were heated and the resulting vapours allowed to act upon the target metal placed in the upper part of the instrument. Mercury was most probably also used in this operation, as it ranked among the alchemical “spirits”. Powdered compounds (eventually metallic amalgams as well) were most probably formed as a result of these operations, which allowed the operator to incorporate *pneuma* into the resulting compound. Under this physical state of “division”, where the metal lost its most characteristic sensory properties like hardness, malleability or colour, thanks to the action of the “*pneumatic*” vapour, the matter was ready to evolve toward a higher degree of perfection helped by the alchemist’s manipulations, which included the addition of silver “seeds” first, and later gold “seeds” to the “earthy” material so prepared (impregnated with *pneumatas*).

The process of depriving a metal of its more notorious physical properties, their *morphe*, as an initial requirement to promote its evolution and replicate at laboratory scale the Creation process (13), become central to alchemy. Several new procedures aiming to accomplish this target were investigated by Muslim alchemists in the Middle Age. *Sal ammoniac* (essentially, ammonium chloride), obtained by sublimation, appeared in the writings of Razhes as a new “spirit” able to transform many metals not only into salts, but also even into “waters”. Indeed, by distilling together alum (potassium aluminium sulphate), *Sal ammoniac* and quicksilver, for example, the previously unknown mercuric[mercury(II)] chloride was obtained. As this possesses a low melting point, it could be brought easily to a liquid state, a kind of “water”. Moreover, mercuric chloride became in the hands of the alchemists a powerful chlorinating agent, for it allowed the preparation of many other metal chlorides, for example, the low melting point antimony trichloride. These procedures led to the very much sought target, that of “dividing matter into fine parts”, as it appears in Razhes’ work (14), a clear continuation of the basic purpose of the Graeco-Egyptian alchemists. Dissolution of a metal in an acid

or its transformation into a powdered compound by calcination were also both considered to produce the “division” of matter.

It is important to understand that the chemical compounds obtained by distillation procedures were deliberately used within a fully alchemical context, as they were regarded key materials according to alchemical theoretical approaches. Their discovery and use manifest a noteworthy, often overlooked progress in the alchemical manipulation of matter.

The alchemical work of the Muslim alchemists found its way into the medieval European culture thanks to the translations of alchemical documents from Arabic to Latin that started in Spain in mid-12<sup>th</sup> century. Alchemy soon flourished in Latin culture, exhibiting a renewed impetus which was nevertheless rooted in the very same ground in which the first alchemists had already laboured.

A few examples suffice to show this old heritage in the western alchemical practices, while at the same time new experimental procedures were developed, very much in the same way Muslim alchemists did before with respect to Graeco-Egyptian alchemy. The pseudo-Llulian work, known as *Testamentum* (14<sup>th</sup> century), highly esteemed by European alchemists through the ages, clearly points to the same target, “*The more utilized the matter would be, the more and best will have the virtues of the celestial and superior nature*” (15).

The early alchemical experiments of Newton were also marked by the preparation and use of several metal chlorides, and his later work and that of Robert Boyle before him, using an antimony-silver alloy treated with mercury, following the path suggested by the alchemist George Starkey (16) can also be interpreted in the light of Graeco-Egyptian alchemy (17).

Muslim alchemical works prescribed the use of alum and vitriols and several salts for laboratory operations. It was probably as a prolongation of intense experimentation with these inorganic compounds that the first recipe for the preparation of *aqua fortis* (concentrated nitric acid solution) appeared in the writings of the Latin pseudo-Geber at the end of 13<sup>th</sup> century (18). This new material was able to bring silver into an aqueous state, and the addition of *Sal ammoniac* to the acid solution allowed it to dissolve gold as well. This was one of the most significant innovations of the early Latin alchemists, but it is important to realise that it was the product of a continuous experimentation to find efficient agents for achieving the much sought *amorphization* and *utilization* of metals.

To summarise, a significant development of experimental techniques has been noted which involves an intensive use of distillation and sublimation, guided by

theoretical considerations. Hence, alchemy presents practical and theoretical aspects. It is convenient to distinguish between material achievements, such as instruments, products and procedures, and the theoretical approaches in the analysis of the transference process of both aspects to the experimental domains, other than alchemy, associated to applied chemistry.

## **Distillation for Chemical Purposes**

Regarding material aspects, there is some historical evidence that distillation techniques, developed in the alchemical milieu, were already in practice outside the field of alchemy before the first alchemical text was translated from Arab to Latin in 1144. Indeed, the technical treatise entitled *On divers Arts*, written before 1130 by the monk Theophilus, had already described the production of vermillion, artificial mercury sulphide, by subliming a mixture of sulphur and mercury (19). The first recipes for the preparation of alcohol (ethanol) by distilling wine appeared in the 12<sup>th</sup> century in two different sources; in manuscripts of the collection of technical recipes known as *Mappae Clavicula*, and in the writing of the Salernitan school of medicine. The Muslim physician Abulcassis described the use of distillation to obtain rosewater at the beginning of 11<sup>th</sup> century (20). Most interestingly, none of these documents refers to alchemy, so it must be concluded that in Muslim culture before 11<sup>th</sup> century, distillation and sublimation techniques were put to practical use, for purposes quite different than those initially purported in alchemy, and that these techniques were transmitted separately from the alchemical writings.

In spite of the transference process to applied chemistry, the techniques of distillation and sublimation remained tightly linked to alchemy for centuries. The Italian painter Cennino Cennini described in his *Treatise of the Art* written at the end of 14<sup>th</sup> century, that vermillion and the yellow arsenicum sulphide colours were obtained by “*means of alchemy*”, because both required the use of sublimation (21). More than one century later, the Italian Biringuccio still declared in his *Pirotechnia* that “*this process [sublimation] is peculiar to the alchemists*”, although he described the method correctly (22). Both works were dealing with applied chemistry, and not with alchemy, and Biringuccio clearly distinguishes the application of distillation and sublimation techniques for the preparation of many different chemicals, from the use that alchemists made of these techniques: “*Alchemists make infinite varieties of acids for their solutions and the aforesaid minerals are the basis of all. It is indeed true that they add sublimes and various salts and other corrosive materials according to their opinion. ....certainly I*

*have seen some [spirits] made by certain alchemists, friends of mine, that not only dissolve silver and gold but would also calcined diamonds, I surely believe” (23).*

A very well known sixteenth century treatise on the preparation of medicinal remedies, the *Treasure of the Secret Remedies* by the physician Conrad Gesner, established that “*non-alchemical drugs are those obtained without using distillation neither sublimation. The remedies prepared by using the vessels proper to the alchemists are made of water, air and fire*” (24), noticing again the alchemical origin of both techniques but distinguishing them from their alchemical use. Gesner remarks about the elemental character of the substances obtained by these procedures, introducing theoretical considerations absent in Biringuccio’s book, which dealt with mining, metals and metallurgy.

Theoretical approaches borrowed from alchemy surface essentially only in the works dealing with therapeutics, where they are instrumentalised and used as theoretical guides for laboratory work. One of the most notorious examples of this impregnation of therapeutics by alchemical theory is the so-called “Quintessence theory”, by the Franciscan monk John of Rupescissa in the 14<sup>th</sup> century. Based on the background of the alchemical work elaborated by Roger Bacon in the 13<sup>th</sup> century, which introduced for the first time notions about therapeutic properties of substances prepared by alchemical techniques; the Philosophers’ Stone in the first place, Rupescissa proposed that it was possible to prepare remedies of high curative power by applying distillation techniques to extract what he called the *quintessence*, the incorruptible portion of any elemental substance. Hence it was possible to replicate on Earth the incorruptible nature of the fifth element or Ether, the stuff of which the stars and planetary bodies were made (25). Rupescissa first elaborates on the properties of alcohol, whose inflammable nature and capacity to preserve organic substances from corruption would have conferred the remedies based on it particular curative properties. However, he soon extended his approach to metallic compounds, such as those of antimony. The preparation of the *quintessence* of a certain substance requires in a first place the separation of a liquid product by distillation, and then this liquid product has to be circulated, i.e., continuously redistilled for a long period of time in a special circulatory vessel named *pelican*, abundantly represented in printed works and manuscripts of the period. As Moran has recently remarked, the success of the operation depends basically on the procedure, the prolonged circulation of the liquid inside the *pelican*, more than on the starting material (26). Gesner described the effect of the operation in these words: “*The distilled wine contains in itself the four elements, but due to the continuous motion and agitation, it is converted from corruptible into incorruptible...*” (27). It is necessary to recall at this point that Gesner was

not dealing with alchemy at all, the specific interests of the alchemists were totally absent from his book; he was simply collecting and describing techniques borrowed from alchemy with the manifested purpose of preparing drugs endowed with enhanced therapeutic activity (28).

The importance of the quintessence theory for the history of chemical technology should not be underestimated, because for the first time some chemicals and procedures to be used for practical purposes are specifically designed under the guidance of theoretical considerations, i.e., what we have was technology inspired by science.

*Liber de arte destillandi de simplicibus* by the German surgeon Hyeronimus Brünshwig was published in 1500 (29), and it is one of the most influential books on applied chemistry issued in the early times of printing. In spite of being widely known and referred in every textbook on the history of applied chemistry, some interesting aspects of the book are often overlooked. Brünshwig explains in his book the different techniques to obtain remedies by distilling a variety of medicinal plants. Some of the woodcuts that illustrate the book attach the time of the distillation operation to specific astrological configurations, which would then imprint upon the distilled product specific curative properties. Moreover, a passage of the 1500 edition advises the collection of the dew of May to distil herbs, and a 16<sup>th</sup> century manuscript belonging to the Library of the University of Leyden depicts an nice coloured illustration of this process (30). It is interesting to compare these clear references to the use of dew with some of the engravings in the *Mutus Liber*, an alchemical work published in 1677, which represents the collection and further treatment of the dew, virtually identical to that which was described by Brünshwig (31). Both books acknowledged the intervention of cosmic factors in chemical processes, but while the *Mutus Liber* placed the operation in a fully alchemical context, the one by Brünshwig uses this knowledge for the preparation of remedies. Furthermore, Figure 1 shows that the very same chemical instrumentation was used for alchemical purposes and for the preparation of useful chemical compounds, which nicely illustrates the flow of technical information between alchemy and applied chemistry in the 17<sup>th</sup> century.

## Acknowledgements

The author acknowledges the Historical Library of Universidad Complutense (Madrid) for permission to reproduced the illustrations of figure 1.





Figure 1. Plates belonging to C. Glaser, *Traité de la Chimie* (1663) (left, bottom and top) and *Mutus Liber* (1677) (right) illustrating the use of distillation furnaces. Reproduced by courtesy of “Marqués de Valdecilla” Historical Library of Universidad Complutense (Madrid, Spain).

## References

1. William R. Newman, Lawrence M. Principe, *Alchemy Tried in the Fire* (Chicago: The University Chicago Press, 2002).
2. George Starkey, *Alchemical Laboratory Notebooks and Correspondence*, eds. William R. Newman and Lawrence M. Principe (Chicago: The University Chicago Press, 2004).
3. W. B. Jensen, “The 2005 Edelstein award paper. Textbooks and the future of the history of chemistry as an academic discipline”, *Bull. Hist. Chem.* 3 (2006): 1-8.
4. Bruce T. Moran, *Distilling Knowledge. Alchemy, Chemistry, and the Scientific Revolution* (Cambridge: Harvard University Press, 2005), 9.
5. R. J. Forbes, *A short history of the art of distillation* (Leyden: E. J. Brill, 1948)
6. Pierre E. M. Berthelot, *Les origines de l'alchimie* (Paris : Steinheil, 1885).
7. Pierre E. M. Berthelot, *Introduction à l'étude de la chimie des ancienes et du Moyen Âge* (Paris : Steinheil, 1889).
8. A. J. Hopkins, *Alchemy, child of Greek philosophy* (1934, New York: Rep. AMS Press, 1967).

9. Stanton J. Linden, *The alchemy reader* (Cambridge: CUP, 2003).
10. Mircea Eliade, *Forgerons et alchimistes* (Paris: Flammarion, 1956).
11. Robert P. Multhauf, *The origins of chemistry* (London: Oldbourne, 1966).
12. F. S. Taylor, *The alchemists* (London: Heinemann, 1949).
13. N. H. Clulee, "The Monas Hieroglyphica and the alchemical thread of John Dee's career", *Ambix* 52 (2005): 197-215.
14. Multhauf, *The origins of chemistry*, 140.
15. Raymond Lull, *Testament* (Barcelona: Indigo, 2001). Text from the Mss 280 of Aragon Kingdom Archives (Spain).
16. William R. Newman, "Newton's *Clavis* as Starkey's Key", *Isis* 78 (1987): 564-574; Lawrence M. Principe, "Apparatus and Reproducibility in Alchemy", in *Instruments and Experimentation in the History of Chemistry* (Cambridge: MIT Press, 1998), 55-74.
17. The use of antimony by Graeco-Egyptian alchemists has been already noticed: R. Patai, *The Jewish alchemists* (Princeton: Princeton University Press, 1994).
18. Pseudo-Geber has been identified as the Franciscan monk Paul of Taranto. William R. Newman, *The Summa perfectionis of Pseudo-Geber* (Leiden: Brill, 1991).
19. Theophilus, *On Divers Arts*. Translated from the Latin with Introduction and Notes by John G. Hawthorne and Cyril Stanley Smith (New York: Dover, 1979).
20. Multhauf, *The origins of chemistry*, 204.
21. Cennino Cennini, *Il libro dell'arte*. Spanish translation (Madrid: Akal, 2002).
22. Vannoccio Biringuccio, *Pirotechnia*, 1540. From *The Pirotechnia of Vannoccio Biringuccio*, translated and edited by C. S. Smith and M. T. Gnudi: Dover, 1990).
23. Biringuccio, *Pirotechnia*, 188.
24. Conrad Gesner, *The Treasure of Evonymus*. English translation (London: 1559). Spanish translation by A. Manrique and A. Fernández (Madrid: Instituto de Estudios Superiores del Escorial, 1996).
25. John of Rupescissa, *De Consideratione quintae essentie rerum omnium* (Basel: 1561).
26. Moran, *Distilling Knowledge*.
27. Gesner, *The Treasure of Evonymus*, Spanish translation, 291.
28. See as an example of medical-oriented Spanish distillation book: Diego de Santiago, *Arte Separatoria* (Sevilla: 1598). Reprinted with an introductory study by J. M<sup>a</sup> López Piñero and Eugenio Portela Marco in 1994 by Instituto de Cultura Juan Gil-Albert, Diputación de Alicante.
29. H. Brünswig, *Liber de arte distillandi de simplicibus* (Strassburg : 1500).
30. Jacques van Lennep, *Alchimie. Contribution à l'histoire de l'art alchimique* (Brussels : Crédit Communal, 1984).
31. *Mutus Liber* (La Rochelle: Petrum Savouret, 1677). The copy belonging to the "Marqués de Valdecilla" Historical Library of Universidad Complutense of Madrid has been consulted.



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## Borderlines or Interfaces in the Life and Work of Robert Boyle (1627-1691): The authorship of Protestant and Papist revisited

*D. Thorburn Burns*<sup>\*</sup>

At least four borderlines or interfaces can be distinguished in the life and work of Robert Boyle, namely those of:

- a) The abrupt changes that occurred during his lifetime between a monarchical government to the commonwealth, followed by the restoration to a monarchical form of government in England and Ireland.
- b) The relatively slow transition from alchemy to chemistry that took place during the Seventeenth century.
- c) The division of his time between studies and publications in science and those on religious, moralistic and ethical topics; and,
- d) his position with regard to the division, within the Christian traditions, as between Protestant and Roman Catholic theology and practices.

The First Earl of Cork (1566-1643), Robert Boyle's father, was a very successful adventurer who prospered and made a vast fortune in the reign of Elizabeth the First, Queen of England and Ireland. He died in 1643 leaving Stalbridge Manor in Dorset and estates in Ireland in his will to Robert. Robert spent the early part of the Commonwealth Period (1649-1660) at Stalbridge (1645-1655) and the later part, from 1656 till well after the Restoration of the Monarchy, in the city of Oxford. Due to his sister Katherine's influential relationships with many in the parliamentary party, coupled with his own insulation from political influences during his "Grand Tour", Robert secured protection for both his English and his Irish Estates [1, 2].

Boyle lived at the time of the gradual transition of alchemy into a more rational based chemistry. He was, in his time, a leading exponent of practical and observational based science, although he made some alchemical excursions [3-5].

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During his working life Boyle divided his time on studies and writing activities between science and religious, moralistic and ethical topics [6]. The ratio of the numbers of his monograph publications in science to those on religious and moralistic topics is 30:10 [7]. It is only in recent times that much attention has been paid to his early, mainly non-scientific, writings, most of which were delayed in publication for many years. Others have remained available only in manuscript form until quite recent times, for example, that started in 1645, at Stalbridge, on "*The ARETOLOGY or Ethicall Elements of Robert Boyle*". This and his early published and unpublished essays are discussed and the various items on "Ethics" in the "Boyle Papers" [8, 9] were assembled by Harwood [10] in 1991. Those for his planned treatise, "*Observations upon the causes and Pretences<and Remedies> of Atheisme*", were very recently brought together by MacIntosh [11].

As noted in Bishop Burnet's sermon at Boyle's funeral, Boyle throughout his life remained an Anglican but exhibited a marked degree of religious toleration. "He was constant to the Church; and went to no separated Assemblies, how charitably soever he might think of their Persons, and how plentifully soever he might have relived their Necessities" [12, 13]. The view that he was seriously anti-Roman Catholic rests on the authorship of a small anonymous tract "*Reasons why a Protestant should not turn Papist: or Protestant Prejudices against the Roman Catholic Religion....*" [14] (see fig. I), this tract is often referred to by its short title, "*Protestant and Papist*". It has in the past, and is by some still, attributed to Robert Boyle. The available evidence with regard to the authorship of this tract is herein revisited. Establishment of the authorship of this tract is regarded as critical to the formation of a fair and balanced view of a key and major aspect of Boyle's work and of his religious outlook on life.

### **The authorship of Protestant and Papist revisited**

It is appropriate, in view of the recent publication of a new edition of *The Works of Robert Boyle* [15] and its omission of probably the most contentious of the anonymous writings, *Protestant and Papist*, which has been frequently attributed to Robert Boyle. As noted by Fulton [7] "this anonymous tract is not included in the earlier collected editions of Boyle's works [16(a), 16(b)] nor was it mentioned in the early biographies of Boulton [17], Birch [18] or Masson [19] or in the *Term Catalogues*". It was assigned to him by Halkett and Laing [20] citing the British Museum Catalogue, but no evidence was given. It is also attributed to Robert Boyle in the current British Library Catalogue and in the web-catalogue of digital facsimile books, "EEBO", "Early English Books Online".

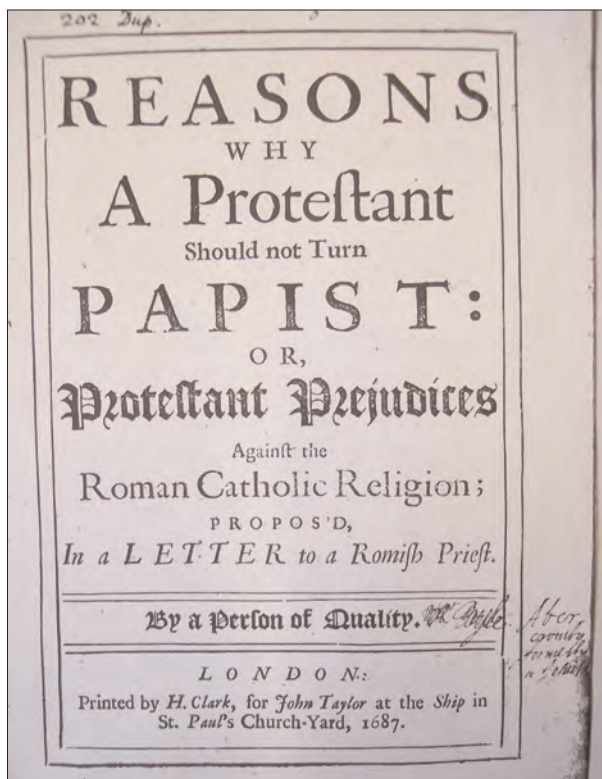


Figure 1. Title page *Protestant and Papist*, corrected version of the first edition, Fulton XXXIII, 175A.

phy of Boyle, and Jacob [23] have avoided the issue by omitting all mention of the tract.

The challenge of the authorship of the tract was taken up by Davis [24], who reviewed all the other anonymous writings now ascribed to Boyle, before discussing in detail the tract, *Protestant and Papist* [14]. Davis then summarised all the evidence against the tract being by Boyle, namely its style, that only one copy has been located with a contemporaneous attribution to Boyle, the significant inconsistency between the anti-Catholic views expressed in the tract as compared to the ecumenical attitudes reflected in Boyle's will and the negative evidence based on extensive study of the Boyle Papers at the Royal Society. For, although these papers contain copies of several anti-Catholic writings by others, not even a scrap from a draft of *Protestant and Catholic* has been found, nor has any document related to it been located [24(a)]. This finding increases in significance when looked at along-

*Protestant and Papist* [14] was discussed at length by More [21]. He cited the entry in the first edition of Fulton's *Biography of the Honourable Robert Boyle* [7a] and recorded Fulton's cautious view, expressed to him in a letter, cited in a footnote to his chapter on "Boyle and Anglican Theology"[21(a)]. However, he then proceeded to discuss the tract as if he were convinced it was by Boyle. Agassi [22] leaned towards the view that *Protestant and Papist* was by Boyle but concluded that it was "more political than theological in nature as well as in thrust; and the view that it is Boyle's is not well founded but is thus far unchallenged" [22(a)]. Maddison [1], in his detailed biogra-

side the fact that a few pages or drafts and/or closely related documents have been located for every single authentic Boyle text published after 1667[15(a)]. Additionally, the tract does not appear in the catalogues of Boyle's theological writings published under his auspices in the last years of his life, in contrast to all works known to be by him [15(b)].

Davis then raised the question, "If Boyle did not write it, then who did?" [24 (a)]. A good case was put forward for the author to be David Abercromby. The main evidence being the contemporary attribution given by a Scottish Divine, Lawrence Charteris, in "A Short Account of Scots Divines" published in James Maidment's *Catalogues of Scottish Writers* [25]. Abercromby was a recipient of Boyle's patronage in the 1680's and translated five of Boyle's books into Latin. The new edition of *The Works of Robert Boyle* [15] has excluded the tract on the basis of the arguments that it was not by Boyle and the cited evidence in favour of Abercromby being the author.

Since the attribution by Davis of the tract, *Protestant and Papist* to Abercromby, Tumbleson [26, 27] has reasserted its attribution to Robert Boyle. His argument that Boyle wrote the tract [26(a)] is that the evolution of thought that occurs between Boyle's *Excellency of Theology* [28] and *Protestant and Papist* [12] parallels that between William Sherlock's *A Papist not Misrepresented by Protestants* [29] and Sherlock's later tract, *A Vindication of Some Protestant Principles* [30]. He also comments, "what is remarkable about Boyle's argument is how unremarkable it is; he covers the same ground in the same terms as his fellow Anglicans". However, in the reference to the tract he seems to be curiously unsure, citing [R. Boyle?] as author, he also states that "the exact provenance of this tract is less important than that the attitude towards Catholicism of this pamphlet is far from alien to the world views implicit in Boyle's oeuvre"[27(a)]. This view is not reasonable. The authorship of this or of similar tracts is of major importance. It is not acceptable to impute views to Boyle, or any other author, dead or living, in the absence of proof.

Tumbleson [27(a)] criticised Davis [24] for his attribution of the tract to Abercromby on two grounds. Firstly, with regard to the value of the contemporary attribution by Charteris to Abercromby, in particular the significance, or truth, that Charteris "may have known Abercromby personally". Secondly he says, "That *Protestant and Papist* resembles other anti-Catholic writings by Abercromby in some respects does not support Davis's case" [27(a)]. The first objection is not convincing in that it matters not that Charteris "may have known Abercromby personally". The real issue is, did he or did he not know what Abercromby had, or had not, written? It is also hard to give any credence to the second criticism by

Tumbleson, when he has used the same “comparison of texts approach” to support the view that the tract was by Boyle [26(a)].

Davis searched carefully for copies of the tract [24(b)] with contemporaneous annotations naming possible authors, but found none that had been attributed to David Abercromby. Just such a copy has now come to light, which supports the view that Abercromby was its author. It is a copy of the corrected issue and is annotated, twice on the title page, both in an early hand and ink. Alongside “By a Person of Quality” is written “R. Boyle”, which has been overwritten “Mr. David” and “Abercromby formerly a Jesuit” is close by, on clear space in the margin (see figure II).

Consideration of the publisher, John Taylor, if he had worked exclusively for Boyle, or for Abercromby would have been helpful to settle the authorship question. However, Taylor did not work exclusively for either, as sight of the publisher’s catalogue proves. In the catalogue, at the rear of the tract, we find among the 10 items listed, Boyle’s *Vulgarly Receiv’d Notion of Nature* [31-33], *Martyrdom of Theodora* [34] by a Person of Honour (now accepted as by Boyle) and *Academia Scientiarum* [35] by D. Abercromby.

Enquiries made at Lambeth Palace Library and also at Stationer’s Hall failed to yield any records of the *Imprimatur*, granted by the Archbishop of Canterbury, July 9<sup>th</sup>. 1687. Records, which, if available, might have shed light as to the authorship of the tract.

In addition to the style within the written text, it is perhaps relevant to consider the layout and the structure of the tract. When compared with all the anonymous works now attributed to Boyle, *Protestant and Papist* [14] is the only work in this group, with no advertisement to the reader, no preface and with a table of contents at the front of the volume. The only two of the anonymous works, now known to be by Boyle, *Martyrdom of Theodora* [34], *The Christian Virtuoso* [36] which have contents (or Heads of the discourse) are both placed at the end of each text. These three observations, on the structure, on the layout, together with the existence of an early-inscribed attribution to Abercromby on a copy of the tract, further support the view that the tract was not written by Robert Boyle, but by David Abercromby.

The opinion that Boyle did not have a sufficiently narrow minded sectarian outlook to have been able to write *Protestant and Papist*, is supported by comments on, and, the records of, his relationships with non-Anglicans. In addition to what he said in the funeral sermon, Bishop Burnet wrote of Boyle in his “Rough Draft of my Life” [37] as follows, “he studied the Scripture with great application and prac-



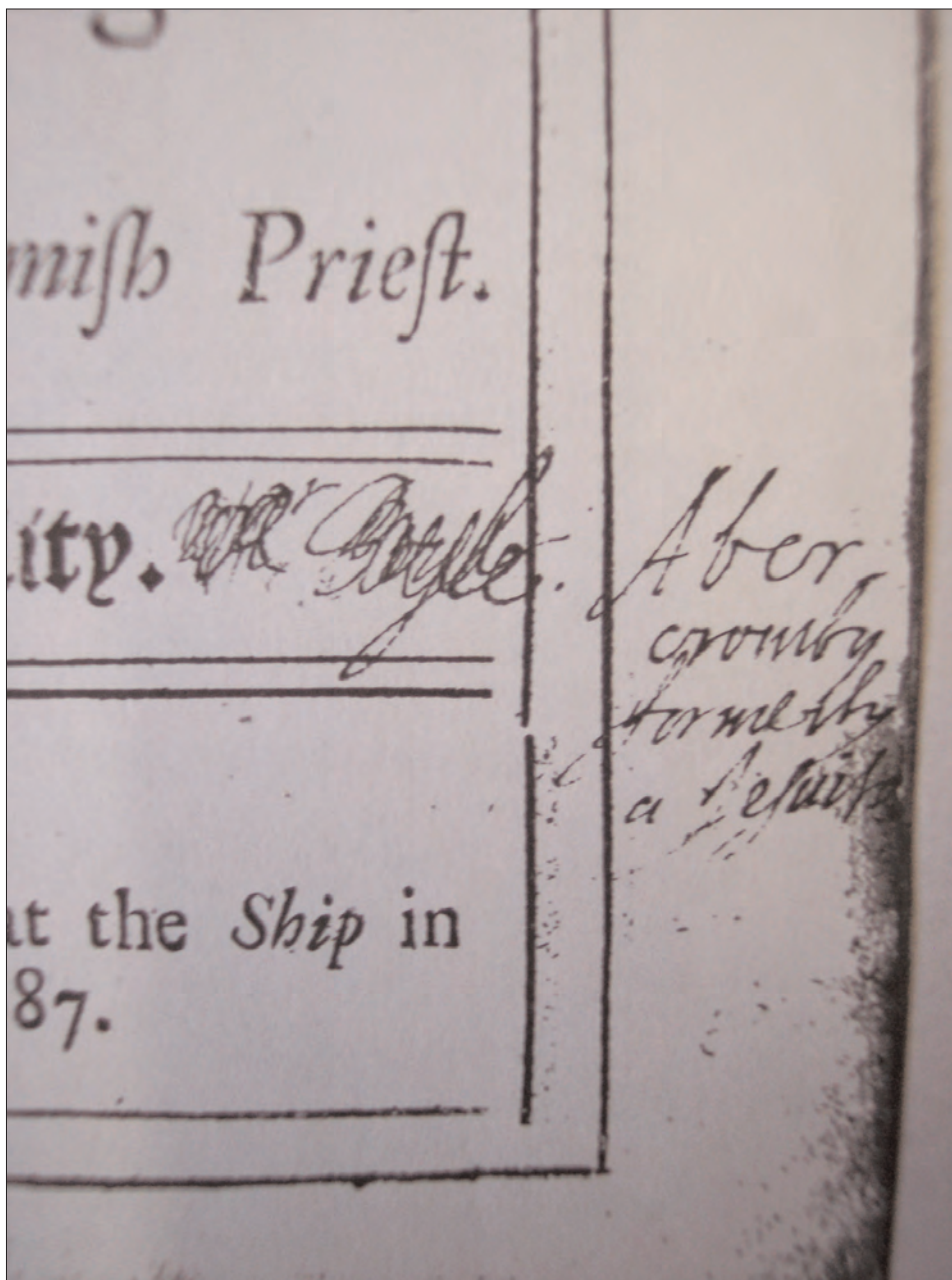


Figure 2. Annotation attributing the authorship of *Protestant and Papist* to David Abercromby. (From collection of D. Thorburn Burns).

ticed universall love and goodnes in the greatest extent possible, and was a great promoter of love and charity among men and a declared enemy to all bitterness and most particularly to all persecution on account of religion”.

Two examples from the extreme ends of the Judaic-Christian spectrum illustrate well Burnet’s view as to Boyle’s lack of religious bigotry. First, when Boyle was in the Netherlands, February-April 1648, he visited Menasseh ben Israel whom he regarded as “the Greatest Rabbi of his Age”, and mentioned him several times in his works [1(a)]. A second and important example, is Boyle’s friendship with the Italian, Count Lorenzo Magalotti, who was secretary to Cosmo III during his London visit 1667-1668. When Magalotti became ill Boyle visited him and sat by his bedside for two or three hours daily [38]. Afterwards they kept in touch and it is clear from Magalotti’s letter, dated Spring 1672 [39], that they had a mutual regard one for the other. The letter also contains a discussion of Boyle religious tract *Seraphic Love* [40] as well as reasons as to why Boyle should turn to Rome.

## Conclusions

Consideration of the structure and layout of *Protestant and Papist*, the documented details of Boyle’s relationships and friendships with people across the whole Judaic-Christian spectrum which demonstrate his lack of religious bigotry, and the existence of a copy with an early inscribed attribution to David Abercromby, support further, Davis’ view that the tract was written by David Abercromby and not by Robert Boyle.

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## References

1. R. E. W. Maddison, “The life of the Honourable Robert Boyle”, Taylor & Francis, London, (1969).(a) p. 73.
2. M. Oster, “Virtue, providence and political neutralism: Boyle and Interregnum politics” in M. Hunter (editor), “Robert Boyle reconsidered”, Cambridge University Press, (1994).



3. M. Boas, "Robert Boyle and Seventeenth-Century Chemistry", Cambridge University Press, (1958); reprinted Kraus Reprint Co., New York, (1968 and 1978).
4. L. M. Principe, "Boyle's alchemical pursuits", in M. Hunter (editor), "Robert Boyle reconsidered", Cambridge University Press, (1994).
5. L. M. Principe, "The Aspiring Adept: Robert Boyle and his Alchemical Quest", Princeton, (1998).
6. M. Hunter, "Robert Boyle (1627-1691) Scrupulosity and Science", Boydell Press, Woodbridge, (2000).
7. (a) J. F. Fulton, "A Bibliography of the Honourable Robert Boyle", Oxford Bibliographic Society Proc., 3, 1-172, (1932).  
(b) J. F. Fulton, "A Bibliography of the Honourable Robert Boyle, Fellow of the Royal Society", Second Edition, Clarendon Press, Oxford, (1961).
8. The "Boyle Papers", held at the Royal Society Library.
9. M. C. W. Hunter, "The Boyle Papers: understanding the manuscripts of Robert Boyle", Ashgate, Aldershot, (2007).
10. J. T. Harwood (edited and annotated), "The Early Essays and Ethics of Robert Boyle", Southern Illinois University Press, Carbondale, (1991).
11. J. J. MacIntosh (transcribed and edited), "Boyle on Atheism", University of Toronto Press, (2005).
12. G. Burnet, A Sermon Preached at the FUNERAL OF THE HONOURABLE ROBERT BOYLE AT St. MARTINS in the Fields, JANUARY 7. 1691/2, R. Chiswell for J. Taylor, London, (1692).
13. M. Hunter (ed. and introduction), "Robert Boyle by Himself and His Friends, with a fragment of William Wotton's lost *Life of Boyle*", William Pickering, London, (1994). Burnet's Funeral Sermon, pp. 35-57.
14. "REASONS WHY A Protestant Should not Turn PAPIST: OR Protestant Prejudices Against the Roman Catholic Religion; PROPOS'D, In a LETTER to a Romish Priest. By a Person of Quality", H. Clark for J. Taylor, London, (1687). Fulton 7. XXXIII.
15. M. Hunter and E. B. Edwards (eds.), "The Works of Robert Boyle", 14 volumes, Pickering & Chatto, London, (1999, 2000). (a) Vol. I, p.civ, (b) Vol. I, p. lxxxii.
16. (a) "The Works of the Honourable Robert Boyle. In five volumes. To which is prefixed a Life of the Author", A. Millar, London, (1774).  
(b) "...New Edition", J. and F. Rivington et al., London (1772).
17. R. Boulton ed., "The Theological Works of The Honourable Robert Boyle, Epitomiz'd," Volume I, W. Taylor, London, (1715).
18. T. Birch, "The Life of the Honourable Robert Boyle", A. Millar, London, (1744).
19. F. Masson, "Robert Boyle", Constable, London, (1914).
20. S. Halkett and J. Liang, "Dictionary of Anonymous and Pseudonymous English Literature", New and Enlarged Edition by J. Kennedy, volumes 1-VII, W. A. Smith and A. F. Johnson, Oliver and Boyd, Edinburgh, (1926-1934); Volume 5, pp. 31.
21. L. T. More, "The Life and Works of the Honourable Robert Boyle", Oxford University Press, London, (1944). (a) p. 167.
22. J. Agassi, "Robert Boyle's Anonymous Writings", *Isis*, 68, 284-287, (1977). (a) p. 287.
23. J. R. Jacob, "Robert Boyle and the English Revolution. A Study in Social and Intellectual Change", B. Franklin, New York, (1977).
24. E. B. Davis, "The Anonymous Works of Robert Boyle and the *Reasons why a Protestant should not turn Papist* (1687)", *J. Hist. Ideas*, 55, 611-629, (1994). (a) p. 622, (b) p. 620.

25. J. Maidment (ed.) "Catalogues of Scottish Writers", J. Stevenson, Edinburgh, (1883).
26. R. D. Tumbleson, "'Reason and Religion': The Science of Anglicanism", *J. Hist. Ideas*, 57, 131-156, (1996). (a) p. 148.
27. R. D. Tumbleson, "Catholicism in the English Protestant Imagination. *Nationalism, Religion and Literature, 1660-1745*", Cambridge University Press, (1998). (a) p. 230.
28. "The Excellency of Theology, Compar'd with Natural Philosophy... by T.H.R.B.E.", H. Herringman, London, (1674).
29. W. Sherlock, "A Papist not Misrepresented by Protestants...", R. Chiswell, London, (1686).
30. W. Sherlock, "Short Summary of the Principal controversies between the Church of England, and the Church of Rome. Being a vindication of several Protestant Doctrines...", R. Chiswell, London, (1687).
31. "A Free Enquiry Into the Vulgarly Receiv'd Notion of Nature;...", By R.B., J. Taylor, London, (1685/6).
32. M. A. Stewart, "The Authenticity of Robert Boyle's Anonymous Writings of on Reasons", *Bodleian Lib. Record*, 10, 280-289, (1978-82).
33. J. R. Jacob, "Robert Boyle and Subversive Religion in the Early Restoration", *Albion*, 6, 275-293, (1974).
34. "The Martyrdom of Theodora, and of Didymus. By a Person of Honour", J. Taylor, London, (1687).
35. D. Abercomby, "Academia Scientiarum; Or, The Academy of Sciences...", J. Taylor, London, (1687).
36. "The Christian Virtuoso: SHOWING, That by being addicted to Experimental Philosophy, a man is rather assisted, than Indisposed, to be a Good Christian...By T. H. R. B. Fellow of the Royal Society,...", J. Taylor and J. Wyatt, London, (1690).
37. H. C. Foxcroft (ed.), "A Supplement to BURNET'S HISTORY OF MY OWN TIME derived from his Original Memoirs, His Autobiography, His Letters to Admiral Herbert and his Private Meditations. All Hitherto Unpublished", Clarendon Press, Oxford, (1902).
39. R. E. W. Maddison, "Studies in the Life of Robert Boyle F. R. S. Part I Robert Boyle and some of his foreign visitors", *Notes and Records Roy. Soc.*, 9, 1-35, (1951).
40. M. Hunter, A. Clericuzio and L. W. Principe (eds.), "The Correspondence of Robert Boyle", Vols. 1-7, Pickering and Chatto, London, (2001). (a) Volume 4, pp. 262-314.
41. "SOME MOTIVES AND INCENTIVES To the Love of GOD, Pathetically discours'd in A Letter to a Friend. BY The Hon R. B. Esq;...", H. Herringman, London, (1659). Commonly known as "Seraphick Love", from its running title.

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## Speaking About the Other Ones: Swedish Chemists on Alchemy, c. 1730-70

*Hjalmar Fors\**

On the seventh of June 1766 a prominent Mine Councillor, and leader of the Swedish Board of Mines, faced a difficult task. His name was baron Daniel Tilas, and he was standing in front of the Royal Swedish Academy of Sciences, in the main assembly room of *Riddarhuset*, the Swedish House of Lords. Tilas' problem was that he was to give an oration to the memory of Count Gustav Bonde, a recently deceased Fellow of the Academy. Bonde had been one of Sweden's most influential politicians. In the 1730's he had been the second most influential man in the country, and he was the twentieth generation of his family to hold the highest position that a Swedish nobleman could aspire to, namely, that of Councillor of the Realm. Tilas had probably been given the task because of his deep insight into the sciences, his noble rank, and his well known diplomacy and tact. The problem was that Count Bonde had been a practicing alchemist and hermetic philosopher. This meant that his views on science in general, and chemistry in particular, were quite different from what Tilas and most of the assembled Fellows of the Royal Swedish Academy of Science believed to be true.<sup>1</sup>

There can be little doubt, that by the time of Bonde's death, young chemists had little more than ridicule for his alchemical views. In the words of the famous chemist and mineralogist Axel Fredrik Cronstedt, from a 1758 manuscript:

"[Alchemists,] how should they be known? Answer: [—] one should not search for them among such, who have learnt the operations and basics with mechanical chymists. Mineralogy is even less needed."<sup>2</sup>

Cronstedt also made public attacks on alchemy. In 1760, he gave an oration to the memory of Henrik Teophil Scheffer at the Royal Swedish Academy of Sciences. Scheffer had been the chief assayer of the Board of Mines and also one of Cronstedt's teachers. Cronstedt used the occasion to attack alchemy and its goals, calling them mystical fancies. He said that, "[d]owsing-rods, amulets, the philosopher's stone and potable gold are the fabrications of [catholic] Monks, without

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doubt to mimick similar things, that the pagans bragged about.”<sup>3</sup> Cronstedt was later echoed by Torbern Bergman, Professor of Chemistry at Uppsala, and an influential chemist of great international renown. In his 1769 oration to the memory of mining expert and chemist Anton von Swab, he called alchemical theorising “fancies, that usually have the bad luck to, when closely examined, turn into unfounded figments of the imagination.”<sup>4</sup> Sentiments like these were common among chemists in Sweden in the 1760’s and often expressed in public at the gatherings of the Royal Swedish Academy of Sciences.

In his oration, Tilas first talked about Bonde’s youth and his political achievements. He then proposed to his listeners, that they would join him on a visit to the Count, to see what he did in his spare time. Tilas now switched style, and painted the picture of inviting his listeners into the home of count Bonde:

“But where does our Count go now? We see him hurry into his Cabinet, to throw off his scarlet robes [...]. Let us follow! We find him at work in his Laboratories, preparing assaying furnaces, muffles and crucibles [...] preparing all kinds of mineral samples to find out their contents. All of this I can reasonably recognize.”<sup>5</sup>

And that he would, as Tilas was an expert mineralogist and metallurgist.

After this visit to Bonde’s chamber of assaying, Tilas followed the Count into an inner chamber, his chemical laboratory. He continued:

“... and I can also reasonably understand, what the meaning of the inner chamber is, where [he] works with various other kinds of furnaces, retorts and recipients, conducting chemical investigations.”<sup>6</sup>

Here Tilas recognised what was happening, but since he did not claim to be an expert chemist, he held a low profile. But behind Bonde’s chemical laboratory, there was another door, and now Tilas did not understand anything anymore:

“...furthest in I see another room, intended for deeper reflections, that I in my ignorance, do not dare to describe, and now I notice, although too late, my own lack of ability to explain to you, Gentlemen, [...]the deep insight into the hidden secrets of nature possessed by our Count.”<sup>7</sup>

Tilas then sneaks into the inner room, and from behind the chair of the Count, he steals a look at the papers on his table, and sees a printed treatise: *Clavicula Hermeticae scientiae* or, the Lesser Key of Hermetic Science, from 1732.<sup>8</sup>

In this way, Tilas could both praise Bonde for his achievements as a public man, and reveal his secret life as an hermetic philosopher. The device he used was a stroke of inspiration. Bonde had led a public life in service of his country. He had

been one of the heads of the Cap party. Furthermore, he was one of the few leaders of the Caps who was also widely respected by their main opponents in politics, the Hats.

Scarlet was the ceremonial colour signifying a Councillor of the Realm. When Bonde, in Tilas's account, "threw" off his robes, he left the public life behind and entered a private sphere that was, implicitly, disconnected from his high office. By talking about the three rooms of Bonde's laboratory, Tilas was able to distance Bonde the public man, from Bonde, the secret hermetic. The first room, the chamber of assaying, had its door open to the outside world, and the world of public utility. Hence, it was the closest room to Bonde's life as one of the country's leaders. It was dedicated to utilitarian purposes and connected Bonde with his time as a President of the Board of Mines. The middle chamber, or chemical laboratory, was partly dedicated to utilitarian purposes, and partly to the lofty realms of chemical theory and speculation. The third, hidden room, was pictured as Bonde's secluded refuge, and as such, it did not really harm anyone that it was there, did it?

Tilas extremely clever oration did not save Bonde's face for posterity. His latter day biographers have described him as cloven in two. On the one hand the rational politician, on the other hand a scientific dilettante and weirdo - a recent researcher has even described him as "monstrous".<sup>9</sup> But such descriptions tend to forget that his political work intersected with his alchemical interests: exactly the fact that Tilas had attempted to gloss over. Bonde was a learned man, and a steady supporter for science. It was during his time as President of the Board of Mines (1721-1727) that the chemical laboratory of the Board had been re-established after a long period of decay. At the Board, Bonde also supported attempts to convert iron to steel without loss of weight (a form of transmutation). Furthermore, as Chancellor of Uppsala University in the 1730's, he had proposed that the University should establish a chemical laboratory, although this did not happen until the 1750's.<sup>10</sup> Hence, Bonde's support for chemistry should not be underestimated, especially since he was part of the innermost circle of the Swedish government until 1738, and a well respected figure even among his political opponents after his and his party's fall from power that year.<sup>11</sup>

However, the re-establishment of the *laboratorium chymicum* of the Board of Mines coincided with what would become a permanent split in the previously unified "chymical" tradition.<sup>12</sup> What from now on would be called **alchemy**, or the Paracelsian and Hermetic philosophy and its associated practice of gold-making, would never again find a place at the Board's laboratory. Instead, the laboratory of the Board, and the new Chair of Chemistry at Uppsala would become

strongholds for mechanical **chemistry**. This new “chemistry” was viewed as a part of the new experimental physics in the tradition of the Dutch teacher of chemistry, Herman Boerhaave. Georg Brandt, the leader of the laboratory of the Board from 1727, was a student of Boerhaave’s. Johan Gottschalk Wallerius, Uppsala’s first Professor of Chemistry from 1750, based his courses and his main textbook on Boerhaave’s *Elementa Chemiae* of 1732. The work can thus be regarded as the *ur-text*, or ultimate source, for much of the later Swedish eighteenth century chemistry.<sup>13</sup>

Yet, Boerhaave’s criticism of Paracelsianism is absent from the published works of both Wallerius and Brandt, and both have been portrayed as old-fashioned alchemists, but for different reasons. Nevertheless, neither of them really followed Bonde all the way into the last alchemical room, in Tilas’s metaphor. There is however some evidence that Brandt conducted alchemical experiments in his old age. Torbern Bergman, in his oration to the memory of Georg Brandt, claimed about the older chemist that: “Brandt was not completely free from Alchemical trials the last years of his life; but He set them up and completed them from another foundation and urge, than the so called Gold-makers.” Cronstedt, too, makes a similar remark to this effect.<sup>14</sup> Passages like these have been interpreted as an indication that Brandt was an alchemist. It is also possible to interpret Brandt’s interest in gold-making as a part of a Boerhaavian program of “experimental history.” That is, to collect and repeat the operations of the arts and crafts, including those of the chymical tradition, to evaluate them and make the useful processes public.<sup>15</sup> Furthermore, in the relatively private forum of his chemical lectures, Brandt was disdainful of seekers of the philosopher’s stone, and attempted to disprove Paracelsian matter theory.<sup>16</sup> Therefore the lack of public criticism of alchemy in Brandt’s work, should rather be taken to indicate that such a public stance would have been inopportune before the 1760’s. And given Bonde’s reputation and influence, it might also have been a really bad career move.

In the case of Wallerius the issue is more complex. Historians of science Sten Lindroth and Tore Frängsmyr have gone to some length to portray Wallerius as a kind of scientific misfit, and as an old-fashioned chemist.<sup>17</sup> Frängsmyr has even presented the view that Wallerius was an alchemist, and as such, a deviant in his own scientific culture and time. This latter view, however, is neither supported by any eighteenth-century documents, nor by any other scholar who has studied Wallerius’s chemistry. Quite to the contrary, Wallerius was one of the most internationally well-respected Swedish chemists and mineralogists throughout the eighteenth-century.<sup>18</sup> Let us return to the already quoted passage from

Cronstedt's critique of alchemy, to hammer home this point, since the passage continues with a reference to Wallerius:

"[Alchemists,] how should they be known? Answer: [—] one should not search for them among such, who have learnt the operations and basics with mechanical chymists. Mineralogy is even less needed: For as I have heard from [the alchemist] Baron Hendrich Wrede, all of the printed works of Wallerius were of no use [for alchemy] except some remarks, about the solidification of water into earth or rock and on the mercurification of metals. Therefore when You see nothing of the chemistry of these times — You are in the right company."<sup>19</sup>

It is left to the readers to decide for themselves, if they want to trust Wallerius's former student Cronstedt's judgement, or Frängsmyr's. However, Wallerius differed from Cronstedt insofar as he refused to disown alchemy. In a letter to Torbern Bergman, Wallerius criticised Bergman's dismissal of alchemy, (quoted above), saying that: "Even if alchemical trials have failed for many who are less knowledgeable about the properties of metals, never the less, chemistry have mostly to thank alchemy for important discoveries. Maybe, for that reason, [alchemical trials] should not rightfully and without exception, be called *fancies*."<sup>20</sup> This was a much more cautious position than Bergman's, but by no means an alchemical one. It can by the way be noted, that Bergman in his later years developed an appreciation for, and a more positive attitude to, the "alchemical" trials found in the older chymical literature.<sup>21</sup>

## Conclusions

Chemists' views about their colleagues and their predecessors have long been used to separate the bad guys from the good guys in the history of chemistry. This is considered to be a faulty practice, which obscures the development of chemistry in the eighteenth century. Many of the views on alchemy quoted in this paper, have been taken from orations written by Swedish chemists active in the 1760's, and celebrating the memories of older, recently deceased, colleagues.

As the historical records of the views of the older chemists, they have often been taken at face value, while they really should be treated with much caution. They often contain attempts to rescue the reputation of beloved teachers and friends by emphasising that their work proceeded from mathematics, physics and Newton, rather than theoreticians internal to the chemical tradition, who were now branded as alchemists, and hence as "bad influence". To some extent, they may also con-



tain attempts to make irritating and formerly or presently influential oldsters suspect: such may for example have been the case for Tilas's oration on Bonde.

That the story was not as simple as the victory of the "good" chemists over the "bad" alchemists, should however be clear from the fact that mechanical chemistry gained its first foothold in Sweden at the Board of Mines when Bonde was its president in the 1720's. That said, all of the chemists discussed in this paper, Bonde excepted, pursued mechanical chemistry. That is, they sought to explain chemical transformations in mechanical terms, and they distanced themselves from the Paracelsian tradition to a lesser or greater degree. Yet they choose different strategies to deal with their alchemical heritage.

Before the 1760's, Swedish chemists did not produce any public criticism of alchemy. In the case of Brandt, Cronstedt and others at the Board of Mines, there is clear indication that they really were very critical of Paracelsianism and the quest for the philosophers' stone. This critique remained in manuscripts, private correspondences, and lectures held before small groups of students. One explanation for this cautious attitude was probably the prominent position and influence of Gustav Bonde, but there were most likely also other reasons. In the case of Johan Gottschalk Wallerius, the Professor of Chemistry in Uppsala, it was somewhat different. Wallerius had a positive view of his discipline's past, and was not afraid of saying so. Due to the lack of critique of alchemy in Swedish public life, it may also have bolstered chemistry's to connect to the discipline's history. In the 1760's all this changed. As the older generation was leaving the scene, it became safe and fashionable to slash the connection of chemistry to alchemy altogether. But how this shift interacted with other changes that took place in Swedish scientific and public life in the 1760's is another story, one which remains to be investigated.

## Notes

<sup>1</sup> Daniel Tilas, *Åminnelse-tal öfver... Gustav Bonde... Hållit i Stora Riddarhus-Salen Den 7. Junii 1766* (Stockholm: Lars Salvius, 1766) 1-47. On Bonde see G. Carlquist "Gustav Bonde" in *Svenskt Biografiskt Lexikon* vol. 5, 362 -377, and Carl Michael Edenborg, *Gull och mull* (Lund: Ellerströms, 1997) for an interpretation of his alchemy.

<sup>2</sup> [Axel Fredrik Cronstedt] *Herr Geschwornen Axel Fredrik Cronstedts Bref til Hr ..... om den mystiska naturkunnogheten. Stockholm den 20 Maj 1758*. Bergskollegiums arkiv, huvudarkivet D VI:3, Riksarkivet (National Record Office) Stockholm, 103. This and all following quotations are translations made by the author from Swedish originals.

<sup>3</sup> Axel Fredrik Cronstedt, *Åminnelsetal Öfver ... Henric Teophil Scheffer ... Hållit i Stora Riddarhus-Salen, Den 17. September 1760* (Stockholm: Lars Salvius, 1760) 1-31, on 14.

<sup>4</sup> Torbern Bergman, *Åminnelsetal öfver ... Anton von Swab ....Hållit i Stora Riddarhus-Salen Den 29 Junii 1768* (Stockholm: Lars Salvius, 1768) 1-54, on 27.

<sup>5</sup> Tilas, *Åminnelse-tal öfver Gustav Bonde*, 30.

<sup>6</sup> Tilas, *Åminnelse-tal öfver Gustav Bonde*, 30.

<sup>7</sup> Tilas, *Åminnelse-tal öfver Gustav Bonde*, 30-31.

<sup>8</sup> Tilas, *Åminnelse-tal öfver Gustav Bonde*, 31.

<sup>9</sup> Edenberg even includes the word “monstrous” in the full, (baroque-length) title of his alchemical biography of Bonde. See Edenberg, *Gull och mull*.

<sup>10</sup> Svante Lindqvist, *Technology on trial: The introduction of steam power technology into Sweden* (Uppsala: Almqvist & Wiksells, 1984) 136-37.

<sup>11</sup> For a source that indicates that Bonde also made informal approaches to other politicians, trying to convince them of the utility of chemistry, see Fr. Wilh. Ehrenheim (ed.), *Tessin och Tessiniana* (Stockholm: Johan Imnelius, 1819) 382.

<sup>12</sup> For these distinctions, see William Newman and Lawrence Principe, “Alchemy vs. Chemistry: The etymological origins of a historiographic mistake” *Early science and medicine* 3:1 (1998) esp. 38-41, and Principe, *The aspiring adept: Robert Boyle and his alchemical quest* (Princeton, 1998) 8-10.

<sup>13</sup> Hjalmar Fors, “Kemi, paracelsism och mekanisk filosofi: Bergskollegium och Uppsala cirka 1680-1770” *Lychnos* (2007): 211-44 on 175-76, 183, and Fors, “Occult Traditions and Enlightened Science: The Swedish Board of Mines as an Intellectual Environment 1680-1760” in, *Chymists and chymistry: Studies in the history of alchemy and early modern chemistry* ed. L. Principe (Sagamore Beach: Science History Publications/USA, 2007) 239-52.

<sup>14</sup> [Cronstedt], *Bref om mystiska naturkunnogheten*, 101.

<sup>15</sup> Torbern Bergman *Åminnelse-tal öfver framledne Bergs-rådet och medicinae doctoren, samt K. Vetenskaps sällskapets i Upsala, och Kongl. Academiens i Stockholm ledamot, Herr GEORG BRANDT*, (Stockholm: Lars Salvius, 1769). On Boerhaave and experimental history, see, Ursula Klein, “Experimental History and Herman Boerhaave’s Chemistry of Plants,” *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences*, 34, (2003):533-567 on 537.

<sup>16</sup> [Georg Brandt] *Föreläsningar i kemi* D 1450a, Uppsala Universitetsbibliotek (Uppsala University Library) henceforth UUB, [pp. 135-137, 180-193].

<sup>17</sup> Sten Lindroth, *Svensk Lärdomshistoria: Frihetstiden* (Stockholm: Norstedt, 1975) 402-3. Tore Frängsmyr, *Sökandet efter upplysningen: En essä om 1700-talets svenska kulturdebatt* (Höganäs: Wiken, 1993) 105. For a more exhaustive critique of the positions of these two scholars, see Fors, “Kemi, paracelsism”, note 72.

<sup>18</sup> See f. ex. Fors, Vetenskap i alkemins gränsland: Om J. G. Wallerius *Wattu-riket*”, *Svenska Linnésällskapets Årsskrift* (1996/97): 33-60. Christoph Meinel, “Theory or practice?: The eighteenth-century debate on the scientific status of chemistry” *Ambix* (1983): 126-9, J. R. Partington, *A history of chemistry* vol.3 (London, 1962) 169-172, Henry Guerlac “Some French antecedents of the chemical revolution” *Chymia: Annual studies in the history of chemistry* 5 (1959): 100-1, Hugo Olsson, *Kemiens historia i Sverige intill år 1800* (Uppsala, 1971) 108-115, esp. 113-4. Nils Zenzén, “Johan Gottschalk Wallerius 1709-1785 and Axel Fredrik Cronstedt 1722-1765” in, *Swedish men of science 1650-1950* ed. S. Lindroth (Stockholm, 1952) 92 –97.

<sup>19</sup> [Cronstedt] *Bref om den mystiska naturkunnogheten* 103.

<sup>20</sup> Letter from [J. G. Wallerius] to Torbern Bergman in *Svensk brevväxling till Torbern Bergman* G21 UUB, 675.

<sup>21</sup> This is evident from Bergman’s *Manuscripta T. Bergman* vol. 4. (*Biografiska och litteraturhistoriska anteckningar om kemister m. m.*) D 1459 d UUB.



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## The Controversy Between Leibniz and Stahl on the Theory of Chemistry

Alexis Smets\*

During the early modern period, the relation between chemistry and the other disciplines was in a state of flux. Aristotle's matter theories presented in *De generatione et corruptione* and in the *Meteorologica* had been embedded in his overall hylemorphic system. With the breakdown of the Aristotelian system, it became unclear where chemistry belonged and whether it had to borrow its principles from another science or had to establish them by itself. The situation was rendered even more delicate as chemistry could be divided into a theoretical part, which was strongly related to natural philosophy, and a practical part, which qualified more as an art than as a science.

Seventeenth-century textbooks of chemistry usually opened with a theoretical account of matter; but indeed, they very often defined chemistry as an art, not as a purely deductive science in the manner of the Cartesian project.<sup>1</sup> However, as Leibniz often pointed out, building theories in a deductive fashion was useful, since it permitted to capture and organise elements of knowledge that would otherwise remain scattered.<sup>2</sup> In other words, deduction allowed finding general principles under which elements of knowledge were to be structured.<sup>3</sup> Thus, in spite of the eventual difficulty of erecting chemistry on its own principles, there existed a real need, internal to chemistry itself, for a theory that gave a solid account of practice.<sup>4</sup>

The existence of this tension between chemical and physical theories, and between practice and theory, is the reason why the controversy that arose between Gottfried Wilhelm Leibniz (1646–1716) and Georg Ernst Stahl (1660–1734) is so interesting. The crucial questions of the status and role of theories were at stake there, as well as that of the relations that chemistry should or should not keep with other disciplines, and notably with mechanics and medicine. In 1709, Leibniz had read Stahl's major medical work, published the year before, the *True Medical Theory*.<sup>5</sup> He showed himself interested in the insights of Stahl, who was a famous

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chemist and an equally famous professor of medicine at the University of Halle, but at the same time disagreed with numerous theoretical statements contained in Stahl's work. In order to explain his disagreements, he sent a fifteen-page text to Stahl. Stahl answered with a hundred-page text, to which Leibniz replied with approximately thirteen pages. Stahl composed a last answer that was, however, never sent to Leibniz. In 1720, four years after Leibniz's death, Stahl published the whole controversy under the title *Negotium otiosum (Idle occupation)*.<sup>6</sup>

### Disciplinary problems

When Leibniz composed his first set of Doubts concerning Stahl's *Medical Theory*, both men had in common that they had been rejecting a part of Descartes's and the Cartesians' attempts to reduce all phenomena to matter, figure and motion. According to both Leibniz and Stahl, Descartes's knowledge of chemistry was far too weak, and his attempt to subject this discipline to a geometric or mechanical approach could not account for the specificities of the transformations of materials.<sup>7</sup> In other words, both Leibniz and Stahl felt that Descartes did not produce any explanation that was useful to chemists.<sup>8</sup> However, the details of Stahl's criticism were very different from those advocated by Leibniz. Also, while both men rejected the pretensions of iatro-chemistry, they did so for different reasons.<sup>9</sup> According to Stahl, it was not the task of chemistry to explain the phenomena of organised bodies. Furthermore, according to him, medicine was a specific field that does not allow for an interpretation of the changes that take place in non-organic matter. He concluded that "chemistry is completely useless to the true medical theory."<sup>10</sup> This rejection is to be understood as the expression of Stahl's goal of building different sciences, namely chemistry and medicine, on specific and therefore separate grounds.<sup>11</sup>

Leibniz forcefully disagreed with Stahl's double exclusion of chemistry from the medical field, and of physics from chemistry. He subsequently reinterpreted Stahl's achievements in chemistry and in medicine in the light of his own general philosophy in order to render their different approaches to the behaviour of substances compatible. Whereas Stahl's view was disciplinary precisely so as to allow each different specific field to build its own theory based on its specific problems, Leibniz proposed a general viewpoint from which the problems appeared interconnected and in which the criterion of general coherence was of primary importance. As he put it: "Although men conceive parts in it, and give names to these parts in accordance to their commodity, the entire body of the sciences can be conside-

red as the ocean, which is everywhere continuous without interruption or division.”<sup>12</sup>

This is the general framework within which will be developed the author’s special topic of investigation, namely the notion of chemistry in both Leibniz and Stahl. However, an account is given herein of only a local part of the controversy. This peculiar point concerns the account of matter and of its infinite divisibility—an issue over which Stahl accused Leibniz of making the chemical concept of mixture impossible. Let us note that however local the question of the nature of matter is in the controversy, it is important for the whole discussion because the account of matter partially determines the relation that chemistry and medicine should have. Stahl elaborates an ontological distinction between the structure of chemical matter and the organisation of living bodies, whereas Leibniz postulates micro-organisms in infinitely small entities. This discussion will continue by explaining the contents of both Leibniz’s and Stahl’s viewpoints on chemistry and will give an account of the relation between divisible or corpuscular matter and of the status of qualities. It will be seen that despite the ambivalence of Stahl’s concept of matter, which somewhat oscillates between chemical principles and physical “elements,”<sup>13</sup> the German chemist strongly rejected Leibniz’s conception of an infinitely divisible matter. Moreover, it will be shown that in spite of an apparent strong proximity between Leibniz’s and Stahl’s chemistries, Leibniz’s commitment to the idea of an infinite divisibility was seen by Stahl as a serious impediment to the progress of chemical thought. He felt that with his two notions of corpuscles and of infinite divisibility, Leibniz seemed to give with one hand what he took with the other.

### **Controversy: chemistry and physics (matter, mixture, qualities)**

For reasons that will become more explicit later, in an essay included in the introductory part of his *True Medical Theory* and entitled *On the true difference between the mixt and the living body*,<sup>14</sup> Stahl ruled out the idea, shared by Aristotle and Descartes, of the infinite divisibility of matter.<sup>15</sup> Being not chemical but “purely mathematical,” this idea had a serious implication for chemical philosophy. According to Aristotle, in a *mixtion*, all the parts of the *mixt* were supposed to be of the same nature, that is, mixed according to the same proportion, however far one divided the *mixt*. There was, in other words, no level at which the *mixt* displayed any heterogeneity. As Stahl wrote:

“[Aristotle] regarded the corporeal *mixture* as an act or an effect that penetrates so intimately that body to which it belongs that any corporeal particle, *infinitely small*, still keeps, among all the organic varieties, *the same and proportional mixture* (as one supposes it in animal bodies), whatever the size of the mass and whatever the material quantity and the sensible volume that constitute this body.”<sup>16</sup>

For Stahl, by contrast, a mixture does not have to be identical to all of its components. That is, what *produces* the *mixture* is not the diffusion of the *form* of the *mixture* through the underlying body, but the physical combination of different particles of matter. As a consequence, it would be possible to image a mechanical destruction of a *mixture* by merely retrieving one of its components.<sup>17</sup> From Aristotle’s viewpoint, one must instead *perform an operation* on the *mixture* in order to change its overall form.

Interestingly, from a chemical viewpoint, Leibniz does not appear to have objected to Stahl’s rejection of Aristotle’s concept of *mixture*, while at the same time affirming both the infinite divisibility of matter and thus the right to view the issue from a “mathematical” perspective. Chemically speaking, the two men would seem to have agreed. Leibniz wrote:

“[Stahl] was right when he noticed the Aristotelians’ mistake who ordinarily think that any part of a *mixture*, however small, can be *mixture* in the same way just as the whole. But certainly, it is neither necessary nor intelligible that after having injected salt into water, its parts are transmuted into a salino-aqueous body, but it is enough that they be diffused throughout the water.”<sup>18</sup>

Such a statement seems to advocate corpuscularism. However, from a “mathematical” perspective, things appear differently. For Leibniz, mathematics is not an essentially separate field of research but belongs to the order of abstraction:

“The mathematical does here only differ from the physical as the mental abstraction does from what is concretely in the things. The mental abstractions do not add something false, but retrieve something true. [...] the body is not only infinitely divisible, but actually divided.”<sup>19</sup>

In the abstraction, what is retrieved from the concept of body (or chemical substance) is the fact that in practice, it is difficult to divide, nay impossible. The resulting concept is therefore an infinitely divisible *matter*. Thus, when Leibniz creates such a concept of matter and assesses that this is the true viewpoint on the question, it is also clear that he has erased precisely that element that defines the specificity of the chemist’s activity: the fact that substances have various ways of resisting divisibility. So, “to abstract” might mean that Leibniz has cho-



sen to neglect the practical evidence according to which (in reason, for example, of reversible chemical reactions) there are probably corpuscles, and has consequently posited his concept of matter in an *a priori* approach. Thus, as it is infinitely divisible and in effect actually divided, and having in mind his metaphysical principle of indiscernables which states that no two individual entities can be completely identical, he states that “there is no part of matter in which one will not find numerous varieties.”<sup>20</sup> And he added this *argumentum ad hominem*: “Those who are not conscious of that do not pay tribute to Nature’s incredible majesty.”<sup>21</sup> According to this view, it is false to regard matter as ultimately composed of families of identical constitutive corpuscles. Thus, although Leibniz’s chemical concept of *mixt* presupposes corpuscles, his physical concept of matter abandons them.<sup>22</sup> In turn, if it is clear that according to him, there is no contradiction between physics and chemistry, it is because chemistry simply considers matter from a different perspective than physics.

Despite the fact that on the issue of chemical composition, Leibniz appeared to agree with him, Stahl’s reply to Leibniz was quite negative. He stated:

“Once this opinion concerning the absolute divisibility is admitted, any consideration is comprehended under this approach. That is why I strongly persist in saying that this false and miserable presupposition of the *infinite divisibility* has especially prevented *Aristotle’s disciples* from reaching a true and positive perception concerning the true reason, *material* as well as *formal*, of *mixtion*, such as that of which we usually conceive the existence. From that was born this confused opinion of the Aristotelians, namely, that *mixtion* constitutes a new *form* that communicates itself to all the *matter* and entirely *changes* it.”<sup>23</sup>

Since Leibniz had agreed on the nature of *mixt* bodies, Stahl’s insistence may appear odd. After all, Leibniz had acknowledged unchanging corpuscles and a difference between the components of a *mixt* and the *mixt* itself. Did his extremely theoretical, physicalist account of the divisibility of matter really make a difference, and why? According to Stahl, who at this point is surprisingly affirmative as to the possibility of manipulating the constitutive principles of matter,<sup>24</sup> there are good experimental reasons of refusing Leibniz’s divisibility: “daily experience testifies that these [material constitutive principles] are close enough and sufficiently often under hand.”<sup>25</sup> Of course, what Stahl does not say is how experimentally we would know that these “principles” are constitutive. Besides an “*a priori* reason” that asserts that there is no proof of divisibility, Stahl gives another *a posteriori* reason that consists precisely in what the consequence of infinite divisibility seems to render impossible: the permanency of the “elements,” demonstrated by their resistance to a series of testing: “Rather, one finds a fairly consistent

magnitude, beyond [which] experience shows that it cannot be rendered any more tenuous.”<sup>26</sup>

### **Two conceptions of chemistry, two conceptions of matter and the qualities**

In the introduction, a reminder was given of the commonly perceived need for useful hypotheses and for theories having a strong relation with practice and the chemist's experimental life. It is at this point of the controversy, it is believed, that this need for theories useful to chemistry is the most visible and that one is able to detect a profound difference between Leibniz's and Stahl's respective relation to chemistry. Stahl's response permits one to grasp the nature of the problem: Leibniz's intervention replaces the dual viewpoint (physical and chemical principles) by another one, allegedly better but, through its ontological status, apparently reductionist. Indeed, in Stahl we may observe a hesitation which manifests itself as his oscillating between physical and chemical constitutive principles, where in the first case the viewpoint is more corpuscular (and probably ontological) than qualitative and in the second, more qualitative (and instrumental) than corpuscular. Leibniz, by contrast, zigzags between a physical viewpoint in which the constitutive principles are not material but expressed by forces and a conceptual matter, and a chemical viewpoint where the first elements have an instrumental status or, to be precise, a *derivative* status.<sup>27</sup> At the heart of these theories, in which chemistry seems at risk of being made to depend on physics, stands a no less theoretical issue that has nevertheless a high practical resonance: where to place qualities studied by the chemist and how to think of them?

As far as chemical practice is concerned, it is evident that both Leibniz and Stahl paid a great deal of attention to experimentation and notably to the process of formation of a *mixture*. In this respect, the situation seems quite straightforward. Both men acknowledged the existence of chemical substances and of instruments (water, air, fire) that could either act chemically or mechanically. Also, they both acknowledged numerous operations such as dissolution, vitrification, sublimation, granulation, fusion and fermentation.<sup>28</sup> As these operations may depend on mechanical, chemical or even on biological causes, distinctions could be drawn between their respective effects and modes of functioning. In turn, in the mind of both scientists, they could also be reduced, according to the mechanistic dogma, to local motion, that is, to the displacement of small particles of matter. Furthermore, for both of them, it was evident that quantities and the substitution of chemicals played an important role in building better explanations of chemical

processes. The important point is, however, that although they offered similar explanations for experimental results, that is, although both men might explain an operation by corpuscular means, they differed over the chemical theory itself, in which the experiments and the central notion of quality were embodied.

### Stahl: mixtures and primary qualities

To put it in modern language, for Stahl, chemistry as an art can be said to consist in the techniques of analysis and synthesis, which are grounded in the science of the structure of substances.<sup>29</sup> It is therefore a science of mixtures. In Stahl's own words, chemistry is "the Art of resolving *mixt, compound, or aggregate* Bodies into their *Principles*; and of composing *such Bodies* from those *Principles*."<sup>30</sup> According to him, there are probably four qualitatively different primary principles, namely, three kinds of earths, each being endowed with a specific property, and water. The first earth approximately replaces the former salt principle and has the quality of vitreousness or fusion. The second replaces the former sulphur principle and is actually to some extent Stahl's *phlogiston*; its quality is fat or inflammability. The last earth is the former mercury principle and has the quality of being liquefiable. Above this "atomic" level, there exist several levels of composition. The first is that of the *mixt*, where different principles combine with each other.<sup>31</sup> Given the firmness of the *mixts*, this level of composition actually constitutes yet another atomic level, though not an absolute one. Above that comes the level of the compound bodies. These are made of *mixts* and eventually also of isolated principles. The presence or absence of water in the compounds may determine the degree of resistance of these bodies, which *as mixts* and *as* compounds are too small to be visible. Finally, came the level of the *superdecomposita* or aggregates, which corresponds to our macroscopic level.<sup>32</sup>

And yet, it is at least suggested in both the *Philosophical Principles of Universal Chemistry* and in the Answers to Leibniz that qualities might be caused by physical principles, defined in terms of shaped corpuscles. However, it was also admitted that such constitutive material principles, in spite of the fact that they exist, were not yet known. As to the chemical principles, they are directly related to generic qualities. In the *Philosophical Principles*, chemical principles were presented as uncertain with respect to their reality, but "consider'd only as to their *generical* [sic] *qualities*, they may be allow'd in Compounds." In other words, in compounds, a chemist should be able to find "parts" causing vitreousness while he would find other parts causing fluidity. These qualities are only three in number but besides them, at the levels of *mixts* and compounds, there were stable mate-

rials that also present specific sets of qualities whose variations or absence of variation was the object of the chemist's study. In this sense, for example the colour and specific weight of gold constitute the qualities of the *mixt*. Thus, Stahl's reader may observe a progression or derivation in the construction of qualities. However, it is unclear to what extent the primary qualities play a real role in Stahl's practice of chemistry. Still, in the *Philosophical Principles*, their importance was asserted: they seem to be an instrument for the classification of substances and perhaps for predicting possible or impossible operations. It does not seem that "tables of affinities" could have been born out of this research,<sup>33</sup> but another type of "visualisation" might have come to the light: a system of chemical equation which, based on the qualitative composition, would have yielded a synoptic view of the possible analyses and syntheses of the substances.<sup>34</sup> Here, chemistry appears not only as a useful art, but as an art whose progress will be quicker since principle-qualities were sought that would allow predictability and classification. In conclusion, Stahlian chemistry, besides postulating physical principles, intentionally displays a very limited first set of qualities, followed by a less limited set of secondary qualities which each time correspond to particular *mixts* or compounds.

### **Leibniz: transformations and secondary qualities**

Leibniz's concept of matter and therefore of chemistry was more complicated than Stahl's, being intertwined with several other disciplines at the same time. Because of his dynamics, Leibniz thought of matter as being endowed with derivative forces.<sup>35</sup> For this reason, though he regarded matter as infinitely divisible, he could not regard it as undifferentiated.<sup>36</sup> On the contrary, the movements, which always passed through matter, endowed it with a form or some secondary qualities, sometimes also depicted as textures, or as folds,<sup>37</sup> and thus, explains Leibniz's paradoxically corpuscular approach to physical matter.<sup>38</sup> Metaphysically speaking, no sufficient reason for unbreakable atoms could be imagined. Physico-mathematically speaking, however, from the point of view of the calculus and of dynamics, it is meaningful to speak of different corpuscular levels. A degree of firmness being caused by conspiring movements due to the forces internal to matter itself, and there being no reason for either a first (or last) degree of smallness or a first (or last) degree of force, there will always be a material consistent enough (with regard to the force required for its destruction) to be called a corpuscle. These corpuscles, in turn, could together have a conspiring movement that would cause them to assemble into what with regard to the force necessary to des-

trophy it would constitute a bigger corpuscle—and thus *ad infinitum*. To illustrate this with an orthodox Leibnizian example, let us consider a heap of sand of one cubic centimetre and a piece of stone of the same dimensions. One can divide the heap into two parts with a finger, whereas it is clear that one cannot do the same with the stone. From this operational viewpoint, the stone is clearly to be regarded as a corpuscle. However, “for a boat with a given speed, the wave becomes as hard as a marble wall.”<sup>39</sup> It is clear that the resistance will be extremely different and could result in the destruction of the boat, and in that of the wave. This suggests that for Leibniz, the “physics of materials” is a discipline that studies the transformations of matter.

However, he did not charge his physics with the task of replacing chemistry, which he defined as follows: “of the predicates there exists a science of qualities, [to show] how we find the subjects in which the qualities are found. To this belongs chemistry.”<sup>40</sup> The corpuscles are not “atoms,” and this is so for two reasons: strictly speaking, atoms are regarded as containing no inner part that would be of a nature different from that of the whole, a view that Leibniz cannot accept; also, bodies are only atomic with respect to our current inability to destroy them. Nor are corpuscles merely *instrumental and logic*, because they empirically exist, which indeed bestows a degree of *reality* upon them.

But then, what kind of chemistry will this be, of which Leibniz says that it belongs to a science of qualities (*poiographia*), and what is its status? As to its epistemic status, Leibniz is very clear: “indeed, chemistry will be the practical part of general physics, and in the same way as medicine is related to man and agriculture to plants, so chemistry is related to elements and bodies, either of the same type or roughly mixed.”<sup>41</sup> “Practical,” as distinguished “theoretical,” here means *contingent* and this is so precisely because chemistry belongs to a “poiographia.” Indeed, the qualities have to be regarded as either primary or secondary. Those qualities which in chemistry allow to evaluate the transformations of matter and whose production is *in fine* also one of the goals assigned to chemistry, are the secondary (or derivative) ones, the primary qualities being extremely few in number and belonging to physics. The qualities are what an observer can feel from the object of his senses. Their contingency is due to their sensible origin. Leibniz cited Democritus’s and Leucippus’s adage with approval that “qualities come from opinion and are rather *viewpoints on things*, not things themselves.”<sup>42</sup> As such, the idea of the particular qualities that we encounter in our relation to things, although also dependent on the conditions of observation, nevertheless have some stability and should allow us to conduct efficient proofing of materials.<sup>43</sup> In turn, chemistry produces real transformations and is also at the origin of true knowled-

ge: when a chemist affirms that there is a substance like gold, he is certainly right; when the chemists' categories allow for classifications of materials, they bring answers that will receive a better explanation when translated into the language of physics, but that remain nevertheless true. Let us note that though chemical activity is described as sensory<sup>44</sup> and contingent, this does not mean that this is untrue, and even less that physics could achieve, in Leibniz's eyes, a better result.

Thus, both Leibniz and Stahl acknowledge corpuscles and their conservation in a hidden state in mixtures (whether they be *mixts* or compounds does not really matter here) and there is, generally speaking, no strong difference between their respective views on the practice of chemistry. Both men regarded qualities and their variations in the course of varied experimental operations as a heuristic criterion for the evaluation of the composition of mixtures. Indeed, both of them thought that these qualities were not primary in substances, movement and figure constituting a more fundamental *explanans*—although Leibniz, with his concepts of conspiring movement, diffusion and alteration, suggested an even more fundamental level of explanation.<sup>45</sup>

## Conclusions

To conclude, it is useful to underline two differences. The first the allows formulation of a hypothesis as to Leibniz's and Stahl's respective relations to Cartesianism. The second provides the reason for why it can be believed that Stahl was right in rejecting Leibniz's appropriation of chemistry.

Surprising as it may seem, it seems that Stahl's theory of chemistry was slightly more Cartesian than was Leibniz's. In the context of his atomism, which within the limits of its theoretical scope, it has been taken to have been more realist than merely instrumental, Stahl seems to have thought that the properties of substances should be explained through movement and figure, it being understood, with Descartes, that motion does not belong to the substance but originates from outside. Leibniz, by contrast, despite his acceptance of movement and figure, additionally also injected the concept of forces (although derivative ones<sup>46</sup>) into the material substances themselves.

As to the second conclusion, it seems that Stahl's refusal of Leibniz's definition of chemistry was less due to pure *a priori* reasons than to the fact that the theory of infinite divisibility put at risk his *a posteriori* theoretical construction of the structural levels of matter. This theoretical construction, which could distinguish bet-

ween a *mixt*, a compound and an aggregate, was useful, as explained earlier, in distinguishing chemical from mechanical composition and was thus also related to the classification of operations and of qualitative change. Hence, by considering matter as liable not so much to different levels of organisation but rather to different levels of firmness, that was, by suggesting that there were corpuscles contained in corpuscles, Leibniz probably complicated the work of the chemist, which was aided by the theory of the structural levels. In fact, Leibniz did not seem to regard chemistry as dealing with structures at all: “all bodies belong to chemistry [...] if treated not as structures but as masses.”<sup>47</sup> Stahl could agree with this at most partially, as he regarded the determination of the modes and levels of mixture (each possessing a distinct name: combination; union; juxtaposition) as a relevant task of the chemist.<sup>48</sup> These structural concepts seem to have been weakened by Leibniz’s theory, which undermined their general validity, restricting it to practice. The most interesting feature of this point of the controversy between Leibniz and Stahl is perhaps that the motive of Leibniz’s opposition lay hidden in what distanced him the most from Stahl, namely his actually infinitely divided matter (together with what Stahl felt to be “vague” practical statements), whereas Stahl seemed to advocate the heuristic value of a theory of which he elsewhere acknowledged its uncertainties.

## Acknowledgement

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## References and end-notes

<sup>1</sup> Consult for example Nicaise Lefebvre, *Traité de la chymie* (Paris: Thomas Jolly, 1660); Christophle Glaser, *Traité de la chymie* (Paris: Jean Dhoury, 1672). Same type of definition in Georg Ernst Stahl, *Philosophical Principles of Universal Chemistry*, ed. Peter Shaw (London: John Osborn & Thomas Longman, 1730). See the extended treatment of this question in Antonio Clericuzio, *Elements, Principles and Corpuscles: A Study of Atomism and Chemistry in the Seventeenth Century* (Dordrecht, Boston, London: Kluwer Academic Publishers, 2000).

<sup>2</sup> See, for example, Leibniz’s criticism of Boyle in the letters exchanged between Huygens and Leibniz following Boyle’s death, of 4 February 1692 and 9/19 February 1692 respectively (Gottfried Wilhelm Leibniz, *Der Briefwechsel mit Mathematikern*, ed. Carl I. Gerhardt (Hildesheim: Georg Olms, 1987), 687-90) see also Leibniz’s correspondence with the chemist Johann Andreas Stisser (see Leibniz, *Opera omnia*, ed. Ludovicus Dutens (Hildesheim: Georg Olms, 1989), vol. 2, part 2, 122 ff.). On the usefulness of the elaboration of theories in relation to



the problem of matter and of chemistry, see Leroy E. Loemker, "Boyle and Leibniz," *Journal of the History of Ideas* 16 (1955): 22-43 and Salvatore Russo, "The Concept of Matter in Leibniz," *The Philosophical Review* 47 (1938): 275-292. For the Leibnizian "reinterpretation" of Boyle, see Clericuzio, *Elements, Principles and Corpuscles*, 104.

<sup>3</sup> Leibniz always said that the "discovery" of the laws of dynamics was made through a metaphysical inquiry. For example, see *Discours de métaphysique*, § 21 in Gottfried Wilhelm Leibniz, *Die philosophischen Schriften*, ed. Carl I. Gerhardt (Berlin: Weidmann, 1875-1890), vol. 4, 427-63.

<sup>4</sup> Concerning the problem of the proliferation of scattered or rivalling pieces of knowledge in chemistry, see Bernadette Bensaude-Vincent, Isabelle Stengers, *Histoire de la chimie* (Paris: La découverte, 2001), 56-8 and 63-5.

<sup>5</sup> The *True Medical Theory* or *Theoria medica vera* is a collection of Stahl's medical publications preceded by several introductory parts in which Stahl set out the fundamentals of his medical theory. The edition used here is the 19th-century French translation by Théodose Blondin (Georg Ernst Stahl, *Œuvres médico-philosophiques et pratiques*, ed. Théodose Blondin (Paris, Montpellier, Strasbourg: J.-B. Baillière et fils, Pitrat et Cie, Treuttel et Wurtz, 1859-1864), 7 vol.). In the course of the controversy, Leibniz mostly attacked the introductory treatises. For a detailed although incomplete bibliography of Stahl's works, see James R. Partington, *A History of Chemistry* (London & New York: Macmillan & St. Martin's Press, 1961-1972), vol. 2, 655-62.

<sup>6</sup> Stahl, *Negotium otiosum seu Skiamaxia [...]* (Halae: Litteris Orphanotropei, 1720). A partial English translation and summary are provided in L. J. Rather, J. B. Frerichs, "The Leibniz-Stahl Controversy—I. Leibniz' Opening Objections to the *Theoria medica vera*," *Clio Medica* 3 (1968): 21-40 and Rather, Frerichs, "The Leibniz-Stahl Controversy—II. Stahl's Survey of the Principal Points of Doubt," *Clio Medica* 5 (1970): 53-67. As to the Leibnizian part of the controversy, I have made use of the edition, translation and commentaries by Sarah Carvallo (*Controverse sur la vie, l'organisme et le mixte* (Paris: Vrin, 2004)). As to Stahl's part, I refer to Blondin's French translation (vol. 6 of the *Œuvres médico-philosophiques et pratiques*).

<sup>7</sup> The relation of both Leibniz and Stahl to Cartesianism is in fact not straightforward, since in both cases, it changed with time. Although he refused to define himself entirely as a Cartesian, the young Leibniz made use of Descartes's shaped corpuscles (Letter of Leibniz to Jakob Thomasius of 30 April 1669, in Leibniz, *Philosophische Schriften*, vol. 6). Similarly, the young Stahl made use of shaped corpuscles (see Ku-Ming (Kevin) Chang, "Fermentation, Phlogiston and Matter Theory: Chemistry and Natural Philosophy in Georg Ernst Stahl's *Zymotechnia Fundamental*," *Early Science and Medicine* 7 (2002): 31-64, on 37) and he even attempted to develop visual models that may recall Descartes's (see Georg Ernst Stahl, *Fundamenta chymiae dogmaticae et experimentalis* (Norimbergae: Impensis B. Guolff. Maur. Endteri consortii et Vid. B. Iul. Arnold Engelbrechti, 1747), vol. 3, 15 and 43). Equally, both Leibniz's and Stahl's relation to *chymistry* (see William R. Newman, Lawrence M. Principe, "Alchemy vs. Chemistry: The Etymological Origins of a Historiographic Mistake," *Early Science and Medicine* 3 (1998): 32-65), is not straightforward either. Although interested by the Cartesian approach to matter, the young Leibniz also showed interest in a more hermetic "chymistry" (see Georges M. Ross, "Leibniz and the Nuremberg Alchemical Society," *Studia Leibnitiana* 6 (1974): 222-48, which shows how unclear Leibniz's affiliation with Rosicrucianism was, but also how Leibniz indeed worked on "alchemical" topics). The young Stahl appears to have believed in the realisation of the philosopher's stone and in chrysopoeia (see Kevin Chang, "Georg Ernst Stahl's Alchemical Publications: Anachronism, Reading Market, and a Scientific Lineage Redefined," in *New Narratives in Eighteenth-Century Chemistry*, ed. L. M. Principe (Dordrecht: Springer, 2007), 23-

44). In the period of the controversy, it appears that both Leibniz and Stahl were occupied with building a more personal approach to chemistry that went beyond a mere affiliation with mechanism or “alchemy” or, in the case of Leibniz, had erected a theoretical construction in line with his more general philosophical thought. For a general refutation of early 18th-century chemistry seen as affiliated with either Cartesianism or Newtonianism, see Lawrence Principe’s claim that chymistry was far less dependent on Descartes’s or Newton’s theories than presumed in the earlier historiography of chemistry (Lawrence M. Principe, “A Revolution Nobody Noticed? Changes in Early Eighteenth-Century Chymistry,” in *New Narratives in Eighteenth-Century Chemistry*, 1-22).

<sup>8</sup> “Mons. des Cartes ignoroit la chymie sans laquelle il est impossible d’avancer la physique d’usage. Ce qu’il dit des sels fait pitié à ceux qui s’y entendent, et on voit bien qu’il n’en a pas connu les différences.” (Leibniz, *Philosophischen Schriften*, vol. 4, 302). However, in other circumstances, as when he criticized Boyle’s lack of theorization, Leibniz could also celebrate Descartes’s attempts at including chemical changes in his physics (see the letter to Huygens of 9/19th February 1692 in Leibniz, *Briefwechsel mit Mathematikern*, 690). As to Stahl, see Hélène Metzger, “La philosophie de la matière chez Stahl et ses disciples,” *Isis* 8 (1926): 427-64, on 429-34.

<sup>9</sup> It is reasonable to say that Stahl’s rejection of iatro-chemistry is not always clear (see Hélène Metzger, *Newton, Stahl, Boerhaave et la doctrine chimique* (Paris: Félix Alcan, 1930), 114 ff. and Chang, “Fermentation, Phlogiston and Matter Theory,” 31-64).

<sup>10</sup> Cited in Metzger, “La philosophie de la matière,” 439.

<sup>11</sup> Here I must consider two opposite views of Stahl’s concept of matter: that of Hélène Metzger, who regards Stahl’s matter as active, and that of Kevin Chang, who tends to qualify it as inactive (in spite of the ambiguities contained in Stahl’s early theories). Compare Metzger, “La philosophie de la matière,” 439-40 with Chang, “Fermentation, Phlogiston and Matter Theory,” 57-59.

<sup>12</sup> “Le corps entier des sciences peut estre considéré comme l’océan, qui est continué partout, et sans interruption ou partage, bien que les hommes y conçoivent des parties, et leurs donnent des noms selon leur commodité.” (Gottfried Wilhelm Leibniz, *Opusculs et fragments inédits*, ed. Louis Couturat (Hildesheim: Georg Olms, 1961), 530).

<sup>13</sup> The expression “physical elements” is perhaps abusive. Indeed, Stahl usually distinguished between chemical and physical *principles*. Inasmuch as they are physical, the principles refer to juxtaposition or apposition, whereas as chemical, they connote the operations to be performed in order to obtain principles from substances or substances from principles (see Stahl, *Fundamenta chymiae dogmaticae et experimentalis ... Editio secunda ... Pars I* (Norimbergae: Impensis B. Guolff. Maur. Endteri consortii. et vid. B. Jul. Arnold. Engelbrechti, 1746), 3-4, or Stahl, *Philosophical Principles of Universal Chemistry*, 4).

<sup>14</sup> *De mixti et vivi corporis vera diversitate* (*Œuvres médico-philosophiques et pratiques*, vol. 2).

<sup>15</sup> In fact, as Leibniz remarked, Aristotle did not regard matter as infinitely divided. Here Stahl commits a error of interpretation (see Leibniz’s “Doubt XVII” and Sarah Carvallo’s note 43 in *Controverse sur la vie*, 171).

<sup>16</sup> “Le chef de l’école péripatéticienne regardait, la mixtion corporelle comme un acte ou un effet qui pénètre si intimement ce corps auquel elle est échue en partage, que toute particule corporelle, *infiniment petite*, conserve encore, au milieu de toutes les variétés organiques, *une même et toujours égale mixtion* (telle qu’on la suppose dans le corps animal), quelle que soit la grandeur de la masse, quels que soient aussi la quantité matérielle et le volume sensible qui constituent ce

même corps.” *Du mixte et du vivant*, § 2, in Stahl, *Œuvres médico-philosophiques et pratiques*, vol. 2, 256.

<sup>17</sup> See *Du mixte et du vivant*, § 4, in Stahl, *Œuvres médico-philosophiques*, vol. 2, 257. In this text, Stahl considers this possibility, but excludes it. It would be consistent to believe that a mixt could be mechanically cut into its parts, while a chemical operation would be required that separate it into its *constituent* parts.

<sup>18</sup> “Illud recte notatur, falsum esse, quod Aristotelici vulgo sibi persuadent, partem mixti, quantum vis parvam, eodem modo mixtam esse posse, ut totum. Certe sale in aquam injecto non est necesse, (imo nec intelligibile) ut partes ejus transmutentur in corpus aqueo-salinum, sed sufficit, ut dispergantur per aquam.” Leibniz, “Doubt XVII,” in *Controverse sur la vie*, 94-5.

<sup>19</sup> “Mathematicum hic a physico non differt, nisi ut abstractum animo a concreto rebus. Abstrahentes animo non aliquid falsi afferunt, sed aliquid veri seponunt [...] nec tantum divisibile est corpus in infinitum, sed et actu divisum.” Leibniz, “Doubt XVII,” in *Controverse sur la vie*, 92-3.

<sup>20</sup> “[...] ita, ut nulla sit pars materiae, in qua non multas rursus varietates notare liceret.”

<sup>21</sup> “Qui haec non animadvertit, parum assurgit ad incredibilem naturae majestatem.”

<sup>22</sup> That is why Sarah Carvallo writes: “La notion d’élément reçoit alors un sens non plus ontologique, mais à la fois expérimental et logique” (*Controverse sur la vie*, 37).

<sup>23</sup> “[...] siquidem stabilita opinione de *divisibilitate in infinitum* absoluta, haec omnes respectus sub se complectitur. Unde iterum iterumque insisto dicere et asseverare, quod proprie haec per-versa praesuppositio de *divisibilitate in Infinitum* obstiterit *Aritotelicis*, quo minus ullum realem et solidum conceptum haurire potuerint de vera tam *materiali*, quam *formali* ratione *Mixtionis*, uti vere fieri solere percipitur. Hinc enim unice quasi manifestum est [...] enatam esse confusam illam sententiam omnium Aristotelicorum, quod Mixtio ita novam *formam* constituat, quae totam *materiam* informet, et secundum totum *immutet*, ut tota *aequaliter* sit illud novum, quod nunc est.” Stahl, *Negotium otiosum*, 67.

<sup>24</sup> Some years before, Stahl showed greater scepticism with regard to the possibility of knowing the constitutive elements, writing: “Those are called Physical Principles whereof a Mixt is really composed; but they are not hitherto settled,” Stahl, *Philosophical Principles*, 4.

<sup>25</sup> “[...] sed experientia indies testatum faciat, quod illa [constitutiva materialia principia] satis prope, & affatim passim, ad manus sint.” Stahl, *Negotium otiosum*, 66.

<sup>26</sup> “Quia experientia nusquam ullum specimen suppediat, quod tale quidquam actu fiat aut existat, ut, inquam, Mixta, etiam sub immensa dissolutione diminutiva, inveniantur. Sed vel satis crassam tueantur magnitudinem, ultra quam se extenuari non ferunt, experientiae suffragio [...]” Stahl, *Negotium otiosum*, 66.

<sup>27</sup> Whereas matter is infinitely divisible, forces are nevertheless at the origin of corpuscles. Leibniz wrote: “Et je m’imagine que si cela [the presence of “metaphysical” forces in matter] estoit plus connu, ou mieux considéré, bien des personnes de piété n’auroient pas si mauvaise opinion de la Philosophie Corpusculaire,” “Extrait d’une lettre de Mr. de Leibniz, sur la question, si l’essence des corps consiste dans l’étendue,” in Leibniz, *Philosophische Schriften*, vol. 4, 466.

<sup>28</sup> However, they could disagree as to the precise effect of these operations, as in the case of fermentation (see Carvallo, *Controverse sur la vie*, 37-41).

<sup>29</sup> In this sense, there is a (relative) continuity with Paracelsus’s view on chemistry. By speaking of analysis and synthesis Stahl knew that he referred to Paracelsus’s “spagyrical philosophy” (a science of analysis and synthesis). (For the meaning of “spagyricism” see Peter Alexander, *Ideas, Qualities and Corpuscles: Locke and Boyle on the External World* (Cambridge: Cambridge

University Press, 1985), 15 and William Newman, *Atoms and Alchemy: Chymistry and the Experimental Origins of the Scientific Revolution* (Chicago: The University of Chicago Press, 2006), 45.)

<sup>30</sup> Stahl, *Philosophical Principles of Universal Chemistry*, 1.

<sup>31</sup> Between the *Fundamenta chymia* and *De mixti et vivi corporis vera diversitate*, there is no difference: a mixt is defined as composed of principles.

<sup>32</sup> The fact that there is a difference between a mixt, a compound, and an aggregate is important because it allows to conduct research into the question of the difference between homogeneous and heterogeneous bodies and because it allows to establish a practical separation between the chemical and mechanical operations that are performed (see Stahl, *Philosophical Principles*, 12, 21-6, 58-63).

<sup>33</sup> See Isabelle Stengers, "L'affinité ambiguë: le rêve newtonien de la chimie du XVIIIe siècle," in *Éléments d'histoire des sciences*, ed. Michel Serres (Paris: Bordas, 1989), 297-319 on 303, 305, 314.

<sup>34</sup> An article by David Oldroyd consisting in a reading of Peter Shaws's translation of Stahl, arrives at conclusions that emphasize the importance of having chemical elements endowed with qualities and taking operations into account (David Oldroyd, "An Examination of G. E. Stahl's *Philosophical Principles of Universal Chemistry*," *Ambix* 20 (1973): 36-52).

<sup>35</sup> The question of the integration of Leibniz's chemistry into his own general philosophy is discussed by Sarah Carvallo, *La controverse sur la vie*, 34-37.

<sup>36</sup> See the "Animadversiones in partem generalem Principiorum Cartesianorum" and "De ipsa Natura," in Leibniz, *Philosophische Schriften*, vol. 4, 388 and 514, respectively.

<sup>37</sup> For Gilles Deleuze, the metaphor of "fold" is representative of all of Leibniz's thought (Gilles Deleuze, *Le pli, Leibniz et le baroque* (Paris: Les éditions de minuit, 1988)). Thus, when he had to explain the "division of the continuous," Leibniz used the image of a piece of fabric where there were folds in which one could always find smaller folds ("[...] ut chartae vel tunicae in plicae, itaque licet plicae numero infinito, aliae aliis minores fiant [...]") Leibniz, *Opusculum et fragmenta inédits*, 615).

<sup>38</sup> See also Leibniz, *Nouveaux essais sur l'entendement humain*, book 2, ch. 23, § 23 in *Philosophische Schriften*, vol. 5.

<sup>39</sup> "A une certaine vitesse du bateau, l'onde devient aussi dure qu'un mur de marbre." Deleuze, *Le pli*, 8. I have not been able to find a similar statement in Leibniz himself and this is Deleuze's interpretation of Leibniz's physics, which I believe is entirely right.

<sup>40</sup> "Praedicatorum est poiographia, ut experiamur in quibus subjectis reperiantur qualitates. [Et huc chymia.]" Leibniz, *Opusculum et fragmenta inédits*, 526.

<sup>41</sup> "Equidem si Physicam illam appellemus generalem, quae communia tribus regnis tractat, perfectio Chymiae erit practica pars Physicae generalis, et uti Medicina ad hominem, aut agricultura ad plantas, ita sese Chymia ad elementa et corpora, vel similia vel rudius mista, habebit [...]" Letter to Stisser, 25 May 1700 in Leibniz, *Opera omnia*, vol. 2, part 2, 128.

<sup>42</sup> Paraphrase of "Primus Democritus quod constet, cum Leucippo purgare Physicam conatus est a qualitatibus ἀρρητοις, dixitque ποιότητος νομῶν εἶναι, qualitates esse ex opinione, quasi in speciem, non veras res." "Antibarbarus physicus pro Philosophia Reali contra renovationes qualitatum scholasticarum et intelligentiarum chimaericarum," in Leibniz, *Philosophische Schriften*, vol. 7, 343.

<sup>43</sup> See for example Leibniz, *Nouveaux essais*, book 2, ch. 29, § 4.

<sup>44</sup> This is Pierre Laszlo's expression, in *La parole des choses* (Paris: Hermann, 1993).

<sup>45</sup> This view is sketched in a text of 1702 and in “De ipsa Natura,” written in 1698 (see Leibniz, *Philosophische Schriften*, vol. 4, 393-400 and 504-16).

<sup>46</sup> The distinction between primitive and derivative forces corresponds to the distinction between spiritual and material worlds: a substance of a kind cannot modify a substance of the other kind. Therefore, in the material world, there are only derivative forces, whereas the spiritual world contains beings (monads) possessing primitive forces (see *Philosophische Schriften*, vol. 2, 251, 262; vol. 3, 457; vol. 6, 150). Leibniz’s materials are not active, since movement is always caused by surrounding materials, but Leibniz’s materials, which always include inner materials, can be *regarded* as active (though not intrinsically but derivatively). For a detailed discussion on Leibniz’s concept of forces, consult Daniel Garber, “Leibniz: Physics and Philosophy,” in *The Cambridge Companion to Leibniz*, ed. Nicholas Jolley (Cambridge: Cambridge University Press, 1995).

<sup>47</sup> “Imo corpora omnia ad Chymiam pertinent, quando secundum operationes physicas, insensibili processu constantes, non ut structurae, sed ut massae tractantur.” Leibniz, “Doubt XII,” in *Controverse sur la vie*, 90.

<sup>48</sup> By contrast, I am not sure that Leibniz really took this type of difference into account. In his reply to Stahl’s answer, and notably from an example that he adduced, I believe that he did not distinguish between the model of mixtion and that of aggregation (see Leibniz, “Reply XVII,” § 3 in *Controverse sur la vie*, 118. For the distinction between mixtion and aggregation and whose return to Aristotle with the differentiation between the mixtion of sugar into water and the juxtaposition of grains, see also Pierre Duhem, *Le mixte et la combinaison chimique: essai sur l’évolution d’une idée* (Paris: Fayard, 1985), part 1, ch. 1).

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## Disciplinary Identity And The Chemical Revolution

*John McEvoy\**

### Introduction

Historians of science have long been interested in identifying the distinct and separate disciplines of science. Historians of the Chemical Revolution with otherwise radically different historiographical agendas also share this interest. This paper surveys a handful of these discipline-based histories, and calls attention to problems raised for the historiography of disciplinarity by the specificity, originality, and temporality of Joseph Priestley's scientific practices.

Comte's "positive philosophy" is an important source of interest in the disciplinary identity and development of science.<sup>1</sup> Comte called upon historians of science to offset "the pernicious influence" of specialisation –inherent in the disciplinary division of labour essential to the progress of science– by attending to "the relations and concatenations of the sciences." He presented this proposal as a perfection of the division of labour, in which the dialectic between the specialised practitioners of the different scientific disciplines and the newly emergent historians of science, or "positivist philosophers," specialising in the study of "general scientific traits," would facilitate the indefinite progress of science, while preserving its clearly demarcated identity. Comte linked the unity and progress of science to the disciplinary coordination of individual investigators serving, without necessarily comprehending or being concerned with the epistemological and sociological interests of the whole.

Interest in the disciplinary identity and coordination of eighteenth-century chemistry outlasted the positivist hegemony that formed it; it is clearly detectable among post-positivist and post-modernist historians of chemistry who otherwise distanced themselves from the synthesising and reforming spirit of positivism. Whereas positivist historians viewed the Chemical Revolution as the moment when chemistry made the transition from a nonscientific to a scientific discipline,

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post-positivists, like Evan Melhado, Carleton Perrin, and Robert Schofield, related it to the shifting boundaries of the pre-existing scientific disciplines of physics and chemistry. While sociologically minded scholars such as John Christie and Jan Golinski identified eighteenth-century chemistry as a “didactic discipline”, other scholars, like Lissa Roberts, Jonathan Simon, Mi Gyung Kim, and Ursula Klein, called for the “history of chemistry’s disciplinary journey from its self-defined status as art to its recognised status as science.”<sup>2</sup> More generally, sociologists of knowledge like Steve Shapin emphasised the important role of the construction of disciplines and their boundaries in the development of science. Indeed the sociological ascendancy in late –twentieth-century history of science ushered in “a remarkable expansion of interests in scientific disciplines, their origins, fusions, fissions, and extinctions”.<sup>3</sup>

### **Historiographies of the Chemical Revolution**

A brief survey of positivist and post-positivist interpretations of the Chemical Revolution highlights both the interpretive flexibility and limitations of the historiography of disciplinarity. The demarcationist form of this historiography characterised the positivist image of the Chemical Revolution as the “postponed Scientific Revolution in chemistry”. On this interpretive model, scientists merely had to deploy the unique scientific method developed during the Scientific Revolution of the seventeenth century to ensure cognitive progress in their respective disciplines. Lavoisier did this in a series of crucial quantitative experiments on combustion and the calcinations of metals, which established the central role of oxygen in the workings of nature. Phlogistians like Joseph Priestley, on the other hand, did not appreciate chemistry’s break with its non-scientific past because they failed to recognise and abide by the disciplinary and methodological strictures of the new chemistry.<sup>4</sup> On this interpretation, Lavoisier’s modernist sense of the autonomy of chemistry contrasts vividly with Priestley’s pre-modern vision, of a unified cosmos grounded in God’s providential presence.<sup>5</sup>

Post-positivist historians of science in the 1960s and 1970s challenged this interpretation of the Chemical Revolution.<sup>6</sup> They identified science with theory and the history of science with the succession or conjunction of different and distinct, if not incommensurable, paradigms or doctrines. The historiography of disciplinarity accordingly shifted its focus from the domain of methodological rules and experimental strictures to the realm of doctrinal conformity and theoretical commitment. It focused not on the monolithic demarcation between scientific and nonscientific disciplines, but on the complicated intrascientific relations between



the disciplines and doctrines of physics and chemistry, thereby bringing to the fore the hallowed problem of the autonomy of chemistry and the totalising ambitions of physics.

Post-positivist scholars, like T. S. Kuhn, Arnold Thackray, and Robert Schofield, upheld the autonomy of eighteenth-century chemistry by rejecting the positivist assimilation of the Chemical Revolution to the Scientific Revolution, emphasising instead the work of Lavoisier's Stahlian predecessors in the eighteenth-century chemical community. Thus, Thackray and Schofield emphasised the "profound failure of the Newtonian program" in eighteenth-century chemistry and argued that Lavoisier did not free chemistry from the influence of George Ernst Stahl, so much as rationalise Stahl's program in a way that resisted the premature and unhelpful solicitations of corpuscular physics.<sup>7</sup>

Other post-positivist scholars replaced this pleasing myth of the liberation of chemistry with a more nuanced sense of the disciplinary horse-trading that characterised physics and chemistry in the eighteenth century.<sup>8</sup> According to Perrin, while Lavoisier pursued a program of methodological reform, in which he used physical instruments and measurements to solve chemical problems, the revolution he envisioned in the fall of 1772 "was a conceptual and theoretical one", based on the introduction of the idea of the gaseous state into the pre-existing science of chemistry promulgated by Guillaume Francois Rouelle. While Perrin defended the orthodox view that Lavoisier "accomplished a revolution *in* chemistry", Arthur Donovan claimed that Lavoisier deployed the methodology of Jean-Antoine Nollet's experimental physics to engineer a "revolution *into* chemistry." Melhado, on the other hand, identified Lavoisier as a follower of Herman Boerhaave, who approached the chemical fixation and liberation of air as "aggregative phenomena", produced by "free [rarefying] fire" or caloric rather than by "fixed [attractive] fire" or phlogiston. According to Melhado, "the Chemical Revolution was generated externally by physics", which transformed an already existing scientific discipline "and then withdrew from it".

Donovan, Melhado, and Perrin never resolved their interpretive differences, thanks in part to Perrin's untimely death, but also to historiographical disagreements that rendered an effective resolution unlikely. While Perrin remained attached to the positivist idea that scientific progress involves the replacement of older, less adequate statements about the world with newer, more adequate statements, Donovan and Melhado stressed the globalist view that scientific revolutions involve changes in the methodological and ontological assumptions that guide the formation of theoretical statements and principles. On this view, Lavoisier achieved a revolution in chemistry only because "his successful refuta-

tion of the phlogiston theory also involved transforming chemistry” by incorporating into it methodological or ontological principles derived from physics. According to Donovan, a just appreciation of the Chemical Revolution required recognition of the historical mutability not only of the facts, concepts, theories, methods, and aims of science, but also of its disciplinary structure.

### **The Doctrine of Airs**

Donovan’s historicist admonitions sensitise us to two possible problems with the application of the historiography of disciplinarity to the interpretation of Priestley’s science. The first problem concerns the specificity of Priestley’s science and the capacity of familiar (modern) disciplinary boundaries to accommodate his “doctrine of airs”. The other problem, which will be dealt with in the next section, draws attention to possible ‘tensions’ between the synchronic structure of disciplines and the diachronic course of Priestley’s natural philosophy.

An overly narrow focus on positivism’s familiar interest in the deductive unity of the disciplines of physics and chemistry leads to a retrospective distortion of Priestley’s science. This is evident in Schofield’s physicalist construal of Priestley’s opposition to Lavoisier’s chemistry, as well as in those interpretations of the Chemical Revolution that identify it as a Second Scientific Revolution, which occurred between 1780 and 1850 and involved conceptual and institutional transformations that separated early modern science, or natural philosophy, from the autonomous disciplines of modern science.<sup>9</sup>

Fortunately, more recent scholars, like Simon and Kim, are sensitive to this problem, focusing on the relation between chemistry and medicine, rather than chemistry and physics, in the eighteenth century. More salient for the current discussion is the suggestion made by the late Larry Holmes that the Chemical Revolution was a revolution in “pneumatic chemistry”, understood not in its modern guise as a subdivision of general chemistry, but in its eighteenth-century grandeur as an interdisciplinary activity that encompassed physics, chemistry, and medicine.<sup>10</sup>

Taking his cue from Henry Guerlac’s suggestion that the Chemical Revolution involved the synthesis of continental analytical chemistry and the results of British pneumatic chemistry, Holmes argued that whereas the continental tradition represented the activity of successive generations with a distinct disciplinary identity, pneumatic chemistry was pursued by “people who were not identified primarily as chemists” and whose results were “not necessarily seen by contem-

poraries as more particularly belonging to chemistry rather than physics, or medicine". Thus, Stephen Hales, the founder of pneumatic chemistry, was a Newtonian natural philosopher, with no special interest or training in chemistry; Joseph Black was a "philosophical chemist," concerned with the role of chemistry in medical education; David McBride and William Brownrigg were practicing physicians; Henry Cavendish was a physicist; and Priestley approached the doctrine of airs from the perspective of a comprehensive philosophy of man and nature. It is also important to note that Priestley was indiscriminately interested in the physical, chemical, and medical properties of the dozen or so airs he prepared and isolated. Nor was his abiding interest in the "purity" of the atmosphere a chemical one. Rather, Priestley's search for the "provisions" in nature, such as vegetation and the agitation of seas and lakes, to offset the "vitiation" of the atmosphere, caused by respiration, combustion, and putrefaction, expressed medical and social concerns, as well as a broader, theistic view of a benevolent nature attuned to the preservation of animal life and human happiness.<sup>11</sup>

When Priestley developed his phlogistic explanations of the compositions of the airs and the role of respiration and vegetation in the balanced economy of nature, he loosened phlogiston from its traditional Stahlian identification with the principle of inflammability. He also developed a new "doctrine of airs", which as Ferdinando Abbri has noted was well received by physicists, chemists, and physicians all over Europe.<sup>12</sup> According to Holmes, it was this new rival program, stemming from Hales and culminating in Priestley, and not the traditional chemical doctrine of Stahl, that Lavoisier had in mind, and with which he aligned himself, when he referred to "a revolution in physics and chemistry". Noting that the "new chemistry" encountered Stahl, and not Priestley, in Paris and Germany, Holmes called for a more decentered view of "the chemical revolution as a set of different kinds of event within different contexts".

### **Disciplines and Discoveries**

Holme's analysis supports the "interactive" model of the Chemical Revolution championed by Ferdinando Abbri and Bernadette Bensaude-Vincent.<sup>13</sup> This model interprets the spread of the "new chemistry" not in terms of the gradual subsumption of the peripheries of the scientific culture under the Parisian centre, but in terms of a dynamic interaction between relatively autonomous, local cultures of chemistry, each with its own disciplinary identity. But Holmes did not take his contextualising interest far enough. His view of the Chemical Revolution remained centered on Lavoisier, and although he rescued "pneumatic chemistry"

from its retrospective identification as a subdiscipline of general chemistry, he still presented it in terms of the disciplinary configurations of physics, chemistry, and medicine, which carry their own retrospective baggage. It is only by linking the scientific dimension of Priestley's self-proclaimed identity as an "aerial philosopher" to its broader philosophical and social context that we can fully appreciate his idiosyncratic notion of the disciplinary nature of the "doctrine of airs" and its relation to general chemistry.<sup>14</sup>

When Priestley identified himself as an "aerial philosopher", he registered not only his ignorance of traditional chemical theory and practice; he also elevated the disciplinary status of the chemistry of gases above that of general chemistry. Work on the mineral acids and their gaseous products led Priestley to conclude that it was only by studying them in the "aerial state", bereft of the moisture united to them in the liquid state, that their real "nature and affinities" could be determined with any certainty. Impressed with the many other discoveries and myriad implications of gas chemistry, Priestley ventured to entertain the possibility that, eventually, chemistry would be conducted almost entirely in the "aerial state":

"The reason of my great expectations from this mode of experimenting is simply this, that, by exhibiting substances *in the form of air*, we have an opportunity of examining them in a less compound state, and we are advanced one step nearer to their primitive elements. It will be a great satisfaction to me, after that part which I have taken in this business, to be *aspectator* of its future progress, when I see the works in so many, and so good hands, and everything, in so rapid and so promising a way".<sup>15</sup>

While it is true that Priestley approached the study of gases with a well-defined repertoire of experimental techniques and instrumental practices, his sense of the progress and future hegemony of pneumatic chemistry does not rest on a clearly articulated vision of its disciplinary structure and domain of application. Its open-ended, sublime character signifies his view of the role of natural philosophy in the endless progress and perfectibility of human nature through the comprehension of an infinite world, bristling with "novelty" and grounded in God's benevolent fecundity.

These considerations lead us to question the role of the historiography of disciplinarity in the interpretation of Priestley's science. The valorisation of communal unity and conformity associated with this historiography –whether in the form of positivism's vision of the unity of science, Kuhn's model of normal science, or Foucault's disciplinary structures of power– obscures the dimension of endless

novelty, or “discovery”, crucial to Priestley’s scientific practice. An adequate historiography of the Chemical Revolution must do justice to the well-worn, but perspicuous, image of Priestley as a “comet in the system”, offsetting in the manner of a Newtonian active principle, the stifling tendencies of disciplinary unity and conformity with a plethora of new discoveries and a mode of theory and practice based on a synoptic sense of man’s unfolding comprehension of nature.<sup>16</sup>

Three lines of response to the Priestley problem are open to the historiography of disciplinarity. It can simply exclude Priestley’s science from its domain, accommodate it in an *ad hoc* manner, or develop a more encompassing, dynamic notion of practice and disciplinarity. The first response, located in the works of Jan Golinski and Simon Schaffer, treats disciplinary structures as inherently communal and practical and locates the age of disciplinarity in the second scientific revolution of the early nineteenth century.<sup>17</sup> On this account, Priestley’s discoveries contributed to, but did not participate in, the disciplinary identity of modern chemistry. This response is reminiscent of earlier historiographies of the Chemical Revolution, which placed Priestley on the ‘wrong’ side of the dividing line between science and nonscience, mature and immature science, though the division is now presented as an historical one between the “development of specialisation and professionalism” and the gentlemanly natural philosophy of the Enlightenment that preceded it. Still, it seems to be a matter of old (normative) wine in new (naturalistic) bottles. However, this line of response to the Priestley problem can be given a more interesting and historically informative construal, which throws light on important differences between chemistry in France and Britain in the eighteenth century. It should be remembered that Priestley rejected the imposition of disciplinary uniformity involved in the spread of the French system of chemistry in the name of a liberal individualism that refused to bend the knee to any arbitrary “authority”, civil or philosophical. Besides increasing our appreciation of the complexity of the Chemical Revolution, this aspect of the dialectic between Priestley and the French chemists suggests that the historiography of disciplinarity, especially in its Foucauldian form, is more likely to be applicable to the science of those countries, such as France, German, and Austria, where the “ideal of bureaucracy and ‘police’”, was actualised more than in the “much less bureaucratised” Britain.<sup>18</sup>

The second option, also floated by Golinski, emphasised the “capacity of individuals for autonomous self-expression”. Resisting the “powerful sway of disciplinarity,” some individuals “creatively” manipulate available resources to forge their own autonomous identity.<sup>19</sup> This response appeals to common sense, but only by sullyng historical understanding with the introduction of the unhistorical, obfus-

cating notion of personal creativity. The spectre of whiggism also haunts this response, imposing on eighteenth-century natural philosophy the nineteenth-century distinction between “genius” and “discipline”.<sup>20</sup>

The third option treats disciplines not as rigid structures dominating a unified and immobile scientific field, but as domains of development articulated within a complex and dynamic field of inquiry encompassing science and non-science. This perspective comports well with our growing sense that the Chemical Revolution was not a monotonic moment of disciplinary purification or isolation, but a complex, multidimensional episode, a veritable “mangle of [disciplinary] practices”. It also recognises that the “mangle” of history can be liberating and constraining, serving the interests and extending the power of some, while restricting and constraining the activities of others: it reflects the Janus face of the Enlightenment.<sup>21</sup>

Above all, this perspective emphasises the need to offset with historicist vigilance the retrospective tendency of the historiography of disciplinarity to speak of past scientific disciplines as “*if they were those obtaining or normative in present-day science*”.<sup>22</sup> Clearly, they were not.

## Notes

<sup>1</sup> See Auguste Comte, *Introduction to Positive Philosophy*, ed. and trans Frederick Ferré (Cambridge, MA: Hackett, 1988), 1-33.

<sup>2</sup> Lissa Roberts, “Setting the Table: The Disciplinary Development of Eighteenth-Century Chemistry as Read through the Changing Structure of Its Tables,” in *The Literary Structure of Scientific Argument: Historical Studies*, ed. P. Dear (Philadelphia, PA: University of Pennsylvania Press, 1991), 99-132, on p. 99.

<sup>3</sup> N. Jardine, “A Dip into the Future,” *Studies in History and Philosophy of Science* 20 (1989): 15-18, on p. 17.

<sup>4</sup> See John G. McEvoy, “Positivism, Whiggism, and the Chemical Revolution: A Study in the Historiography of Science,” *History of Science* 31 (1997): 1-33.

<sup>5</sup> See John G. McEvoy, “Modernism, Postmodernism, and the Historiography of Science,” *Historical Studies in the Physical and Biological Sciences* 37 (2007): 383-408, on pp. 387-393.

<sup>6</sup> See John G. McEvoy, “Postpositivist Interpretations of the Chemical Revolution,” *Canadian Journal of History* 36 (2001): 453-469.

<sup>7</sup> See T. S. Kuhn, “Robert Boyle and Structural Chemistry in the Eighteenth Century,” *Isis* 43 (1952): 1-23; R. E. Schofield, *Mechanism and Materialism* (Princeton, NJ: Princeton University Press, 1970); A. Thackray, *Atoms and Powers* (Cambridge, MA: Harvard University Press, 1970).

<sup>8</sup> See the discussion between Perrin, Donovan, and Melhado in *Isis* 81 (1990): 259-276.

<sup>9</sup> See R. E. Schofield, “Joseph Priestley: Natural Philosopher,” *Ambix* 14 (1967): 1-15.

<sup>10</sup> F. L. Holmes, “The ‘Revolution in Physics and Chemistry’: Overthrow of a Reigning Paradigm or Competition between Contemporary Research Programs,” *Isis* 91 (2001): 735-753.

- <sup>11</sup> See John G. McEvoy, "Joseph Priestley, 'Aerial Philosopher': Metaphysics and Methodology in Priestley's Chemical Thought, 1772-1781, Part 1," *Ambix* 25 (1978): 1-55; "Part 2," *ibid*, 93-116; "Part 3," *ibid*, 153-175; "Part 4," *Ambix* 26 (1979): 16-38.
- <sup>12</sup> See F. Abbri, "The Chemical Revolution: A Critical Assessment," *Nuncius* 4 (1989): 303-315.
- <sup>13</sup> See B. Bensaude-Vincent and F. Abbri, eds. *Lavoisier in European Context: Negotiating a New Language for Chemistry* (Canton, MA: Science History Publications, 1995).
- <sup>14</sup> See, e.g., McEvoy, "Joseph Priestley: 'Aerial Philosopher'."
- <sup>15</sup> Joseph Priestley, *Experiments and Observations on Different Kinds of Air*, 3 vols (London: J. Johnson, 1774-1777), vol. 3, ix.
- <sup>16</sup> See John R. Clarke, *Joseph Priestley: A Comet in the System* (San Diego: Torch Publications, 1990).
- <sup>17</sup> Simon Schaffer, "Scientific Discoveries and the End of Natural Philosophy," *Social Studies of Science* 16 (1986): 387-420; Jan Golinski, *Making Natural Knowledge: Constructivism and the History of Science* (Cambridge: Cambridge University Press, 1998), 66-78.
- <sup>18</sup> Jay Goldstein, "Foucault Among the Sociologists: The 'Disciplines' and the History of the Professions," *History and Theory* 23 (1984): 170-192. See also John G. McEvoy, "Priestley Responds to Lavoisier's Nomenclature," in *Lavoisier in European Context: Negotiating a New Language for Chemistry*, eds. B. Bensaude-Vincent and F. Abbri (Canton, MA: Science History Publications, 1995). pp. 123-142, on pp. 132-133.
- <sup>19</sup> Golinski, *Making*, p. 78.
- <sup>20</sup> Schaffer, "Scientific Discoveries," pp. 406-413.
- <sup>21</sup> For 'disciplines' as liberating rather than constraining see Robert E. Kohler, "The Constructivists' Tool Kit: Essay Review of Jan Golinski, *Making Natural Knowledge*," *Isis* 90 (1999): 329-331.
- <sup>22</sup> Steven Shapin, "Discipline and Bounding: The History and Sociology of Science as Seen through the Externalism-Internalism Debate,"



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# From Science to Techno-science: The Formation of the Disciplinary Identity of Chemistry in the 19th Century

*Joachim Schummer\**

## Abstract

The 19th century is widely considered the most important period of modern discipline formation. The establishment of research universities and the upgrading of the philosophical faculties – on par with the faculties of theology, medicine, and law – provided an important social context in which the identities of the main scientific disciplines were shaped still for much of the 20th-century. In this paper I analyze four factors that contributed to the formation of the disciplinary identity of chemistry, focused on synthetic organic chemistry and oriented toward what we today would call technoscience. The first one is chemistry's disciplinary academic origin in the faculties of medicine that continued to be influential during the 19th century, although chemistry departments moved to or were newly established in the philosophical faculties. I argue that the traditional link to medicine enabled the rapid growth of what soon came to be known as organic chemistry. The second factor is the parallel formation of other disciplines from which chemistry needed to distinguish itself and among which I will particularly look at the formation of modern physics. I argue that the discipline of modern physics emerged quite late both by assuming specific research fields that previously belonged to other disciplines, including mathematics and chemistry, and by assuming the role of being fundamental to all the other sciences, both of which had a profound impact on the research questions at the core of the disciplinary identity of chemistry. As a third factor I consider internal developments of chemistry from the late 18th to the mid-19th century. Against the background of the classical methodological pair of analysis and synthesis, I argue that the early emphasis on analysis turned into a strong emphasis of synthesis. All three factors shaped the disciplinary identity of chemistry in such a way that synthetic organic chemistry became

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the lead area in the second half of the 19th-century, which was eventually reinforced by a forth factor, the emergence of the dye-stuff and pharmaceutical industries. I conclude with some remarks on how this still influential disciplinary profile has contributed to the public image of chemistry, its scientific status, and remoteness from philosophy.

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## Boundaries of Chemistry: Interest and Identity in Early Twentieth Century

*Gabor Pallo*\*

The relationship between chemistry and its neighbors can be studied from a science-policy point of view. From this latter aspect, the demarcation problem seems differently when looked at from the philosophy of science approach. Both policy and philosophy analyse the differences between science and non-science but their conclusions diverge. Similarly, chemistry's relationship with other sciences can be considered as a demarcation problem between chemistry and non-chemistry. This problem can also be analysed both from a philosophical and a policy point of view. The difference between these two approaches is demonstrated by two case studies: the discovery of hafnium and the first national congress of Hungarian chemists.

### **The philosophical approach**

The philosophical approach raises the problem of defining chemistry as a field having its own identity. Philosophers provided various criteria for defining the identity of science aiming at demarcating science from non-science. Chemistry can be assumed to be defined by some criteria demarcating it from non-chemistry, such as physics, biology and other areas. Logical positivists applied various criteria like verification or testability. Thomas Kuhn relied on paradigms, Imre Lakatos on research programs, while Robert Merton on some ethical norms for defining science. All these approaches can be considered essentialist because they all tacitly assumed that chemistry can be defined by some criteria that demarcate it from non-chemistry such as physics, biology and other areas. These approaches can be considered essentialist because they tacitly presuppose that there is something in the real world that distinguishes science from all other things, something that belongs to science, which is constant and does not change over time. This unchanging component is the essence of science. Since the essences of various representations sharply differ from each other, when defining

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the essence of science, we automatically demarcate science from all other representations.

Ironically, Karl Popper while attacking essentialism also took an essentialist stand himself. His hypothetico-deductive system was just as essentialist as the theories (Marxism and Freudism) that he opposed. Neither Popper, nor many other philosophers of science would be happy to be called essentialist because they fought against all kinds of metaphysics. Indeed, essentialism originates in Plato and Aristotle's philosophy, in their metaphysics or metaphysical presumptions but it became an important doctrine in the Thomist philosophy in the Middle Ages.

### **Essence of chemistry: the hafnium story**

When thinking of chemistry as a distinct, unchanging form of scientific knowledge, which differs in principle from other forms, we are essentialists. We follow the logic of those who consider science as a distinct, constant form of representation. The debate over reductionism is connected to this view. Mary Jo Nye has summarised and analysed in detail the difficult relations between chemistry and physics in a historical perspective.<sup>1</sup> Without considering the debate over the late 18<sup>th</sup> century chemistry about the role of the theory of heat or pneumatical physics in the chemical theory of combustion let us turn to the 20<sup>th</sup> century. In some of their papers, Anna Simoes and Kostas Gavroglu discussed the problem that quantum chemistry raised the issue whether chemistry had been, in principle, but not in practice, reduced to physics right after Heitler and London published their seminal article in 1927.<sup>2</sup>

Karl Popper, however, came to the same conclusion even before the emergence of quantum chemistry in 1927. He wrote in his *Logic of Scientific Discovery* that “all attempts to find it (the chemical element with atomic number 72) were in vain until Bohr succeeded in predicting several of its properties by deducing them from his theory.”<sup>3</sup> Popper also wrote that the discovery of hafnium “struck us then as the great moment when chemistry had been reduced to atomic theory.”<sup>4</sup>

The discovery of hafnium was related to a collision between inorganic chemistry and Bohr's old quantum theory concerning the unknown chemical element number 72.<sup>5</sup> In 1911, George Urbain, a French expert of rare earth elements, believed he had found element 72 in a mineral called yttria by his method based on a ‘separating element’ that crystallised with rare earths. Urbain called the element, celtium. He thought that celtium was another rare earth to be placed in the row

below the periodic table. Niels Bohr, on the other hand, concluded that the unknown element 72 should be a transition metal to be placed on the table, below zirconium. He based his view on the so-called old quantum theory, mainly his atomic model created in 1913 and articulated in the early 1920s. In 1922, relying on Bohr's theory, George Hevesy, a Hungarian chemist of Bohr's institute in Copenhagen with a young Dutch physicist, Dirk Coster found the element 72 in a Norwegian zirconium mineral. The element was subsequently called hafnium. Chemical investigations proved that hafnium was a transition metal, as Bohr predicted. Because of the exciting subsequent priority debate between Hevesy and Urbain, this story then became well known. Hevesy's victory, which brought him nominations to the chemistry Nobel Prize, can be attributed to Bohr's model. In his memoirs, Hevesy recognized the decisive role of Bohr's theory in his celebrated discovery.<sup>6</sup>

Nevertheless, was Popper right? Can the important contribution of the old quantum theory to the discovery of hafnium be considered as a reduction of chemistry to physics? Philosopher, Eric Scerri denied this in several publications. He argued against the Popperian view in two points. First, some chemists, including the Danish Thomsen, guessed, on purely chemical evidences, that element number 72 should be a zirconium homologue. Second, Bohr's model was not a physical theory deduced from first principles of physics; rather it was a strange mixture of inductive generalisations largely based on chemical experimental data.<sup>7</sup>

Whilst partly agreeing with Scerri's opinion, it is considered that both Popper's and Scerri's argumentation was essentialist. They, with other discussants of reductionism, assume that we have chemistry here, physics there, with different essential characteristics, and it can be decided whether a particular statement is connected to one or the other's essence. It is a metaphysical problem whether chemistry has its own essence or not, whether physics and chemistry are representations of two separate parts of nature or chemistry represents just a part of nature investigated by physics.

### **The policy approach: boundary work**

Since the 1990s, a new approach to the demarcation problem has been developed inside studies of science and technology. Based on a social constructivist view, some authors looked at the problem of demarcation as a battlefield in a fight for more resources, influence and authority. Thomas Gieryn considered science as being a space on the map of the cognitive activities. In this space, science has an

unquestioned cognitive authority. The map is divided between various activities, which have their own lands with boundaries. The lands are competing for enlarging or defending their boundaries. Science, as one of these lands, has vested interest to defend its boundaries against the attacks of non-science and to expand its boundaries by all legitimate means. This defending and expanding activity is called boundary work.<sup>8</sup>

The boundaries of science are not fixed. They are contingent, flexible, context-dependent and negotiable. This is a non-essentialist view, as it does not assume that science has an unchanging, fundamental difference from any other social institutions representing some cognitive authority, such as arts, politics, and business. Science becomes one of the many parties competing for cognitive authority, resources and social influence. In this competition, the boundaries of science are not fixed. Whether astrology or psychoanalysis is in a better position (to use a Popperian example) compared with astronomy or behaviorism depends on the actual state of the competition between the fields. Science has vested interest to defend its boundaries against the attacks of the other parties and it attempts to expand its boundaries by all legitimate means of the competition. In this approach, ideological debates, such as the debate over reductionism, can be considered normal processes of the boundary work.

This approach throws different light on the identity of chemistry. In the land of sciences, chemistry claims to have a part of its own. It has boundaries with neighboring fields, such as physics. Chemistry works at its boundaries that are attacked from various directions such as physics, environmentalist movements, agricultural interests, traditional medicine, and many others. Inside its boundaries, chemistry claims to have cognitive authority defined by the population of chemists. So, who are the chemists?

### **The first national congress of Hungarian chemists**

From the problem of what chemistry is, now shift to the problem of who lives in the land of chemistry. This constituted a major practical issue in the chemical community in Hungary at the beginning of the 20<sup>th</sup> century. The problem was formulated in a letter sent to a journal of pharmacists in 1905: "Would you know, dear Mr. editor, to whom you are talking if he introduced himself as X. Y., chemist? You would not, would you? Because this man could well be an engineer graduated from a technological institute, or a chemist trained by an industrial school but he could also be a philosopher who learned some chemistry at the fac-

ulty of humanities, or a pharmacist, perhaps an assistant at a drug store, but he could also be just a dry cleaner. Depending on their taste, they all can call themselves chemists.”<sup>9</sup>

The issue seemed so important at the time that a chemical engineer, editor of a journal of chemistry, Gyula Halmi, decided to discuss it at a conference. He started to organise the first national congress of Hungarian chemists that was finally held in 1910. The task of the congress was to discuss the common interests of chemists coming from diverse areas of the field.

During the five years between publishing the above letter to the editor and the opening of the congress, a number of articles and letters appeared in the Hungarian chemistry journals on the identity of chemists. The pharmacists were particularly active. According to an article, pharmacists and those chemists, who graduated at the faculty of humanities should be expelled from industry because their theoretical and practical training were of a low level.<sup>10</sup> The reply was published in a journal of pharmacists. It argued that the chemistry training of the pharmacists was good enough for occupying jobs in drug stores, and for enabling them to develop into very good chemists. The argument was supported by a list of some leading chemists of the country who were originally trained as pharmacists. In 1907, an author explained that the chemistry training available in Hungary included the pharmaceutical schools, faculty of humanities, Technical University, and industrial schools. In industry, only those people should be employed, he said, who are trained in industrial schools or at the Technical University. He concluded that the good chemists were the chemical engineers.<sup>11</sup> Someone else reported that a war was breaking out between the chemical engineers and the industrial school graduates. Earlier, factory positions were despised as non-gentleman occupations for someone having a university degree because the working hours were long and the industrial plants were dirty. Now, he said, unemployment had changed this judgement.

The debate went on in the same tone for years. A group of chemists wanted to exclude some other groups from the field of chemistry. In other words, they wanted to defend their territory by narrowing and strengthening the boundaries of chemistry. The boundary work intending to tighten the boundary had a nationalist tendency too. Hungarian chemists wanted to prohibit the employment of immigrant chemists. They wanted to exclude all competitors from the territory of ‘Hungarian chemists’.

The underlying reason for this debate was the scarcity of jobs for chemists. An article explained this motive by referring to the growing number of graduates,



which reached 28 in 1907, in contrast with the earlier 2-3 persons per year. The author stated that Hungary did not need further chemists. Therefore, their training at the Technical University should be stopped.<sup>12</sup>

The main goal of the first national congress of Hungarian chemists was to discuss the job situation, the measures to be taken for defending the common interests of the chemists, and to promote the development of the field. Because of the unclear boundaries, Halmi, the organizer of the congress, decided to make a survey about the number of Hungarian chemists. While doing this, he wanted to register all people who could somehow be considered as a chemist. According to his not very precise estimation, 540 persons were employed as chemists. They worked in different fields (Table 1), mainly in civil service (Table 2) and industry (Table 3); there were many more chemists in civil service than in industry. Remarkably, pharmaceutical industry was not listed. Public education proved to be the largest employer.

**Table 1**

**Chemists' employment in various sectors**

<i>Sectors</i>	<i>Number of persons</i>
Civil service (state or city jobs)	277
Industry	225
Private practice	26
Unknown occupation	110
Working in foreign country	32
<b>Total</b>	<b>670</b>

**Table 2**

**Employment in Civil Service**

<i>Employment</i>	<i>Number of persons</i>
Teacher (high school, industrial school)	109
Experimental stations	75
College (Hoch Schule) adjunct, assistant	32
City quality control institutes, customs officer	9
<b>Total</b>	<b>215</b>

**Table 3**  
**Employment in Industry**

<i>Employment</i>	<i>Number of persons</i>
Sugar industry	37
Iron industry	26
Sulfuric acid, hydrochloric acid, chemical fertilizer	25
Distillation, yeast	23
Textile, dye industry	13
<b>Total</b>	<b>124</b>

In his survey, Halmi did not differentiate between the levels of training or between the schools. He apparently wanted to produce the largest numbers to prove the social relevance of the debate on the chemists' situation. Therefore, at the end of his article, he remarked that besides the chemists he listed, there would be some colleagues working in unknown jobs and other people with related expertise in neighboring fields such as pharmacists, physicians and teachers. With them, he estimated that the 230 participants of the congress represented around thousand people related to chemical profession. At the time this number seemed large. Halmi, however, seemed to forget that the representatives of the related fields did not attend the congress, although many pharmacists regretted their absence after seeing the success of the meeting. According to an article written by a pharmacist, the pharmacists' organisation had not even replied to the chemists' invitation to the conference because pharmacists had not recognised the significance of chemistry in their field, even in the search for new medicines.<sup>13</sup>

The main decisions of the congress included the harmonisation of interests of the industrial capital, and the chemist's profession. They decided to adjust the training of chemical engineers to the specific requirements of industry in the way that new departments would be set up and industrial chemists would occasionally give classes.

## **Conclusion**

The first national congress of the Hungarian chemists can be seen as a typical act of boundary work. Apart from the nationalist dimension, the main task of the congress was to represent a field, as an entity in its own right. This policy context inspired the organisers to show chemistry as a large, powerful territory. They realized this aim by including into their census all possible groups that were relat-

ed with chemistry, even the pharmacists. This act had nothing to do with any essentialist definition of chemistry. The organisers did not attempt to deduce the boundaries of chemistry from any metaphysical idea. In establishing the boundaries of chemistry, they were influenced, as explained earlier, by the interests of acquiring better job opportunities for people whom they considered chemists. In the discussions, it became clear that by chemists the majority of the speakers meant chemical engineers. They tried to exclude all graduates of industrial schools and the ‘philosopher chemists’, the graduates of Budapest University, from the field of chemistry.

Boundaries change according to the context. For example, Michael Polanyi, a philosopher physical chemist, sharply distinguished between pure and applied sciences, and opposed science policy in general. Based on his views, and many others, ‘philosopher chemists’ could be considered to be closer to chemistry than could the engineers, because ‘philosopher chemists’ had more theoretical orientation than had the engineers. Hence, in an essentialist approach they might seem to be more real chemists than were the engineers. Therefore, pharmacy is clearly outside the boundary of chemistry, at least until chemists wanted to occupy positions in pharmaceutical companies that became prevalent after the 1920s in Hungary.

Since then, collaboration (peaceful coexistence) has proved to be a normal form of relationship at the boundaries of chemistry, as the hafnium story exemplified. The chemist Hevesy collaborated with the physicist Bohr, inorganic and analytical chemistry with old quantum physics.

The exclusion and inclusion technique reveals that the boundaries are change according to context. Boundary work relies on various devices, including ideologies. Essentialist arguments, such as reductionism, the always-debated relationship between chemistry and physics, and biology can be considered such an ideological instrument in the never-ending negotiation about the boundaries of lands. In the policy approach, the relationship between chemistry and its neighbors is a changing, collaborative, and negotiable process.

## References

<sup>1</sup> Mary Jo Nye, *From Chemical Philosophy to Theoretical Chemistry: Dynamics of Matter and Dynamics of Disciplines, 1800-1950* (Berkley, Los Angeles, London: University of California Press, 1993) 13-56.

<sup>2</sup> A. Simões, “Chemical Physics and Quantum Chemistry in the Twentieth Century”, in *The Modern Physical and Mathematical Sciences. The Cambridge History of Science*, Vol. 5., ed. Mary Jo Nye, (Cambridge: Cambridge University Press, 2003) 394-412., A. Simões, K. Gavroglu,

"Issues in the History of Theoretical and Quantum Chemistry, 1927-1960", In: C. Reinhardt (ed.), *Chemical Sciences in the 20<sup>th</sup> Century*, ed C. Reinhardt, (Weinheim: Wiley-VCH, 2001), and K. Gavroglu, A. Simões, "The Americans, the Germans, and the beginnings of quantum chemistry: The confluence of diverging traditions". *Historical Studies in the Physical Sciences* 25, (1994): 47-110.

<sup>3</sup> K. Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959), 69.

<sup>4</sup> Karl Popper, *The Open Universe* (London: Hutchinson, 1982). Cited by Eric Scerri, "Popper's Naturalized Approach to the Reduction of Chemistry", *International Studies in the Philosophy of Science* 12 (1998): 33-44.

<sup>5</sup> The history of hafnium's discovery has been told in other places. See, e.g., Helge Kragh, Peter Robertson, "On the discovery of element 72," *Journal of Chemical Education*, 56, 1979, 456-459. Gabor Pallo, "Scientific recency: George de Hevesy's Nobel Prize", in: *Historical Studies in the Nobel Archives: The Prizes in Science and Medicine*, ed. Elisabeth Crawford, (Tokyo: Universal Academy Press, 2002). 65-78.

<sup>6</sup> See, e.g., George de Hevesy, "A scientific career", in George de Hevesy *Adventures of Radioisotope Research. Collected papers*. (London: Pergamon Press, 1962), 21.

<sup>7</sup> E. Scerri, "Prediction of the Nature of Hafnium from Chemistry, Bohr's Theory and Quantum Theory." *Annals of Science*. 51 (1994): 137-150.

<sup>8</sup> Thomas Gieryn, *Cultural Boundaries: of Science: Credibility on the Line* (Chicago, London: Chicago University Press, 1999).

<sup>9</sup> unknown author, "Néhány szó a vegyészi hivatásról", (Some words on the vocation of chemists) *Gyógyszerészi Hetilap* 44 (1905): 774.

<sup>10</sup> Short unsigned article *Vegyí Ipar* 4 (1905): 22. 4.

<sup>11</sup> Molnár Varga Ödön, (letter to the journal) *Vegyészet Lapok* 2 (1907): 145.

<sup>12</sup> S, (no full name), "A végzett vegyészek helyzete", (The situation of the chemistry graduates) *Vegyészet Lapok* 3 (1908): 271.

<sup>13</sup> Unsigned article "Vegyészek országos kongresszusa", (National congress of chemists) *Gyógyszerészi Értesítő*.

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## Chemistry in the 21st Century: Death or Transformation?

*Peter J. T. Morris\**

For some time now, the author has been concerned about the future of chemistry. A concern –as for many chemists– stemmed from the closure of several chemistry departments in the UK over the last decade and attempts to re-brand chemistry elsewhere as part of the biomedical sciences, forensic science or even heritage research. This paper is a first attempt at an analysis of the possible future for chemistry in the 21<sup>st</sup> century. In a brief paper such as this it is impossible to cover chemistry across the world and it is also not certain that such a global analysis would give a very clear picture. As a British historian it is realistically only possible to deal only with the situation in the United Kingdom. A study of the USA or Germany would differ somewhat and the position in Asia (China, India, Japan) is very different. It may well be that the Asian situation might even undermine my thesis.

It also has also become clear that it would be too complicated and possibly even confusing to analyse long term series of data. For simplicity and clarity, 1977 –the centenary year of the Royal Institute of Chemistry (RIC)– will be compared with 2006. As it happens, 1977 was also the year the author graduated in chemistry.

This is not just a statistical analysis; it is also a cultural study of what chemists are doing. It is thus argued that the well-being of chemistry is not only a matter of data –how many chemistry degrees taken, how many chemistry papers published– but also the internal and external perception of chemistry, how chemists see themselves and how others see chemistry. To put it another way, is what chemists do in the 21<sup>st</sup> century actually chemistry insofar as it would have been recognised by chemists living 50 or 100 years ago as being chemistry? This is not a trivial point, is there any point in talking about chemists and chemistry if what they do is no longer recognisable in the *longue durée* as being chemistry? This is one aspect of the debate where historians have a particular role to play.

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In the RIC's centenary year of 1977, there was a special exhibition at the Science Museum based on a major revamping of the chemistry galleries, which then covered almost 12,000 square feet. A book on the history of the RIC written by three professional historians of chemistry at the Open University, Colin Russell, Noel Coley and Gerrylynn Roberts. A special leather-bound copy was even presented to Prime Minister, Jim Callaghan. The Royal Mail even issued a set of stamps celebrating British chemistry to mark this milestone. There was much to celebrate. The total professional membership of the RIC had grown exponentially since 1915 to over 23,000. After steady but moderate growth since it was founded in 1841, membership of the broader-based Chemical Society had also grown rapidly since 1940. There was an average of about 2,500 chemistry degrees awarded a year between 1965 and 1977; 1977 was actually a trough year with around 2,250.

Chemistry was in good shape in this period. Thanks to the expansion of higher education in the 1960s, there were more chemistry departments in universities than ever before including Sussex, Warwick, York, Stirling, Bath, Lancaster and Kent. The number of non-German publications in chemistry per annum had risen from 8K in 1918 to 302K in 1975. There were also many new journals, including *Accounts of Chemical Research* (1968), *Macromolecules* (1968), *Chemical Physics* (1973), *Heterocycles* (1973), *Synthesis* (1974), *Tetrahedron Letters* (1975).

In the modern jargon, chemistry was a strong brand in 1977. It had a clear identity and was seen as successful. Nearly all Nobel Laureates in chemistry in this period were clearly "chemists" and worked in chemistry departments: Barton (1969), Wilkinson (1973), Flory (1974), Prelog (1975) and Lipscomb (1976). Among the successes of chemistry during the previous decade were: the total synthesis of vitamin B12 (Wilkinson and Eschenmoser, 1973) and the prostaglandins (Corey, 1969), the Barton-McCombie reaction (1975), organorhodium catalysis (Wilkinson, 1966), uranocene (Streitwieser and Müller-Westerhoff, 1968) and crown ethers (Charles Pedersen, 1967).

Three decades of research by historians and sociologists of science have shown that this success was a culmination of a century and a half of discipline building, specifically:

- Positioning chemistry as a profession not a craft.
- Restricting entry into the field (only partly successful).
- Emphasising pure science rather than application to problems.
- Invoking the linear model of innovation.
- Protecting the boundaries of chemistry from invasion by physics and biology-medicine.

Although chemistry may have been a strong brand, it did have its weaknesses even in the 1970s, namely:

- Loss of biochemistry and molecular biology only partly counterbalanced by the creation of physical organic chemistry and bioorganic chemistry.
- Only a minority of chemists belonged to the RIC.
- Chemistry had a poor image of long-standing, which was increased by the explosions at Flixborough in England in 1974 and Seveso in Italy in 1976.
- Chemists to most people in the UK were pharmacists not chemists.

So we could sum up the situation in 1977 by saying that while chemistry had its problems, both chemists and the public had a clear sense of what chemistry was about and most people accepted that –at a minimum– chemists did more good than harm, a view that was promoted by advertising campaigns by the major chemical companies including Du Pont, ICI, BASF and Bayer.

Turning to 2006, the situation at first glance does not look bad at all. The total Corporate Membership of the Royal Society of Chemistry (RSC) was 37,970 compared with 27,850 in 1980 when the RSC was formed by a merger of the RIC, Chemical Society and Society for Analytical Chemistry. The number of chemistry graduates in 2004 was 2,735 roughly comparable with 1977 although the figures are calculated differently.

But these good numbers look different when considered as a trend...

After a long period of stability thanks to a slow decline in Fellows being counterbalanced by a growth in Members, the number of all corporate members began to fall slightly from 2003. Similarly the number of chemistry degrees awarded fell from 4,144 in 1996 to 2,735 in 2004. This fall in the number of chemistry students was paralleled by the closure of six chemistry departments between 2003 and 2005 including King's College and Queen Mary's in London, Lancaster, Kent and Swansea although Queen Mary's has since reopened.

However the most striking change is the changing nature of the subject. Whereas chemistry in 1977 was very much physical, organic and inorganic chemistry, by 2006 it had become biomolecular science, materials and nanotechnology. Many recent Nobel Laureates in chemistry have not been traditional chemists. Of the nine Nobel Laureates in chemistry since 2003, six have come from biomedical faculties or institutes not chemistry departments.

One major change in chemistry over the last three decades has been the relative decline of the chemical industry relative to the pharmaceutical industry in the United Kingdom. It is worth reflecting that the chemical industry in 1977 had a



strong historical continuity with the past –all the major companies could easily trace their history back to the mid-19<sup>th</sup> century– and the larger chemical companies were a major economic power in their native countries –such as Du Pont in the USA, ICI in the UK and BASF in Germany. Indeed it is even noteworthy from the perspective of 2008 that most chemical companies in 1977 were still associated with specific countries. Above all these companies were household names, who had not heard of BASF tapes, Dulux paint (associated with ICI in Britain rather than Du Pont), nylon or Perspex? It was only when the workers at Grangemouth refinery in Scotland recently went on strike, that the author discovered that Ineos –the owner of Grangemouth refinery– was one of the largest chemical companies in the world.

As the chemical industry faded from the public gaze, the pharmaceutical industry –important even in 1977– has become crucial to the survival of chemistry. The number of R&D scientists employed in the UK pharmaceutical industry doubled between 1978 and 1999 whereas it remained static in the chemical industry. Furthermore more and more academic organic chemists were working on pharmaceutical topics. This shift has come however at a price. Whereas the chemical industry was a standard-bearer for chemistry, the pharmaceutical industry is increasingly coy about its links with chemistry. For instance, prescription medicines are never described as chemicals even though, for example, pantoprazole tablets contain povidone, titanium dioxide, methacrylate-ethacrylate copolymer and even sodium lauryl sulfate (oddly enough this information is only given in the Italian labelling, not the English). Perhaps more importantly the pharmaceutical industry stresses the role of biomedical sciences as a whole –and rightly so– rather than just chemistry.

To meet these challenges from other scientific fields, the RSC has been re-branding itself in recent years as “largest organisation in Europe for advancing the chemical sciences” broadly defined, rather than a British chemical society. But as we have seen that chemistry is itself changing. Will this re-branding work or will chemistry as it was understood it back in 1977 disappear?

What are the possible outcomes for the future of chemistry? Chemistry may change its nature and become a fusion of biomolecular science, materials and nanotechnology but retain the name. There has been similar changes before –chemistry in 1750 was different from chemistry in 1840 and again from chemistry in 1930– but this is a momentous change... will this new constellation of sub-disciplines and quasi-disciplines stick together?

Whereas the relative importance of sub-disciplines within chemistry and even the rise of new sub-disciplines could be accommodated fairly easily, taking over other disciplines is much more difficult, the more so since they had already “escaped” from chemistry. But the key issue herein is that biomolecular science and biomedicine as a whole and materials science are both more powerful than chemistry itself—and crucially they are both seen as being more relevant than chemistry so this takeover bid is unlikely to succeed.

Although the chemical industry still exists and indeed still employs a large number of chemists, its relative decline (and indeed the decline of manufacturing in general) makes it harder for chemistry to prosper as its relevance is seen to be declining and it lacks the PR boost that a strong high-profile chemical industry could give it as it has done in the past.

If chemistry is unable to re-brand itself successfully, it will either continue to just reposition itself and trust in the preference of some scientists to label themselves chemists –despite the lack of any continuity with the past– or it will just fade away as former would-be chemists identify themselves as biomedical scientists, materials scientist or physicists. Perhaps nanotechnology will be the final refuge of chemists although it is as much physics as chemistry. As all institutions –including scientific disciplines and chemical societies– seek to prolong their existence, I suspect it will be a drawn out “whimper” rather than a “bang”. Perhaps in a century from now, chemical societies will have become like the medieval guilds that still exist in the City of London, for example the Salters Company, which however does excellent work in the field of chemical education.

If these conclusions are correct, and they are at present a hunch rather than a rigorously proved hypothesis, this suggests that chemistry over the next half-century will revert back to its eighteenth century origins in medicine (= biomedicine) metallurgy and mineralogy (= materials science) and natural philosophy (= physics). Obvious alchemy as a pseudoscience will have no part to play but it is entirely possible that alchemy as practised today may outlive chemistry! Indeed even now the Science Museum gets more enquiries from the media about alchemy than about chemistry.

Do the conclusions have any relevance to historians of chemistry? It is argued it changes the way the subject is to be viewed. If its history leads ultimately to dissolution it must alter the perception of that history. For instance it surely affects the way one studies the coming together of mineralogy and metallurgy in the eighteenth century if it is known –at some point in the future– that alliance was dissolved. An understanding of the development of chemistry –which is very dif-

ferent from the very limited way most practising chemists view it— enables practising chemists as well as historians of chemistry to better understand why it is unravelling.

But if chemistry does unravel and becomes biomedicine and material sciences, where does that leave the history of chemistry? Will the history of chemistry survive or will it too unravel to become part of the history of medicine and the history of the material sciences? Is there precedence for the history of a “dead” discipline? Certainly the history of alchemy is very healthy, more so than the history of chemistry.

The author does not wish to be unduly alarmist or to claim that the end of chemistry is nigh. Chemistry is not dead or in pieces yet. It is still a major scientific discipline with many professionally trained practitioners. In some countries, notably in China and India, it is still flourishing. So it is probably a bit early to start worrying, but if it does unravel over the next few decades, please remember you read it first in this paper.

## Sources

This is not intended to be a fully referenced paper. Most of the data, with the exception of the number of non-German publications, was kindly supplied by the Royal Society of Chemistry for which I am very grateful, but the conclusions drawn from them are entirely my own. The number of non-German publications was taken from Arnold Thackray, *et al.*, *Chemistry in America* (Dordrecht: Kluwer, 1985), Table 6.8, 400. I had previously touched on some of the themes in this paper in “Between the Living State and the Solid State: Chemistry in a Changing World” in *Chemical Sciences in the 20<sup>th</sup> Century: Bridging Boundaries* (Wiley-VCH, 2001), ed. Carsten Reinhardt, 193-200. Other papers which deal with this question include John W. Moore, “What’s the Future of Chemistry?” *Chemical Education International*, 1 (1) (2000): 8-10 and Stephen W Breuer, “Does Chemistry have a Future?” *University Chemistry Education* 6 (1) (2002): 13-16.

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## Chemistry Around Medicine and Pharmacy in the Work of Amatus Lusitanus in the Sixteenth Century

Fátima Paixão\*

In the 16<sup>th</sup> century arose considerable chemical knowledge via Medicine and Pharmacy, which like today, are in very close contact. At the same time Chemistry joined the techniques and ways of thinking which helped it to develop in its own right a few centuries later.

Amatus Lusitanus (1511-1568) was a notable physician from the Renaissance, born in Castelo Branco/Portugal, who obtained a great reputation all in the Europe of his time. His work has many and interesting aspects on the beginning of Chemistry as associated to Medicine and Pharmacy. The collection of the seven *Medicinalium Centuriae*, written by Amatus Lusitanus, reunited an enormous set of episodes reporting medical situations, in which he participated, both those in which he succeeded and those in which he did not succeed. Described in detail and rigour, all the episodes demonstrate his meritorious human character as well as his technical expertise.

This descriptive study will be approached in five parts: in the first will be presented a brief description of the life and work of this important physician, reliving his troubled face travelling in renaissance Europe, in part due to his Jewish origin; the second aspect presented is the analysis made on the content of the first *Medicinalium Centuriae* (100 medical cases) highlighting organic (plant and animal origin) and mineral materials included in the medical prescriptions; in the third phase the laboratory techniques and operations in order to prepare the medicines are mentioned and briefly described; In the fourth part, aspects related with precision and rigour in the prescriptions, particularly in what respects measurements of the quantities used in the preparation of medicines, are evidenced. The thematic topic of weighs and measures was our main interest in the analysis recently made on the work of Amatus Lusitanus (Paixão, Jorge and Florido, 2005). In this context, were also developed a project with young pupils centred on

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the topics of precision and measures based on historical documents (Fradesso da Silveira, 1856), carried out to associate the history of science into primary and secondary school activities (Paixão et al, 2006). Other aspects of the work of Amatus Lusitanus, as described in his *Medicinalium Centiriae*, are also very appropriate to use in such activities for young pupils. Finally, is a discussion of the relations of Amatus Lusitanus with the ideas and theory of his time, especially supported by *Paracelsus*. All these interrelated aspects highlight the very beginning Chemistry, materialised in the applications and rigour required by Medicine and Pharmacy.

### The Physician of the Renaissance

The physician João Rodrigues de Castelo Branco (1511-1568) adopted the name of Amatus Lusitanus after he left Portugal in escaping the Inquisition which devastated the south of Europe at the time.

Their studies at the University of Coimbra, in 1529-1533 Amatus Lusitanus graduated in Medicine at the University of Salamanca, Spain, when he was 18 years old. After the graduation, he came back and travelled all over his country, practising



**Figure 1.** The statue in honour of Amatus Lusitanus in Castelo Branco

tising medicine and searching for medicinal plants. It is well known that by the middle of the century he practised medicine in Portugal. Some of the reports in the *First Medical Centuria* report medical cases observed in his country. The absence of liberty caused by the Inquisition imposed him the precarious condition of being an exile in several countries of the Europe for the rest of his life. He never came back again to Portugal, but he never forgot his native country, adopting in his name a direct reference to the lands where he was born (*Lusitania*).

In 1534 he was in Antwerp practising medical clinic, but the Inquisition obliged him to change his work place. He went to Ferrara, where he also taught Medicine,



Figure 2. The title page of *Centuria*

staying there for 6 years. In the next three years he worked in Ancon and probably in 1551 he arrived in Florence. He practised medicine in several Italian States, and he went to Rome several times being asked by influential people. Again for religious motives, he took refuge in Ragusa (1558), and finally lived in Thessalonica. The plague spread in this region and Amatus Lusitanus involved himself arduously in his work trying to save people and lost his own life in 1568. Among his main books are *Index Dioscorides* (published in Antwerp 1536), *In Dioscorides Anabarzaei de Medica materia librum quinque enarrationes eruditissimae* (Venice 1553), *Curacionum Medicinalium Centuria septem* (Venice, between 1552-1559).

*Lusitanus* was the first to observe the valves of veins (1547) giving a valuable contribution to the study of the blood circulation and the *Pharmacopea Lusitana* (Santo Antonio, 1704) refers to him as a notable botanical observer and recorder, as well as an expert in the field of Pharmacy. The main work of Amatus Lusitanus was the set of seven medical *Centuriae*, which were not translated from Latin to Portuguese until the beginning of the 20<sup>th</sup> century (Pita, 2000). His clinical attitude was very relevant. As he declared: “As a physician I never abandon a patient despite I known that he will die, because some times occurred surprises (*miracula*) as I has verified some times” (Castelo Branco, 1552 [ed.,1946]).

Also meriting a great attention is the Medical Oath written in 1559 in Thessalonica. In this he reaffirms his great dedication to the patients, independently of economical power, social status or religion; for him all suffering people merited dignity, care and attention.

The physician began each episode in the *Centuriae* by a careful description of the case presenting the whole situation, a detailed diagnostic and the prescription, including the curative ingredients and the technical procedures to be carried out by the patient or by their accompanying person.

Amatus Lusitanus stated all the materials used, the operations needed for the preparation of medicines and the proper medical interventions as well as the precise quantities needed. The rigour and critical reflection about his decisions and results are their main scientific attributes. He observed the case until the end, death or health, reflected on and wrote about it, patiently reporting and collecting them.

Despite having been the physician of Catharine of Medici and of Pope Julius III he never discriminated anyone because of their origins or religion and he never forgot his birthplace making various references to it in his vast work. As a brilliant professional he gained the status of citizen of Europe.

### **Organic and mineral materials in the first *Curationum Medicinalium Centuria***

Plants are the most abundant materials used in the first *Curationum Medicinalium Centuria* of Amatus Lusitanus. Among these were: endive, chamomile, aneto, fennel yellow, flax, marjoram, penny-royal, roses, madder-plant, senna and violets. Despite that the Mediterranean flora were the most influential, there were references to some oriental species such as saffron, ginger, pepper and cinnamon. Frequently some parts of animals were prescribed in the preparation of medicines, for example, bees, birds (chicken, pheasant, gander, duck...), blood-sucking worm, wild boar, bull, among others. Another aspect which must be mentioned was that the reports were full of suggestions for nourishing diets, which included animal and vegetable products.

Also, some minerals were prescribed to the patients and used as medicines in a diversified range of processes: water, rocks, salts, iron, mercurial, nitro of Alexander and *lapizlazzulli* (lazurite).

Amatus Lusitanus referred to other organic materials and substances easy prepared at the time, such as sugar (coming from Madeira Islands), wine, Arabic gum, opium, vinegar, olive oil and oxymel (vinegar with honey).

In Table 1 we present the numbers of different plants, animals and minerals cited by the physician in the first set of medical episodes (100 cases). There are a small number of designations of materials which scientific identification and origin we did not yet do.



**Table 1**  
**Numbers of plant, animal and mineral materials cited in the first *Centuria***

<i>Plants</i>	<i>Animals</i>	<i>Minerals</i>
88	25	10

### **Techniques and Operations**

Some techniques and operations could be identified in the work of Amatus Lusitanus evidencing the involvement and the progressive development of the Iatrochemistry art and knowledge.

Preparation of syrups, pills, plasters, unguents, potions, used in liniments, as cataplasms, clysters, smoking were frequently prescribed. Techniques like maceration, extraction, dissolution, decoction, infusion, refrigeration, fermentation and others, are frequently suggested for the preparation of medicines.

### **Precision and Rigour in the Prescriptions**

Being the Renaissance the time of the development of the European trade, the need to measure was compulsive. A rigorous physician like Amatus Lusitanus, when he prescribed the medicines he indicated the amount of each species to be used, with a high precision. The use of rigorous amounts, expressing the unit of measure, was a constant in all the medical episodes reported in the *Centuriae* (Paixão, Jorge and Florido, 2005). Mainly he used or suggested amounts to be measured by a balance. Frequently he also suggested volumes, mainly for liquids. For the mass he frequently points to units of measure like the pound and half-pound, the ounce and half-ounce, drachma and half-drachma and the scruple. For the volumes he used, among others, the ounce for liquids.

Only a few times he use amounts not well defined, using anthropometric units as was the case of maniples and handfuls, despite these two units have been used frequently by apothecaries. There were, also, mention to units of length (mile, fathom, ell – three spans long, foot, span...) and of time (including the definition of hour as the 24<sup>th</sup> part of a day).

At this time we were far from any standardisation of the units of measure but his concern about the rigour is explicit in the frequent definition of the unit to be used. The case of the definition of the “day”, in the first *Centuria*, before the report

of the medical episodes, is an important example. After a detailed explanation he clarifies that the day is the equinoctial gap of 24 hours.

### **“The spirit of the times”**

The physician Amatus Lusitanus adopted a way of working, mainly based on observations and hypothesis or diagnostics of the sickness; the sound reflection during and after the situations or prescriptions conducted him to the analysis of the results describing carefully each clinical case. When he reflected on the cases he never hesitates to compare and, if it was his opinion, to make criticisms of earlier physicians, such as Dioscorides and Galenius.

In his work he said directly, that he did not accept the Aristotle’s theory of the four elements. In which respect to the field of Chemistry, *Paracelsus* was the most famous alchemist living in the same time of Amatus Lusitanus. There were no references to this alchemist in the text of the first *Centuria*. But it appears evident that he disagreed with *Paracelsus’* vitalist theory of the three principles of salt, sulphur and mercury. His public position was clearly against the idea to consider the human body as a place where the three principles changed one to another, giving disease or health. He considered himself as more rational, based on reflection, observing and experimenting (for more details see, among other references in the ambit of the history of chemistry, Salzberg, 1991, Multhauf, 1993 and Debus 2001, as well as the written work of Amatus Lusitanus). However, he used the substance mercury in some cases, mainly in those related with syphilis, as was the practice in Europe at that time.

### **Conclusion**

After having highlighted some aspects of the troubled life of the important renaissance Portuguese Physician João Rodrigues de Castelo Branco, identified as Amatus Lusitanus, examples were given of materials and substances, techniques, operations and units of measure referred in the text of the first *Medicinalium Centuria* of Amatus Lusitanus, some conclusions can be given:

- (i) The most used ingredients were organic; botanical species coming from the Mediterranean flora but also a vast range of animal origin materials were used, also minerals and some botanical species coming from the Orient and

Brazil. His contribution as a botanical observer and recorder is considered to be very important.

- (ii) There are descriptions to numerous operations in order to prepare the medicines, extractions, dissolutions, fermentation or refrigeration, well known even today in Chemistry Laboratories.
- (iii) There are references related to the precision and rigour in the prescriptions, particularly in respect to measurements of the quantities used in the preparation of medicines. Units of mass, volume, length and time were always indicated.

The growing of the use of balances in the instructions and prescriptions of Amatus Lusitanus in which respects to the preparation of medicines was an indicator of his great concern with rigour.

Amatus Lusitanus can be considered as a symbol of an anticipated rigour which would be translated into Modern Chemistry, the principle of mass conservation (Lavoisier, 1864). It is to be noted that his work methodology, like in modern sciences, included the rigorous observation and description of medical cases.

These aspects are distinctive marks of an important contribution to the development and empowerment of Chemistry. Only two centuries latter it changed and triumphed as a rigorous scientific field by the systematic use of sophisticated balances by Lavoisier and his followers. In his time Amatus Lusitanus made an important contribution in the field of Medicine and Pharmacy giving Chemistry the opportunity to affirm its importance.

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## References

- Allen G. Debus, *Chemistry and Medical Debate. Van Helmont to Boerhaave*. (Canton MA: Science History Publications, 2001).
- Antoine Laurent Lavoisier, *Œuvres de Lavoisier*, (Paris: Imprimerie Impériale, 1864).
- Caetano de Santo Antonio, *Pharmacopea Lusitana* (Coimbra: Impressão de Joam Antunes Mercador de livros, 1704).
- João Rodrigues de Castelo Branco [Amatus Lusitanus], *Primeira Centúria de Curas Médicas*. (Lisboa: Livraria Luso-Espanhola, tradução 1946).

- João Rui Pita, *História da Farmácia*, (Coimbra: Minerva, 2000).
- Joaquim Henrique Fradesso da Silveira, *Compendio do Novo Systema Legal de Medidas*. (Lisboa: Typographia do Centro Commercial, 1856).
- Fátima Paixão; Fátima Regina Jorge; Ana Isabel Florido, “Pesos e Medidas na obra de Amato Lusitano: Dos saberes e das certezas da época”. *Medicina na Beira Interior. Cadernos de Cultura*, 19(2005), 19-28.
- Fátima Paixão; Fátima Regina Jorge; Paulo Silveira; José Teodoro Prata. *Projecto Ciência Viva: Problemas com conta, peso e medida. Projecto 1374*. (Lisboa: Ciência Viva, 2006)
- Hugh W. Salzberg, *From caveman to Chemist. Circumstances and Achievements*. (Washington, DC: American Chemical Society, 1991)
- Robert P. Multhauf, *The Origins of Chemistry*. (Langhorne: Gordon and Breach Science Publishers, 1993).

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## Pharmaceutical and Chemical Laboratories in Eighteenth-Century Germany

*Ursula Klein*<sup>\*</sup>

Among the leading German chemists in the 1780's, J. C. Wiegleb (1732-1800), M. H. Klaproth (1743-1817), L. Crell (1745-1816), J. F. Gmelin (1748-1804), J. F. Westrumb (1751-1819), F. C. Achard (1753-1821), J. F. A. Göttling (1753-1809), F. A. C. Gren (1760-1798), and S. F. Hermbstädt (1760-1833) – Wiegleb, Klaproth, Westrumb, Göttling, Gren and Hermbstädt were apprenticed and became practicing apothecaries, and Wiegleb and Westrumb remained apothecaries throughout their professional careers.<sup>1</sup> Around half of the one to two hundred Germans carrying out chemical investigations and acknowledged as “chemists” in the 1780s became acquainted with chemistry as pharmaceutical apprentices and practicing apothecaries. The interconnectedness of pharmaceutical art and chemistry in eighteenth-century Germany also becomes manifest from analysis of the readers of and contributors to professional periodicals such as the *Chemische Annalen*. Among the 564 German subscribers to Crell's *Chemische Annalen* between 1784 and 1789, 260 (46%) were apothecaries, and among its German contributors more than 40% were apothecaries as well.

The socio-cultural German context can hardly explain how owners of apothecary's shops and manufacturers of remedies became acquainted with the practice and theory of chemistry and how they became visible as skilled and knowledgeable chemists in the Republic of Letters. What kind of activities earned them the attention of a learned and supportive audience? What were the sites and resources of these activities? How did apothecaries' chemical investigations relate to pharmaceutical manufacture? As a matter of fact, A. S. Marggraf, like other apothecary-chemists, did not begin his career as a chemist after leaving the pharmaceutical business, but rather developed it alongside, and even in conjunction with that business. Furthermore we may ask why apothecaries, who were trained in an artisan system of apprenticeship and earned their living as merchants and manufacturers of remedies, merged so smoothly with other factions of chemists,

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for instance those who had earned a medical doctorate or were mining officials and assayers. Were there any aspects of the actual practice of apothecaries that were similar to other chemists' practice? Were there, in addition to individual talent, collective beliefs, and state intervention, any collective material resources and elements of the practice and material culture of pharmacy that enabled apothecaries to carry out the same or similar kinds of chemical investigations as chemists working at other artisan sites or at academic institutions?

It can be argued that Marggraf, like other German apothecaries who became renowned chemists, was a truly hybrid apothecary-chemist, and further, that an indispensable condition for the existence of the persona of an apothecary-chemist in the eighteenth and early nineteenth centuries was the high degree of correspondence between the material culture and practice of pharmacy and the material culture and practice of "academic chemistry." Apothecaries did not have to bridge a huge gap between a rigid "realm of recipes" and pharmaceutical routine, on the one hand, and a realm of innovative, pure chemical science, on the other. Rather, pharmaceutical art and academic chemistry overlapped in the eighteenth and early nineteenth centuries, in Germany and elsewhere in Europe. Laboratories, pharmaceutical and academic-chemical, were the institutions where manufacture (in the case of pharmaceutical laboratories) or technological inquiry (in the case of academic chemical laboratories) and inquiry into nature were firmly entwined.<sup>2</sup>

As a consequence of the introduction and acceptance of "chemical remedies" during the seventeenth century, in the eighteenth century the pharmaceutical art was in a state of continuous change and innovation.<sup>3</sup> There was hardly any recipe for the manufacture of chemical remedies that was not questioned, varied, improved or replaced by a new one. And there was hardly any chemical remedy that was not on the test-bench for possible adulteration or a material that had not yet been identified unambiguously. Chemical techniques and instruments, connoisseurship of chemical substances, and chemical analysis became significant tools for mastering problems of manufacture. Inversely, the solution of problems of manufacture provided insight into the "nature" of substances and their chemical transformations. The similarity of the material culture and techniques of manufacture in eighteenth-century pharmaceutical art to the material culture and experimental techniques of academic chemistry enabled apothecaries to shift their activities smoothly from pharmaceutical manufacture to the chemical investigation of nature, or to perform chemical analyses alongside pharmaceutical manufacture. Likewise, it enabled chemists performing experiments at academic

chemical laboratories to shift from inquiries into nature to pharmaceutical and other technological inquiries.

## Notes

<sup>1</sup> See Karl Hufbauer, *The Formation of the German Chemical Community (1720–1795)* (Berkeley: University of California Press, 1982).

<sup>2</sup> For a further discussion of the theme and a more elaborate bibliography see Ursula Klein, “Apothecary-Chemists in Eighteenth-Century Germany,” in *New Narratives in Eighteenth Century Chemistry*, ed. Lawrence M. Principe (Dordrecht: Springer, 2007), 97–137; Ursula Klein, “Apothecary’s Shops, Laboratories and Chemical Manufacture in Eighteenth-Century Germany,” in *The Mindful Hand. Inquiry and Invention from the Late Renaissance to Early Industrialisation*, eds. Lissa Roberts, Simon Schaffer und Peter (Amsterdam: Royal Netherlands Academy of Arts and Sciences, 2007), 246–276; See Ursula Klein, “Die technowissenschaftlichen Laboratorien in der Frühen Neuzeit,” *NTM* 16 (2008): 5–38.

<sup>3</sup> See Erika Hickel, “Der Apothekerberuf als Keimzelle naturwissenschaftlicher Berufe in Deutschland,” *Medizinhistorisches Journal* 13 (1978): 259–76; Erika Hickel, *Apotheken, Arzneimittel und Naturwissenschaften in Braunschweig, 1677–1977*, (Braunschweig: Hagenmarkt-Apotheke, 1977); Wolfgang Schneider, *Geschichte der pharmazeutischen Chemie*, (Weinheim: Verlag Chemie GmbH, 1972).



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## Chemistry and Pharmacy in the Eighteenth Century; Lessons from and Limits to a Disciplinary Approach

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Researching and publishing a book on the relationship between chemistry and pharmacy, *Chemistry, Pharmacy and Revolution*, allowed time to reflect a great deal on its central arguments.<sup>1</sup> Originally conceived as a project in the history and philosophy of chemistry, the idea was to use a neighbour to outline the contours of the discipline of chemistry by default –a portrait in relief as it were– for this crucial period and place around the French Revolution. As was explained in the introduction to the book, the relationship between the French and chemical revolutions was much more appealing to me as an area for research than the relationship between the chemical and scientific revolutions. This latter question has tended to draw the attention of historians of chemistry in the past and was crystallised as a position to debate in Butterfield's much cited description of the chemical revolution as the 'postponed revolution'. Thus, Butterfield implicitly suggested that it was necessary to understand why the chemical revolution did not happen a hundred years earlier, orientating the history of chemistry around a project that seems less promising in light of contemporary approaches to the history of science.<sup>2</sup> Be that as it may, the point is that the original project for the book was firmly focused on chemistry.

Those who have read the book will have noticed that it turned out to be just as much a history of pharmacy –at least in its institutional aspects, but also some of its more technical scientific ones– as a history of chemistry. This history of French pharmacy around the French revolution, while not an unexplored area, lacked coherence, precisely because it had not been articulated around a major historical event. In sum, there was no equivalent of the chemical revolution in the history of pharmacy; no theorised 'pharmaceutical revolution'. After all, it is this chemical revolution centred on Lavoisier –or some other rival version of it– that has been behind a minor industry in the history of science focussed on the history of eighteenth-century chemistry.<sup>3</sup> The history of pharmacy that served as the context for my version of the chemical revolution had to be gleaned from biographies

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and partial institutional histories mostly involving famous Parisian pharmacists. This work was worth doing from my point of view because this history of pharmacy was needed precisely in order to present my central argument concerning chemistry. Nevertheless, what has just been said betrays a deep suspicion I have concerning my own work. The fact that I qualify the institutional history in these terms –history of chemistry versus history of pharmacy– suggests that I too assumed the existence of the entity I intended to reveal through this research –some kind of proto-discipline of chemistry that was going quite naturally to lead into chemistry. While my aim was not just to do the history of chemistry, but to describe its development in relation (and in opposition) to its neighbours, I suspect that this approach obliged me to find that this disciplinary division made sense there and then –at the end of the eighteenth century– whether it did or not. This question of the *a priori* assumption of the existence of a discipline is an important conceptual point that will be discussed in what follows.

The neighbour of chemistry that I chose to study was pharmacy, although this was not the only possibility, with dyeing and mineralogy two other obvious and interesting alternatives. In the course of this research, I was reassured that the choice of pharmacy was a relevant and significant one due to the large numbers of seventeenth and eighteenth century publications with chemistry in the title that one would have to consider to be primers in pharmacy or else simply courses in practical pharmacy. In my reading of these texts, I consciously looked beyond the introductions of these so-called chemistry books, following the methodological exhortations of the post-positive generation of historians, whose writings served to orient me in this field.<sup>4</sup> Considering eighteenth-century chemistry books as a whole, and not just looking at their preliminary reflections on conceptual issues (the nature of the elements, the possibility of mechanical explanations of chemical reactions, etc.), it was striking to find that the bulk of their content consisted of a mass of practical empirical (or experimental) information designed primarily to provide pharmacists and other medical practitioners with the skills necessary to prepare the medicaments listed in the pharmacopoeia.

These observations led me to underline an important feature of the history of chemistry that I was neither the first, nor no doubt will be the last to notice. To illustrate this observation, consider the work of Nicolas Lémery (1645-1715), perhaps the most successful writer of chemical textbooks of the seventeenth century. Lémery's *Cours de chymie*, which went into many editions, is well known in the history of chemistry, but the element that is most often discussed by historians is Lémery's corpuscular theory to the exclusion of the rest. Looking at James Partington's history of chemistry from 1961, to consider only one enduring refe-

rence work, it can be seen that he has little to say about the function of Lémery's work for pharmacists in terms of supplying practical techniques for preparing medicines.<sup>5</sup>

Nevertheless, a typical protocol for preparing extract of rhubarb taken from this seventeenth-century chemical classic clearly illustrates Lémery's orientation:

"Slice six or eight ounces of good *Rhubarb*, and steep it twelve hours warm in a sufficient quantity of *Succory Water*, so as the Water may be four fingers above the *Rhubarb*; let it just boil, and pass the Liquor through a cloth; infuse the Residence in so much more *Succory Water*, as before, then strain the Infusion, and express it strongly: mix your Impregnations, or Tinctures, and let them settle ; filtrate them and consume the moisture in a glass Vessel, over a very gentle fire, until there remains a Matter that hath the consistence of thick honey, this is called *Extract of Rhubarb*, keep it in a Pot."

This practical description is interesting on several levels. First, for a modern chemist, it reads more like a cookery recipe than the protocol for a chemistry experiment. But it is not this artisanal 'recipe' feature of the description that I want to emphasize; it is the goals of such a preparation. Thus, it is important to consider the paragraph that follows the protocol I have just presented.

"The Dose is from ten Grains to two Scruples in Pills, or dissolved in *Succory water* for Diseases of the Liver and Spleen, it binds as it purges."<sup>6</sup>

Here, Lémery provides the reason for the inclusion of the preparation of rhubarb extract in a chemistry course. The extract is used in medicine to treat diseases of the liver and spleen. Of course, for the modern reader this is pharmacy and not chemistry, but what I argue in my book is that such a clear and categorical distinction did not make sense in either the seventeenth or the eighteenth centuries. I do not want to suggest that this preparation of the extract of rhubarb represents any form of innovative chemistry that ought to have attracted Partington's attention but failed to do so. I simply want to use this example to make two points:

First, this kind of preparation is typical of a large part of Lémery's book, but he usually features in histories of chemistry because of his corpuscular theory. This theory, with, for example, an explanation of acidity in terms of pointed corpuscles, is no doubt interesting, but it represents neither the majority of the content nor the principal object of his book.

The second point is that the practical use of chemistry is downplayed in classic histories of chemistry like the one written by Partington. For many historians of chemistry, such protocols for preparing medicines do not constitute authentic or

legitimate chemistry, and, in a sense, this is a legitimate reason to exclude them from histories of chemistry. What one includes in a history of chemistry necessarily depends on what one considers chemistry to be. Rather than shifting to an abstract historiographical meta-level of analysis, however, we can simply note that books like Lémery's did contain a great deal of 'theoretically unsophisticated' practical chemistry. Furthermore, the endless macerations to produce extracts of bark or other organic substances were not for the most part considered to be chemistry by the nineteenth- and twentieth-century chemists writing the history of their own discipline. This tendency to exclude 'doubtful' material from a discipline's history is even more marked in periods of uncertainty concerning a discipline's future. As this seems to be the situation for chemistry today, it would not be surprising to see this tendency in the history of chemistry promoted as much as it is challenged in contemporary work in the field.

It is of course easy for the modern historian to criticise nineteenth- and early twentieth-century historians of chemistry as naïve (or maybe not so naïve) discipline builders, an accusation that comes readily to a historian who is only too aware that most of these men from Berthelot to Partington were themselves academic chemists.<sup>7</sup> Nevertheless, however sophisticated we may think ourselves today, questions of identity and the task of identifying what is and is not chemistry, what is or is not pharmacy, who is and is not a chemist and who is or is not a pharmacist always present their own pitfalls. Furthermore, just identifying conflicts of interest is not enough to negate their effects, a rule that is as true for historical as it is for scientific research. It is not because we realise that others have projected scientific disciplines inappropriately onto the past that we will not end up doing the same thing. Following these cautionary reflections, it is time to return to the approach I adopted in my book, which was intended to give pharmacy the place it merits in the history of science, while at the same time helping us to understand the rise of the discipline of chemistry that inspired the great histories of chemistry I have just been criticizing.

### Theories of Nationalism and Disciplinary Identity

The inspiration for the approach adopted in *Chemistry, Pharmacy and Revolution* was work on nationalism, and one theoretical approach in particular. The year 1983 saw the publication of two landmark books on nationalism, *The Invention of Tradition*, a volume of essays edited by Eric Hobsbawm and Terence Ranger, and Benedict Anderson's *Imagined Communities*.<sup>8</sup> *The Invention of Tradition* is a wonderfully amusing collection of articles that demonstrate with brio the pressing

need in the nineteenth and twentieth centuries to construct coherent nationalist narratives. These more or less conscious constructions of nationalist narratives often promoted relatively new inventions as age-old traditions endowing them with enduring significance as markers of national identity. These traditions could then serve to define an authentic culture that legitimized a nationalist movement either in its hegemonic rule or in its struggle against the powers that be. Suggestive though this collection of essays may have been, Anderson's book had a more direct influence on my work. I used Anderson's idea of 'imagined communities' to try and make sense of disciplinary formation among chemists, thereby transplanting the model he proposed for nation formation into the domain of the sciences. I chose this model because I found it particularly compelling in terms of the experience of nationalism –but this maybe says more about me and my training in history than anything else. Thus, Anderson's approach reflects a cultural sensibility, rather than an essentialist one. There have been those who have argued that nations reflect natural ethnic groupings, or discrete geographical structures, while others –doubtless the majority today– reject this essentialist vision of the nation. In brief, the argument of the anti-essentialists is that *nations are made –and are not natural objects*. If there is one point in common that deserves to be underlined between my approach to chemistry and Anderson's to nationalism, it is this anti-essentialism.

Anderson argues that nations were formed by a series of mechanisms that put people into relation with one another, but in a certain way. The newspaper is a particularly significant element in this history for him, and he clearly identifies print capitalism as the most important motor for emergent nationalism. The simultaneous reading of daily newspapers in the vernacular language allowed people who might never meet one another to share a sense of community. Thus, individuals separated in space and even time could project an importantly similar image of the national community, which constituted the precursor to the nation in so far as it coincided with the projection made by others. The community was, therefore, in this picture imagined before it was realised. Note that while the national community is imagined it is NOT imaginary, and it is vitally important to avoid this trivial misinterpretation of Anderson's position. So while this model is constructivist in the minimal sense of being anti-essentialist, it is not an extreme social constructivist vision –on Anderson's model, one cannot create national communities at will.

In *Chemistry, Pharmacy and Revolution*, I attempted to transpose this model onto the nascent chemistry community in France. Chemistry in the second half of the eighteenth century experienced the constitution of a new identity that was crow-

ned in France and in the rest of Europe in the nineteenth century by the development of academic and industrial career paths for chemists as chemists. It was these chemists who championed Lavoisier as the founder of a reformed discipline if not the inventor of chemistry. Paradoxically, though, while in their minds he was one of them, he never really was. Thus, Lavoisier was not by any means a typical chemist, in that while he did find applications for his chemical knowledge, his substantial income did not depend (directly) on his mastery of chemical operations or his sophisticated theoretical knowledge in this domain. He was neither a doctor nor a pharmacist nor a mineralogist but rather an amateur in the modern sense of being an unpaid practitioner of the art. One of the advantages of identifying Lavoisier as the starting point of modern chemistry, however, was that, while not posing the question explicitly in terms of the discipline or definition of chemistry, he was eager to divide chemistry off from its associated chemical arts. Indeed, this underexplored feature of Lavoisier's work provided much material for the book.

Another issue developed in the book was the role of the French Revolution in this history of the rise of philosophical chemistry, and more particularly Lavoisier's chemistry. This political and social revolution through its multiple reforms imposed a certain degree of uniformity across the Republic and disseminated the new chemistry through a series of new and reformed establishments for higher education. Of course, Lavoisier's version of chemistry was well suited to the promotion of the science in an academic setting, as it offered a combinatorial system of a rather unchemical sort, which lent it academic credibility. While other books had carried the title *Elements of Chemistry* or *Elementary Chemistry* before, the parallel with the works and approach of Euclid were never as clear as they were in Lavoisier's work. Looking at a table of the combinations of oxygen with the 'simple substances' or elements as determined by Lavoisier, we can see the combinatorial logic as it plays out across the page (see figure 1). Thus, the reader can see at a glance the results of the combination between each element and oxygen in varying degrees. Furthermore, the reformed language presents another compelling form for presenting this same combinatorial logic of the new chemistry. The pretension to raise chemistry up to a geometric if not mathematical science is quite clear.

In what remains of this paper, I want to explore two issues concerning the place of Lavoisier and his fellow 'new chemists' in this picture of disciplinary identity. Thus, returning to the parallel with Anderson's 'imagined communities', I want to pose the question of how chemists imagined themselves in this period. I also want to ask whether this approach adds anything significant to a less abstract vision of



Figure 1. "Table showing a series of compounds formed between simple substances and oxygen" from Antoine-Laurent Lavoisier *Traité élémentaire de chimie*, Paris, 1789.

Page 203. **TABEAU des Combinaisons binaires de l'oxygène avec les substances métalliques & non métalliques oxydables & acidifiables.**

	Premier degré d'oxygénation.		Second degré d'oxygénation.		Troisième degré d'oxygénation.		Quatrième degré d'oxygénation.	
	Noms nouveaux.	Noms anciens.	Noms nouveaux.	Noms anciens.	Noms nouveaux.	Noms anciens.	Noms nouveaux.	Noms anciens.
Combinaisons de l'oxygène avec les substances non métalliques, telles que :	Le gaz oxygène.....	Air vital ou dephlogistique.....			Acide nitrique.....	Acide nitreux.....	Acide nitrique oxygéné.....	Acide nitrique oxygéné.....
	L'hydrogène.....	On ne connaît qu'un degré de combinaison de l'oxygène & de l'hydrogène, & c'est combinaison forme de l'eau.			Acide nitrique.....	Acide nitreux.....	Acide nitrique oxygéné.....	Acide nitrique oxygéné.....
	L'azote.....	Oxyde blanc ou sale de gris.....			Acide carbonique.....	Acide carbonique.....	Acide carbonique oxygéné.....	Acide carbonique oxygéné.....
	Le carbone.....	Oxyde de carbone.....			Acide sulfurique.....	Acide sulfurique.....	Acide sulfurique oxygéné.....	Acide sulfurique oxygéné.....
	Le soufre.....	Oxyde de soufre.....			Acide phosphorique.....	Acide phosphorique.....	Acide phosphorique oxygéné.....	Acide phosphorique oxygéné.....
	Le phosphore.....	Oxyde de phosphore.....			Acide arsénique.....	Acide arsénique.....	Acide arsénique oxygéné.....	Acide arsénique oxygéné.....
	Le radical arsenique.....	Oxyde de radical arsenique.....			Acide manganique.....	Acide manganique.....	Acide manganique oxygéné.....	Acide manganique oxygéné.....
	Le radical boracique.....	Oxyde de radical boracique.....			Acide boracique.....	Acide boracique.....	Acide boracique oxygéné.....	Acide boracique oxygéné.....
	Le radical boracique.....	Oxyde de radical boracique.....			Acide boracique.....	Acide boracique.....	Acide boracique oxygéné.....	Acide boracique oxygéné.....
	Le radical boracique.....	Oxyde de radical boracique.....			Acide boracique.....	Acide boracique.....	Acide boracique oxygéné.....	Acide boracique oxygéné.....
Combinaisons de l'oxygène avec les substances métalliques, telles que :	L'antimoine.....	Oxyde gris d'antimoine.....			Acide antimonique.....	Acide antimonique.....	Acide antimonique oxygéné.....	Acide antimonique oxygéné.....
	L'argent.....	Oxyde gris d'argent.....			Acide argentique.....	Acide argentique.....	Acide argentique oxygéné.....	Acide argentique oxygéné.....
	L'indium.....	Oxyde gris d'indium.....			Acide indumique.....	Acide indumique.....	Acide indumique oxygéné.....	Acide indumique oxygéné.....
	Le bismuth.....	Oxyde gris de bismuth.....			Acide bismuthique.....	Acide bismuthique.....	Acide bismuthique oxygéné.....	Acide bismuthique oxygéné.....
	Le cobalt.....	Oxyde gris de cobalt.....			Acide cobaltique.....	Acide cobaltique.....	Acide cobaltique oxygéné.....	Acide cobaltique oxygéné.....
	Le cuivre.....	Oxyde rouge brun de cuivre.....			Acide cuivrique.....	Acide cuivrique.....	Acide cuivrique oxygéné.....	Acide cuivrique oxygéné.....
	L'étain.....	Oxyde gris d'étain.....			Acide stannique.....	Acide stannique.....	Acide stannique oxygéné.....	Acide stannique oxygéné.....
	Le fer.....	Oxyde noir de fer.....			Acide ferrugineux.....	Acide ferrugineux.....	Acide ferrugineux oxygéné.....	Acide ferrugineux oxygéné.....
	Le manganèse.....	Oxyde noir de manganèse.....			Acide manganique.....	Acide manganique.....	Acide manganique oxygéné.....	Acide manganique oxygéné.....
	Le mercure.....	Oxyde noir de mercure.....			Acide mercurique.....	Acide mercurique.....	Acide mercurique oxygéné.....	Acide mercurique oxygéné.....

group formation. This will lead to a consideration of the mechanisms behind disciplinary formation that might lie behind the rise of modern chemistry.

There is a significant dissimilarity between the situation described in *Chemistry, Pharmacy and Revolution* and the formation of new nations in the nineteenth and twentieth centuries. In the story of nationalism as it is recounted in the works of Anderson and others, imagined communities replace smaller more intimate 'real' communities constituted via direct social interaction. Thus, at the level of nations or incipient nations, large groups necessarily rely on mediation to compensate for the anonymity of their members. This means that such imagined communities are at once both a response to modern (mass) society and a phenomenon that relies on modern (mass) communication technologies (print media or wireless). A nation is composed of people who feel they share significant 'constitutive' experiences with their 'compatriots' rather than knowing them all personally. The difference is like that between a small club where everyone knows everyone else and has a more or less precise idea of what binds them together (shared interests, shared schooling, or some other commonality), and a large anonymous organisation where people share an identity, in this case national identity, but without personally knowing more than a few of the other members.

What is paradoxical in applying Anderson's approach in the context of Lavoisier's chemistry is that the people concerned, namely Lavoisier, Berthollet, Fourcroy, Chaptal, Guyton de Morveau, and Hassenfratz, among others, did all know each other. The new chemists constituted, initially at least, a small intimate community. Nevertheless, while there are clear dissimilarities between emergent nations and this early case of an incipient scientific disciplinary community, Anderson's idea provides a pertinent analytical approach that evidently needs to be adjusted as it is transferred. In order to develop and disseminate a new science of chemistry what these new chemists needed was not to know one another (they already did) but to share a common vision of what constituted chemistry. What I do in displacing Anderson's approach is to mobilise the idea of the 'imagined' discipline, as it were, to understand the conceptual constitution of the new chemistry rather than to explain the construction of a social disciplinary group. Thus, Lavoisier's oxygen theory and the accompanying reformed nomenclature were parts of a wider conception of theory-oriented chemistry that would transform the science in the nineteenth century. Analysis and Lavoisier's new list of (in principle, provisional) simple substances that resulted from it were also important ideals shared by the partisans of this new chemistry. By contrast, one does not have to invoke Anderson's theory to think about the constitution of disciplines. Making implicit reference to other nationalist theories, Mary Jo Nye in her treatment of



nineteenth-century French chemistry from 1993 proposed six elements for establishing a discipline, namely a genealogy, a core literature, codified rituals, a physical homeland, recognition from the outside and a shared set of scientific values.<sup>9</sup>

To close this short reflection on discipline formation, a return to the classic historical problem of causes will be fruitful. At the level of etiology, I do not want to suggest that the trigger for the constitution of an independent chemistry was chemists suddenly imagining that they could form a group apart around a shared 'ideal' of chemistry. The question is; if chemistry came into being as an independent discipline because a growing group of scientists shared a similar enough perspective, was this perspective defined by experimental practice, theoretical approach, raw materials or something else?<sup>10</sup> While I do not pretend to have a full answer to this question, I can suggest a reason why I don't have one. Thus, my historical response turns around a double periodization of the constitution of chemistry as a discipline. We need to consider two periods, with each one conditioned and oriented by the other.

First, there is the development of what I termed 'philosophical chemistry' across the long eighteenth century, a phenomenon that had no doubt already started in the second half of the seventeenth century. In this context we can pose the question of what were the mechanisms behind the development of the identity of chemistry in the eighteenth century. Several candidates need to be considered including the novel and stimulating chemistry of the airs, and, in particular, the growing popularity of chemistry among a wide and influential public with no direct professional interest in the science.

The second period is the nineteenth century, a period that is particularly important in the rise of 'academic' chemistry, which assumed a place as an independent discipline in the modern university during this later period. While *Chemistry, Pharmacy and Revolution* was largely about the first such period, a significant part of the work of imagining and constituting the 'imagined community' of modern chemistry falls into the second period. Thus, while in the book I started to present the role of journals, including the *Annales de chimie* and various pharmacy journals in this history, there is not sufficient follow-up concerning the development of these journals in the nineteenth century to form a clear idea of how precisely the successors of Lavoisier and Fourcroy constituted and reconstituted chemistry in the nineteenth century. It is in this period, that academically successful chemists constructed their hagiography, promoting Lavoisier to the head of the pantheon of great chemists. In the end, I would want to defend the historiographical hypothesis that the chemical revolution was definitively conceived

and described (in the form that we know it today) in the nineteenth and twentieth centuries from the perspective of 'modern' compositional chemistry.

Since publishing *Chemistry, Pharmacy and Revolution*, I remain thoroughly convinced of certain elements of the argument, but less convinced of others. Overall, the idea that the separation of chemistry from pharmacy was a significant transformation in both disciplines remains clear to me, along with the role played by the French revolution. But the question of the nature of the 'chemical revolution' remains open.

In the end, it is a well-worn philosophical issue that will divide historians over the interpretation presented in the present paper. What do you consider science to be? If you believe it is constituted by an operational set of theories putatively describing the physical world, then you will tend to favour a vision of the chemical revolution as a theoretical event that laid the foundations for modern chemistry. From here you can debate the relative significance of Boyle, Lavoisier or Dalton, but it is the theory, whether you consider it correct or simply adequate, that constitutes the most important factor. If, on the other hand, you believe science to be a certain form of organized human practice, then you will probably be more sympathetic to my vision (and periodisation) of the formation of chemistry as a modern science.

What is interesting from my perspective, however, is that stepping back and asking the big questions in history and philosophy of science, we return to the philosophical question underlying the debate over nationalism. Do nations reflect natural –ethnic, geographic, or other– realities, or are they cultural creations?

## Notes

<sup>1</sup> Jonathan Simon, *Chemistry Pharmacy and Revolution in France 1777-1809* (Aldershot: Ashgate, 2005)

<sup>2</sup> Herbert Butterfield, *Origins of Modern Science: 1300-1800* (New York: Macmillan, 1957).

<sup>3</sup> One only needs to note the number of biographies of Lavoisier that appeared to coincide with the anniversary of his death in 1995 to see that this single figure attracts a huge interest in the domain.

<sup>4</sup> The most influential historian of chemistry for me was probably Larry Holmes, in particular Frederic L. Holmes, *Eighteenth-Century Chemistry As an Investigative Enterprise* (Berkeley: Office for History of Science and Technology, University of California at Berkeley, 1989).

<sup>5</sup> James Riddick Partington, *A History of Chemistry*. 4 vols. (London: St Martin's Press, 1961-1970).

<sup>6</sup> Nicolas Lémery, *Cours de chymie* (Paris: Lémery, 1675), pp. 375-376. English translation from *A Course of Chymistry*. Trans. Walter Harris. (London: Kettilby, 1677).

<sup>7</sup> Marcellin Berthelot, *La révolution chimique : Lavoisier ; ouvrages suivis de notices et extraits des registres inédits de laboratoire de Lavoisier* (Paris: Félix Alcan, 1890) and Partington, *A History of Chemistry*.

<sup>8</sup> Eric Hobsbawm and Terence Ranger (eds) *The Invention of Tradition* (Cambridge: Cambridge University Press, 1983) and Benedict Anderson, *Imagined Communities*, (London: Verso, 1983).

<sup>9</sup> Mary Jo Nye, *From Chemical Philosophy to Theoretical Chemistry: Dynamics of Matter and Dynamics of Disciplines 1800-1950*, Berkeley: University of California Press, 1993.

<sup>10</sup> While the case of the founding of biochemistry described by Kohler is interesting in this respect, it concerns the formation of a sub-discipline within a discipline. Robert E. Kohler, *From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline* (Cambridge: Cambridge University Press, 1982).

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## Chemical Microbiology, an Interdisciplinary Field on the Road to Molecular Biology, 1920-1948

*Soňa Štrbáňová\**

Since the second half of the 19<sup>th</sup> century scientific development has been characterised among others by intense integration and differentiation of scientific disciplines resulting in the evolvement of new interdisciplinary sciences and fields. This is also true about chemistry which has interacted with numerous other disciplines and fields with serious consequences for the structure of scientific disciplines which was this way enriched with new interdisciplinary fields and sciences. Although a number of authors attempted to characterise and analyse the notions of “scientific discipline”, “interdisciplinary science” or “interdisciplinary field”,<sup>1</sup> historians of science still have not agreed on their generally acceptable definitions. This paper is aimed at contributing to this discussion using the example of emergence of chemical microbiology which became constituted between 1930 and 1950, approximately, on the boundary of several chemical and biological disciplines.

### From biochemistry to chemical microbiology

In order to understand the genesis of chemical microbiology it is necessary to recall a few particulars related to the formation of biochemistry, one of its “parent” interdisciplinary sciences, which dominated the scientific scene in the first half of the 20<sup>th</sup> century.<sup>2</sup>

As many historians have agreed, biochemistry became an independent discipline after 1900 with all necessary attributes: an institutional and communication base, an international scientific community, an independent subject taught in a number of universities, with a specific social mission and social acknowledgement and various strategic concepts outlining its program. The most significant for the future development of biochemistry, and will be seen, also for chemical microbiol-

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ogy, became especially the strategic programme of the new independent discipline published by the leading British biochemist F.G. Hopkins in 1913 in the journal *Nature* under the title “The Dynamic Side of Biochemistry”.<sup>3</sup>

The essential problems to be resolved by the young discipline, marked out by Hopkins, were amongst others:

- Cellular intermediary metabolism
- The nature and function of enzymes
- Mechanism of maintenance of the cellular dynamic equilibrium
- Cellular regulatory mechanisms
- Response of tissues and cells to chemical stimuli, including immunity reactions and drug action
- The role of cellular particles (structures) in these events
- Devising specific biochemical methods

It is obvious that focus on chemical approach to biological problems was the leit-motif to Hopkins’ program aiming in the first instance at resolving cellular chemical processes and their regulation. At the same time, he understood the cell as a general entity, a place where all crucial chemical processes took place.

Hopkins’ paper turned out to be the unifying agent of the various biochemical programmes presented earlier and also a certain guideline of biochemistry development for the next forty years to come. Hopkins invited chemists and biologists to participate in his agenda with a special appeal on organic and physical chemists, who in the 19<sup>th</sup> century had kept aloof from biological problems.

In 1914 was opened his Biochemical Department in Cambridge<sup>4</sup> where his vision was to be realised. It gradually developed into a major institute with international fame, the Dunn Institute of Biochemistry launched in 1924<sup>5</sup> where Hopkins’ collaborators<sup>6</sup> were making real the previously outlined programme of dynamic biochemistry. The important distinction of the Hopkins group was its interdisciplinarity and the research freedom given to its members. As pointed out by Kohler, “*Hopkins’ operating method was to hook ambitious young biochemists with the prospect of big biological problems to be solved, then to leave them alone to develop their special areas.*”<sup>7</sup>. Many of those who for a longer or shorter period joined the Department were to become leading figures of world biochemistry, among them Hans Krebs, J.B.S. Haldane, Albert Szent-Györgyi, Joseph and Dorothy Needham and many others. We will pay here special attention to Marjory Stephenson who joined the Cambridge Biochemical Laboratory in 1919.

## **Stephenson and Hopkins<sup>8</sup>**

Marjory Stephenson was a noteworthy personality not only because of her scientific achievements but also as one of the first women in a managerial position in a scientific institution and one of the first two women elected Fellows of the Royal Society.<sup>9</sup> She was born on January 24, 1885 at Burwell near Cambridge in an educated family of farmers. Both parents and Marjory's governess influenced the scientific and artistic interests of the young girl. After attending the Berkhamsted High School for Girls in 1897-1902, where she also received tuition in physiology, she enrolled in 1903 in Newnham College, Cambridge, where she took a Part I Natural Science Tripos in chemistry, physiology and zoology. Stephenson's scientific career started in 1911 when she became research assistant to the nutritional chemist R. H. A. Plimmer at University College, London. Nutrition and dietetics also became the bridge to her cooperation with F.G. Hopkins who got the Nobel Prize in 1929 for his early studies of vitamins. Paradoxically, when she arrived to Cambridge in 1919, Hopkins was no longer interested in vitamins. Dynamic or general biochemistry was the area he offered Stephenson in which to participate in his vision of biochemistry. Stephenson went on to accomplish one of the central points of his programme, which was research into the biochemistry of micro-organisms.

How did Stephenson come to such task? As already hinted, Hopkins had focused since the second decade of the 20<sup>th</sup> century on cellular biochemical processes and chemical approaches to clarifying them. He claimed that these processes were analogous in the cells of various organisms and if biochemists want to learn more about them they should study metabolism and its regulation in simple suitable models like unicellular organisms, preferably bacteria. This was an unexplored area and Stephenson was to develop it through research into bacterial metabolism. Encouraged by Hopkins, Stephenson devised her programme accordingly and pursued it during her whole life.

In 1922 Stephenson joined the Medical Research Council (MRC) and as such was not dependent anymore on the Biochemistry Department's budget. From 1929 she was for the rest of her life a full-time member of the MRC's staff at the newly established MRC research unit in Cambridge linked to the Dunn Biochemical Institute. She soon started to manage her own small research group. Under her guidance worked not only collaborators paid by the MRC but also graduate students, local and foreign visitors and several members of the Hopkins Department paid from various other sources (Table 1).

It is necessary to emphasise that a woman heading a research team was at that time an unusual phenomenon but the sources<sup>10</sup> bear witness to the fact that Stephenson coped with her managerial position in the laboratory and developing her scientific field, like most men scientists of the time holding a similar rank. However, the experience of a woman playing prominent role in the scientific community differed from that of a man. Although in reality she acted as director of the Cambridge MRC Laboratory with all attributes and responsibilities, the MRC management never recognised her position officially.

**Table 1**

**Collaborators of Marjory Stephenson 1922-1948<sup>11</sup>**

Collaborators at the MRC Research Unit	18
Members of the Biochemistry Department	21
Loosely attached collaborators	11
Others	6
Total number of collaborators	56
Number of women collaborators	14
Percentage of women collaborators	25

**Research pursued by Stephenson and her group<sup>12</sup>**

In the 1920s, studies of enzymes and metabolic phenomena in microbial cells were still at their beginnings and thus exploration of biological organisation of chemical reactions in cells and tissues (Table 2) represented a new original research direction.

**Table 2**

<i>Time interval</i>	<i>Topics</i>	<i>Main collaborators</i>
1920s	Washed cell or resting cell technique	Quastel, Whetham
	Notion of “active centres” in cell surfaces	Quastel, Whetham, and Wooldridge
1930s	Enzyme action in microbial cell	Cook, Whetham
1930s-1940s	Discovery, isolation and investigation of new bacterial enzymes	Stickland, Gale
1932-1936	Adaptation and control phenomena in bacterial cells	Yudkin, Gale
1930-1949	Bacterial Metabolism monograph	
WW2	New biotechnologies	
1946-1948	Nucleic acids in bacteria	



## **Main research topics in chemical microbiology pursued by Stephenson and her collaborators**

Since the early 1920s Stephenson with her direct collaborators and in cooperation with other members of the Biochemical Department, investigated various manifestations of metabolic activities in microorganisms, like the actions of bacterial enzymes. To accomplish such studies they also had to devise new methods. Among the most important ones was the washed cell or resting cell technique developed by Juda H. Quastel and Margaret D. Whetham;<sup>13</sup> this facilitated utilising bacteria as models of the cell for biochemical investigations. Bacterial cells were centrifuged out of culture, washed in water, suspended in a medium without nutrients and then used for experiments. Such a method made it possible to study chemical activities in whole intact living non-growing cells in contrast to the previous routines studying the contents of disrupted cells. Using the method various aspects of the metabolic activities in microbial cells, for instance actions of hydrogen-activating enzymes, were investigated. Several new enzymes were isolated and identified, the first being lactate dehydrogenase, in 1928.

The endeavour to investigate how chemical reactions in the cell are organised got its most pronounced expression in studies of enzyme adaptation phenomena.<sup>14</sup> In 1930, Stephenson and Leonard Stickland discovered a new enzyme, formic hydrogenlyase, produced by *Escherichia coli*. In the years 1932 to 1936 Stephenson with John Yudkin and Ernest F. Gale proved that this and several other enzymes, called adaptive enzymes, were formed in *E. coli* when a substrate of the enzyme is added to the growth medium. Such observations were not by then a novelty. Earlier, in the first two decades of the 20<sup>th</sup> century, various instances of such adaptive enzyme formation in multiplying cultures were investigated, but Stephenson and her collaborators were first to prove that also non-growing bacterial cultures and individual cells were capable of fast adaptive formation of substrate-specific enzymes. These findings prompted Stephenson and Yudkin to define in 1936 the *enzymic type of adaptation* as a “*direct a response of the enzymic composition of the cell to the constituents of the growth medium*”, independent of mutant formation or cell division, that “*is definitely temporary and does not affect the heredity mechanism of the cell, which reverts to normal ...when the organism is grown without the specific stimulus*”.<sup>15</sup> The first relevant so-called “mass action” theory of adaptive enzyme formation was then advanced in the years 1936 to 1938 by John Yudkin.

During World War II, Stephenson was involved among other things, in projects related to strategically important biotechnological production of organic compounds and this way contributed to the fast advance of biotechnology after WW2. In the last two years of her life, Stephenson investigated nucleic acids in bacteria and their enzymatic breakdown within the cell.<sup>16</sup>

These studies were interrupted by her illness and death by cancer on December 12, 1948.

### **Formation of a new field – criteria and circumstances**

Herein it is attempted to reason that in the years 1930-1948, approximately, a new interdisciplinary field chemical microbiology or general microbiology was emerging. Support to this statement relies on some criteria defining the process of the formation of a new interdisciplinary field or discipline specified earlier and explained in more detail in a paper on the formation of biochemistry as an interdisciplinary science.<sup>17</sup> The preferred criteria are the following:<sup>18</sup>

1. The process leading to the formation of an interdisciplinary science or field is complex and cannot be identified with an isolated event, like a discovery, creation of an institution or journal, a method and the like.
2. Interdisciplinarity is a historical notion connected with complex multistage interaction of various “parent” disciplines resulting in the formation of a qualitatively new field or discipline.
3. If speaking of an interdisciplinary science or fields one should always have in mind a customary term originating historically from interrelations of various scientific disciplines, fields and specialties.
4. To decide whether a new discipline integrating several parent disciplines has emerged one should settle on features which characterise a new interdisciplinary science or a field. The following are regarded as decisive:
  - New quality of cognition with methods specific to the new field.
  - Establishment of specialised institutional and communication network
  - Introduction of the field as a separate teaching subject at the university
  - Declaration of a concept and/or programme of the field or discipline and definition of its contents and objective of research.

Investigation of the question, how the process of the formation of chemical or general microbiology answered these criteria now follows.

### **New quality of cognition and specific methods in chemical microbiology**

It results from what has been said about Stephenson’s research that microorganisms represented for her both objects of research and a model enabling deeper

understanding of cellular events and their organisation in general, that is, not only in bacteria. She understood very soon that “*Data on the chemical activities of bacteria...may help us to gain an insight into the chemical processes accompanying the life of the organisms concerned...Perhaps bacteria may tentatively be regarded as biochemical experimenters...*”.<sup>19</sup> Her experiments demonstrating that bacterial enzymes behave similarly as enzymes in higher organisms and that metabolism and its control in bacteria was governed by regularities analogous to those in higher organisms, contributed to the acceptance of the principle of unity in biochemistry, coined by F.G. Hopkins and articulated by Kluver and Donker.<sup>20</sup> The experimental and theoretical approach to enzymatic adaptation (later known as enzymatic induction) elaborated in her laboratory in the 1930s was taken up by J. Monod in the 1940s and as such became the point of departure for theories of cellular regulatory mechanisms and protein synthesis developed in the 1950s and 1960s as a theoretical base for molecular biology. The methods developed in Stephenson’s laboratory, for instance the resting cell method, became standard methods when using micro-organisms as models in molecular biology.

### **Establishment of institutional and communication base in chemical or general microbiology**

Stephenson deliberately built her laboratory within the MRC as a centre of interdisciplinary studies related to her research programme in spite of a certain disapproval of the MRC authorities. Her attempts to retain her scientific autonomy used to be permanent source of argument with her superiors. From the very beginning Stephenson had to defend the direction of her research concentrating on the problems of chemical microbiology that were not directly related to the MRC’s scheme, in particular to practical medical problems. This is evident for instance from the letter the Secretary of MRC Fletcher wrote in 1931, where he urged Stephenson to reduce the “*purely abstract biochemical point of view*” with a threat that the MRC will only support investigations which are “*likely to assist the progress of medicine*”.<sup>21</sup> This determined clinging on the research programme, she considered focal, might have been one of the reasons why the Cambridge MRC laboratory was never given the official status of an MRC Unit.<sup>22</sup> Its interdisciplinarity was accentuated by the fact that under Stephenson’s guidance there worked in the laboratory specialists of diverse backgrounds; not only collaborators paid by the MRC but also graduate students, local and foreign visitors and several members of the Hopkins Department paid from different sources.

Stephenson was fully aware of the fact that the new field must be anchored in a specialised institution also *de iure*, therefore she herself exerted pressure on the MRC to legitimise the laboratory as an official unit for microbiological chemistry, however without success. Since 1944 in her annual reports she started to call her workplace “*The M.R.C. Research Unit for Microbiological Chemistry, The Biochemical Laboratory Cambridge*” and had the same name painted on the door in spite of the persistent deprecation of the MRC leaders to officially recognise such status. It was a special success that must be attributed to Stephenson’s persistent efforts that the MRC appointed during WW2 a special *Committee on Chemical Microbiology* where Stephenson was invited for membership. Ironically, the unofficial name of the laboratory became legalised by the MRC immediately after her death in 1948 when E.F. Gale was appointed Director of the Unit for Chemical Microbiology.

Another momentous event in institutionalisation of chemical or general microbiology represented the creation of the *Society for General Microbiology*. Although its establishment had been prepared by an organising committee under the leadership of M. Stephenson since 1943, the formal inaugural meeting was only held on February 16, 1945. As the records show, the first candidate for presidency was the biochemist David Keilin, and when he declined Stephenson was asked to take this post.<sup>23</sup> Nevertheless, Stephenson did not accept the position most possibly due to her political tactfulness and/or serious illness<sup>24</sup> so eventually Sir Alexander Fleming was elected. We should observe that the society omitted the term “chemical” in its title probably because the Society aimed at transcending the realm of chemical microbiology and drew up its program more generally. The inaugural meeting “*decided that the Society should concern itself with the study of bacteria, viruses, micro-fungi, protozoa and microscopic algae in their various biological activities*” dealing “*predominantly with the more fundamental aspects of the study of these forms, including their physiology, nutrition, chemotherapy, systematics and ecology*”.<sup>25</sup> The chemical aspects were still there but hidden in the terms physiology, nutrition, chemotherapy. In 1947, Stephenson eventually agreed to accept the presidency and Fleming became Honorary Member. The same year the *Journal of General Microbiology* was founded. Its contents show that the chemical direction in the journal has been predominant and the majority of papers were related to biochemistry and molecular biology.<sup>26</sup> In memory of Marjory Stephenson the Society of General Microbiology awards biennially the Marjory Stephenson Prize Lecture “*for any outstanding contribution of current importance in microbiology*”.<sup>27</sup>

Another means of specialised communication and institutionalisation in the new field was the *First International Symposium on Chemical Microbiology* which took place in 1951 under the aegis of the World Health Organization,<sup>28</sup> to celebrate the opening of the *International Research Center for Chemical Microbiology* in Rome.<sup>29</sup>

### **Chemical microbiology in university education**

Attempts to establish chemical microbiology as a separate interdisciplinary field also included university education. Stephenson worked hard for several years to start a special Part II Biochemistry (microbiological) in Cambridge. Eventually in 1947 chemical microbiology was recognised by the University as a discipline in its own right and she herself was appointed the first University Reader in Chemical Microbiology.<sup>30</sup> As explained the letter of H.H. Dale<sup>31</sup> to the University's Vice Chancellor H. Thirkill written in 1947, the introduction of the new discipline at the University had been motivated among other things by the great and growing national need "...for scientists who have had a training in the fundamental sciences suitable to equip them for research in the general field of microbiology, with its growing range of technical applications". Stephenson's success as a teacher could "be measured by the steady flow of recruits from the Part II Class to her research team...".<sup>32</sup>

### **The concept and programme of chemical microbiology**

The strategic program of chemical or later general microbiology was designed and communicated by Stephenson on several occasions, especially in her monograph and textbook *Bacterial Metabolism* published in three revised editions, 1930, 1939 and 1949.<sup>33</sup> Written in a "lucid and forceful style", as characterised by the prominent British biochemists Elsdon and Pirie,<sup>34</sup> it became a reference work for several generations of biochemists and microbiologists all over the world. From the prefaces to the three editions one can follow the development of Stephenson's programmatic vision of chemical microbiology over the years.

Already in 1929<sup>35</sup> in the Preface to the 1930 Edition, Stephenson stressed the importance of data "on chemical activities of bacteria which may help us to gain an insight into the essential chemical processes accompanying the life of the organisms concerned...and to appraise our knowledge of bacteria as living organisms apart from their rôle as disease germs or the bearers of commercially impor-

*tant catalysts.*"<sup>36</sup> In the second edition of 1938 Stephenson observed the necessity of bacterial biochemistry in determining the common laws followed by enzymes belonging to the bacterial, animal and vegetable world.<sup>37</sup> The preface to the third edition 1949 is almost prophetic in its view of study of bacterial metabolism and its ever more extensive application: "...During the last few years a fresh view of bacterial metabolism has been opened up. Information is now being rapidly gained on the course of the biochemical processes leading to cell synthesis; such studies are peculiar to microbiology though certainly of wider application.; they owe their success to use of biological material which is prone to biochemical variation and tolerant of interference with its normal biochemical habit [that is microorganisms, comment by SŠ]. This new stream of knowledge has its origin in several sources: microbial genetics, nucleic acid metabolism, adaptive enzyme formation...antibiotics...and interference with metabolism resulting from the introduction into the cell of chemical analogues of essential cell metabolites. All these are contributing to produce a picture –at present incomplete and patchy– of the biochemical machinery of growth".<sup>38</sup>

The programme of chemical microbiology grew into its more definite and extended shape of general microbiology as explained in Stephenson's plenary lecture "*Levels of Microbiological Investigation*"<sup>39</sup> read at the inaugural meeting of the Society for General Microbiology in 1945. In the lecture Stephenson defined areas in which research in microbiology should be undertaken, none of which should be considered higher or lower than another in the list:

- (1) Mixed cultures of organisms growing in natural environment,
- (2) Pure growing cultures in complex media,
- (3) Pure growing cultures in highly purified chemically defined media,
- (4) Non-proliferating cells in pure cultures containing chemically defined substrates, and
- (5) Cell-free enzymes and coenzymes and their action on pure substrates.

This programme was exceptional from several standpoints. It was designed in a more universal way than that of chemical microbiology and invited a much wider scientific audience. It stressed not only chemical investigations of cells but also appealed to interdisciplinary collaboration of scientists experimenting at different levels of living matter; from naturally occurring mixed cultures to cell-free environment. It also called attention to the fact that only studying cells at various levels may result in a complex knowledge of the cell's activities. And last but not least it pleaded for better understanding of bacteria as they are found in Nature, a view that has been neglected both by biochemistry of the 1950s and contemporary molecular biology, preferring areas (3-5) and almost completely ignoring area (1).

Stephenson's proposal of problem-solving in area (1) may also be understood to a certain extent as gender-related.<sup>40</sup> To explain this view it is worth to note, beforehand in 1937 she had called attention to the fact that enzymes studied in the laboratory may behave quite differently than those found in nature. In her essay on cell organisation pleading against simple teleology, Stephenson calls for investigation of live objects, even as small as microorganisms, in their natural environment.<sup>41</sup> It is her respect for the integrity of Nature which can be attributed to her gender and which is at odds to the usual scientists' reductionist stereotype and efforts to study Nature through domination and disintegration.

## Conclusions

Robert Kohler in his paper of 1985<sup>42</sup> evaluates Stephenson's contribution as a "program" of "innovation" in "normal science" namely innovation in bacterial physiology.<sup>43</sup> He states: "*Stephenson's program for bacterial physiology was a mixture of Cambridge-style enzymology, comparative physiology and evolutionary biology*". The present paper considers Stephenson's contribution to discipline building to be more substantial than a mere innovation programme within a previously existing field or discipline. Its essence is in a complex formation of a new research field chemical or general microbiology with momentous consequences for the structure of scientific disciplines in the 20<sup>th</sup> century as it represents an intermediate link between biochemistry and what we call today, molecular biology.

This paper endeavours to show that chemical microbiology, or as it was called later general microbiology, was a new interdisciplinary research field which evolved stepwise in the years 1930-1948. Among its "parent" disciplines were microbiology, biochemistry, bacterial physiology, physical and organic chemistry. Chemical microbiology acquired the features of a genuine discipline with its well-defined subject and objective of research including special methods, offering its specific strategic programme and building its institutional, publication and communication base; it became a university education subject, and encompassed its institutionally anchored scientific community and adequate social acknowledgement. However, the terms "general microbiology" or "chemical microbiology" appeared much earlier, at the times before they could be identified with a new field. Already in 1910 the German microbiologist Walther Kruse whom Stephenson used to quote as her predecessor, published a monograph by entitled "*Allgemeine Mikrobiologie*",<sup>44</sup> and E.B. Chain even considered Pasteur founder of chemical microbiology.<sup>45</sup>



Although there is still much confusion in defining scientific disciplines and their formation, attention is drawn to the recent paper of H. Laitko<sup>46</sup> who considers one of the “most important perspectives of science research” the one “related to the disciplinary dimension of science that divides science into units dependent upon the difference of subject areas...Corresponding to the multitude of possible initial arrangements a lot of different discipline formation patterns may be described as well as supra-disciplinary types of units like specialties, research areas, etc. which “may be transformed historically into genuine disciplines”. For several reasons, chemical or general microbiology should not be considered according to this terminology discipline but rather as a field of a supra-disciplinary type which played the role of an intermediate stage on the road from biochemistry to the new discipline - molecular biology.

## Notes

<sup>1</sup> See for instance *Der Ursprung der modernen wissenschaften*, ed. Martin Guntau and Hubert Laitko (Berlin: Akademie Verlag, 1987); *Die Entstehung biologischer Disziplinen vol. 1*, eds. Ekkehard Höxtermann, Joachim Kaasch and Michael Kaasch, *Verhandlungen zur Geschichte und Theorie der Biologie* (Berlin: VWB, 2002), vol. 8; *Die Entstehung biologischer Disziplinen vol. 2*, eds. Uwe Hossfeld and Thomas Junker, *Verhandlungen zur Geschichte und Theorie der Biologie* (Berlin: VWB, 2002), vol. 9. For the analysis of the formation of biochemistry as an interdisciplinary science, see e.g. Soňa Štrbáňová, “Formation of Interdisciplinary Sciences. The Case of Biochemistry,” in *Science and Technology at the Turn of the 19th and 20th Centuries: Interrelation of Disciplines*, eds. Luboš Nový and Yuri S. Voronkov, *Acta historiae rerum naturalium necnon technicarum* 19 (1985), 193-245, this paper offers detailed reasoning with references on the formation of interdisciplinary sciences and biochemistry; Soňa Štrbáňová, “Forming the Strategy of 20th Century Biochemistry,” in *Highlights of Modern Biochemistry*, eds. Arnošt Kotyk, Jan Škoda and Václav Pačes (Utrecht, Tokyo: VSP International Science Publishers, 1989), 1689-1697.

<sup>2</sup> For recent treatise on the history of biochemistry see especially Joseph H. Fruton, *Proteins, Enzymes, Genes. The Interplay of chemistry and Biology* (New Haven and London: Yale University Press, 1999); it contains a substantial bibliography on the history of biochemistry and related disciplines and fields.

<sup>3</sup> Frederick G. Hopkins, “The Dynamic Side of Biochemistry,” *Nature* 92 (1913): 213-223.

<sup>4</sup> For details on Hopkins’ biochemistry department see e.g. *Hopkins and Biochemistry*, eds. Joseph Needham and Ernest Baldwin (Cambridge: Heffer, 1949); Joseph Needham, “Frederick Gowland Hopkins,” *Perspectives in Biology and Medicine* 6 (1962): 1-46; Robert E. Kohler, *From Medical Chemistry to Biochemistry* (Cambridge, London, New York, New Rochelle, Melbourne Sidney: Cambridge Univ. Press, 1982), 73-92.

<sup>5</sup> Robert E Kohler, “Walter Fletcher, F.G. Hopkins, and the Dunn Institute of Biochemistry: A Case Study in the Patronage of Science,” *Isis* 69 (1978): 331-355.

<sup>6</sup> By 1950 passed through Hopkins’ laboratory total about 370 people from all over the world, as shown by Soňa Štrbáňová, “Marjory Stephenson and the Medical Research Council – a New

Managing Role for a Woman Scholar,” in *Women Scholars and Institutions*, eds. Soňa Štrbáňová, Ida H. Stamhuis, Kateřina Mojsejová, *Studies in the History of Sciences and Humanities*, (Prague: Research Centre for the History of Sciences and Humanities, 2004), vol. 13 A, 415-449, on 422.

<sup>7</sup> Robert E. Kohler, “Innovation in Normal Science. Bacterial Physiology,” *Isis* 76 (1985): 162-181, on 166.

<sup>8</sup> For Stephenson’s life and work and her relation to Hopkins, see Štrbáňová, “Marjory Stephenson”.

<sup>9</sup> Stephenson’s managerial position at the MRC is analysed in detail in Štrbáňová, “Marjory Stephenson”. For her Fellowship of the Royal Society see Muriel Robertson, “Marjory Stephenson (1885-1948),” *Obituary Notices of Fellows of the Royal Society* 6 (1948/49): 563-577; Joan Mason, “The Admission of the First Women to the Royal Society of London,” *Notes and Records of the Royal Society* 46 (1992): 272-300.

<sup>10</sup> Ample evidence to this assertion is presented in Štrbáňová, “Marjory Stephenson”.

<sup>11</sup> The table was taken from Štrbáňová, “Marjory Stephenson,” 447.

<sup>12</sup> Facts in this paragraph are taken from literature on Stephenson, especially Sidney R. Elsdon and Norman W. Pirie, “Marjory Stephenson 1885-1948,” *Journal of General Microbiology* 3 (1949): 329-339; Muriel Robertson, “Marjory Stephenson”; Donald D. Woods, “Marjory Stephenson 1885-1948,” *Biochemical Journal* 46 (1950): 376-383; Robert E. Kohler, “Stephenson, Marjory,” in *Dictionary of Scientific Biographies* 18, Suppl. II (1981): 851-860; Mark Weatherall, and Harmke Kamminga, *Dynamic Science* (Cambridge: Wellcome Unit for the History of Medicine, 1992), 43, 48-49; Soňa Štrbáňová, “Stephenson, Marjory,” in *Lexikon der Bedeutenden Naturwissenschaftler*, eds. Dieter Hoffmann, Hubert Laitko and Staffan Müller-Wille (München: Elsevier Spektrum Akademischer Verlag, 2004), vol. 3, 326-327.

<sup>13</sup> See e.g. Juda H. Quastel and Margaret D. Whetham, “The Equilibria Existing Between Succinic, Fumaric and Malic Acids in the Presence of Resting Bacteria,” *Biochemical Journal* 18 (1924): 519.

<sup>14</sup> See Soňa Štrbáňová, “Enzyme adaptation - the road to its understanding: Early theoretical explanation,” in *Biology Integrating Scientific Fundamentals. Contributions to History of Interrelations Between Biology, Chemistry and Physics from the 18th to the 20th Centuries*, ed. Brigitte Hoppe, *Algorismus* (München: Institut für Geschichte der Naturwissenschaften, 1997), vol. 21, 260-285. In this paper are described the experiments and their theoretical outcomes with references.

<sup>15</sup> Both quotes are from Marjory Stephenson, *Bacterial Metabolism*, 3<sup>rd</sup> Edition (London, New York, Toronto: Longmans Green, 1930), 296.

<sup>16</sup> Robertson, “Marjory Stephenson,” 573.

<sup>17</sup> Štrbáňová, “Formation of Interdisciplinary Sciences”, see especially pages 194-195 and 227-231.

<sup>18</sup> I have chosen these criteria, although other ones may exist that would lead to different conclusions.

<sup>19</sup> Marjory Stephenson, *Bacterial Metabolism* (London-New York-Toronto: Longmans Green, 1930), xi.

<sup>20</sup> Albert J. Kluyver and Hendrick J.L. Donker, “Die Einheit in der Biochemie,” *Chemie der Zelle und Gewebe* 13 (1926): 134-190.

<sup>21</sup> Letter, Fletcher to Stephenson, February 2, 1931, MRC Archives 2036/2/I.

<sup>22</sup> The other reason was obviously the mere fact that Stephenson was a woman, compare Štrbáňová, “Marjory Stephenson and the Medical Research Council”.

<sup>23</sup> Copy of letter to Stephenson from Honorary Secretaries, (apparently L.A. Allen and R. St. John-Brooks), 13 July 1944. Archives of the Society for General Microbiology, Reading. The other candidate for presidency was according to this letter D. Keilin.

- <sup>24</sup> In September 1944 Stephenson discovered a tumour in her right breast and underwent an operation. Letter Stephenson to Mellanby 21 September 1947, MRC Archives, P.F.216.
- <sup>25</sup> "Society for General Microbiology", *Nature* 155 (1945): 340.
- <sup>26</sup> Both the Society and the Journal exist until today.
- <sup>27</sup> See [http://www.socgenmicrobiol.org.uk/about/prize\\_lectures\\_rules.cfm](http://www.socgenmicrobiol.org.uk/about/prize_lectures_rules.cfm)
- <sup>28</sup> *Microbial Growth and its Inhibition. First International Symposium on Chemical Microbiology, World Health Organization Monograph Series* (Geneva: W.H.O., Palais des Nations, 1952), No. 10.
- <sup>29</sup> I have this information from the internet, see <http://www.ajtmh.org/cgi/content/abstract/2/5/944-a>, <http://veterinaryrecord.bvapublications.com/cgi/content/citation/63/25/435> and have no other knowledge about this centre as well as about the international symposia on chemical microbiology and their further fate. Nevertheless even these data suggest that for some time chemical microbiology was considered a very important field.
- <sup>30</sup> Complicated negotiations concerning the establishment of the new subject apparently started at the beginning of 1946 as reflected by the correspondence reprinted in Memorandum for the meeting of the MRC General Board. General Board Paper No. 2064, MRC Archives 2036/2/III.
- <sup>31</sup> H.H. Dale was at that time President of the Royal Society and member of the Scientific Advisory Committee to the Cabinet.
- <sup>32</sup> Woods, "Marjory Stephenson," 378.
- <sup>33</sup> Marjory Stephenson, *Bacterial Metabolism* (London-New York-Toronto: Longmans Green, 1930); 2<sup>nd</sup> Edition, 1939; 3<sup>rd</sup> Edition, 1949.
- <sup>34</sup> Elsdon and Pirie, "Marjory Stephenson," 335.
- <sup>35</sup> Date of Stephenson's Preface to the First Edition is March 8<sup>th</sup>, 1929, so she wrote it the year before the book was published.
- <sup>36</sup> Stephenson, *Bacterial Metabolism* (1930), xi.
- <sup>37</sup> Stephenson, *Bacterial Metabolism* (1939), viii-ix.
- <sup>38</sup> Stephenson, *Bacterial Metabolism* (1949), v-vi.
- <sup>39</sup> The text of the lecture has not been published. Information on its contents comes from Woods, "Marjory Stephenson," 378-379; Elsdon and Pirie, "Marjory Stephenson," 331; and the article reporting on the inaugural meeting, see "Society for General Microbiology".
- <sup>40</sup> For various aspects of gender-related science see Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven, London: Yale University Press, 1985); Section "Gendered Knowledge and the Reshaping of the 'Normal Science'", in *Women Scholars and Institutions*, ed. Soňa Štrbáňová, Ida H. Stamhuis, Kateřina Mojsejová, *Studies in the history of sciences and humanities*, (Prague: Research Centre for the History of Sciences and Humanities, 2004), vol. 13 B, 727-809.
- <sup>41</sup> Marjory Stephenson, "The Economy of the Bacterial Cell", in *Perspectives in Biochemistry*, eds. J. Needham and D.E. Green (Cambridge: At the University Press, 1937), 91-98.
- <sup>42</sup> Kohler, "Innovation", 170-171.
- <sup>43</sup> Kohler, "Innovation", 171.
- <sup>44</sup> Walther Kruse, *Allgemeine Mikrobiologie: die Lehre vom Stoff-und Kraftwechsel der Kleinwesen* (Leipzig: Verlag von F. C. W. Vogel, 1910).
- <sup>45</sup> E.B. Chain, "Contributions from Chemical Microbiology to Therapeutic Medicine," *Proceedings of the Royal Society of Medicine* 58 (1965): 85-96.
- <sup>46</sup> Hubert Laitko, "Die Disziplin als Strukturprinzip und Entwicklungsform der Wissenschaft – Motive, Verläufe und Wirkungen von Disziplinengenesen," in *Die Entstehung biologischer Disziplinen 1*, eds. Ekkehard Höxtermann, Joachim Kaasch and Michael Kaasch, *Verhandlungen zur Geschichte und Theorie der Biologie* (Berlin: VWB, 2002), vol.8, 19-55.

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# The Development of Organic Chemistry in Japan: Riko Majima and His Research School of “Natural Product Chemistry” in the First Half of the Twentieth Century

Masanori Kaji\*

## 1. Chemistry before the Meiji Restoration

In Japanese history, the year 1868 is usually considered to be the beginning of modern Japan. In that year, the Tokugawa government, ruled by the shoguns of the Tokugawa family, based in Edo (today's Tokyo) was replaced by a modern government ruled by an Emperor, who was formerly confined to Kyoto. This revolutionary political change is called the Meiji Restoration, because the ancient Imperial system was nominally restored under Emperor Meiji. Although initially mixed in its leanings, the new government soon adopted a policy of full-fledged modernisation.<sup>1</sup>

The introduction of western science had already started long before the Meiji Restoration. From the middle of the 17th century until 1853, the Netherlands was the only European country with which Japan traded; some books in Dutch on science, technology, and medicine were imported into Japan during that period. In the 1770s, some Japanese medical doctors, including Genpaku Sugita (1733-1817), Ryoutaku Maeno (1723-1803), Junan Nakagawa (1739-86), Hoshu Katsuragawa (1751-1809), began to learn Dutch and translated a Dutch introductory anatomy textbook, *Ontleedkundig Tafelen*,<sup>2</sup> into Japanese. This was the beginning of the so-called “Dutch learning (*Rangaku*),” that is, learning of about western civilisation through the Dutch language.<sup>3</sup>

By the 1830s-40s, Yoan Udagawa (1798-1846), a scholar of Dutch learning, had already written a textbook “Seimi Kaiso [An Introduction to Chemistry]” –basically Lavoisier's chemistry– using contemporary Dutch chemistry textbooks, including the Dutch translation of Lavoisier's *Traité Élémentaire de Chimie*.<sup>4</sup>

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In 1854, after the feudal government abandoned its policy of tight control over foreign trade, the political situation became destabilised. During the political turmoil and colonisation threats, some intellectuals who belonged to the samurai, the military ruling class, began to study western science and technology, including chemistry and chemical technology, as militarily useful subjects.

During this period scholars studied western knowledge not only in Dutch, but other in other European languages such as English, French and German. They were called scholars of Western learning. Some of them, who were interested in chemistry, followed its development in 19<sup>th</sup> century, including Dalton's atomic theory, which Udagawa did not pay attention in his textbook. Komin Kawamoto (1810-71), a teacher of chemistry at a governmental school of Western learning, was such a scholar and translated a number of newer chemistry textbooks and wrote a textbook based on Dalton's atomic theory.

## **2. Institutionalisation of Chemistry in Japan: Joji Sakurai and Organic Chemistry<sup>5</sup>**

The institutionalisation of science, including that of chemistry, started after the Meiji Restoration. Then the modern Western education system was introduced to Japan, and many foreign teachers were employed by the new government to teach in higher education. Even though most children studied only up to the elementary education level, some middle- and upper-class children, including those of the former samurai class, studied western languages from an early age and continued their studies at higher educational institutions in which westerners taught. The Meiji government sent the most successful students to Europe and the United States to study further. When they returned, they replaced the foreign teachers to become the first generation of Japanese professors to teach in the new system.

Joji Sakurai (1858-1939) was a chemist who belonged to the first generation of Japanese scientists. He was born in 1858 to a samurai family in Kanazawa, the capital of one of the most powerful feudal lords. He started to learn English at an early age and entered an institution of western learning, founded by the Ministry of Education, in 1871. He studied in the three-year preliminary course and the two-year specialist chemistry course under the English chemist, Robert William Atkinson (1850-1929). Then, he was sent to England and studied at University College London between 1876 and 1881 under a famous chemist and Atkinson's teacher, Alexander William Williamson (1824-1904).

When Sakurai returned to Japan in 1881 at the age of 23, he obtained a teaching position as a lecturer in the Faculty of Science at Tokyo University as the successor of his mentor, Atkinson, and became a professor the next year. He was the second Japanese professor of chemistry after the American-trained chemist Naokichi Matsui (1857-1911), who had been appointed a year earlier.

With the foundation of the Imperial University in Tokyo in 1886, the education system in Japan was fully established. Sakurai became the Head of the Department of Chemistry at the College of Science at the Imperial University. Sakurai taught organic chemistry as well as physical and theoretical chemistry. His lectures, especially those on organic chemistry, were highly praised by students, even though Sakurai researched in physical chemistry rather than in organic chemistry.

### **3. Riko Majima and the Beginning of Research in Organic Chemistry<sup>6</sup>**

One of Sakurai's students, Riko (Toshiyuki) Majima (1874-1962), was the first organic research chemist in Japan. Majima was born in Kyoto in 1874, the eldest son of an affluent medical doctor. His generation was the first to study fully within a modern educational system. Majima entered the Imperial University in Tokyo ten years after its establishment. Kyoto Imperial University was established the next year. Soon after his graduation in 1899, he remained in the department as a research assistant and a graduate student of the department under the supervision of professor Sakurai. Sakurai gave him total freedom in choosing his research topics, but Majima felt insecure because Sakurai gave no advice on research. Since there was no one to provide advice on research in organic chemistry, he studied well-known German organic chemists' work that had been published in German journals. When he started to conduct research in organic chemistry, he decided to study local natural products that could not be obtained easily in Europe in order to be able to compete with chemists in the West.<sup>7</sup> He first studied the structure of urushiol, the main component of the sap of the Japanese lacquer tree (*Rhus verniciflua* Stokes, *urushi-no-ki* in Japanese). The black glossy varnish is sometimes known as japan, the lacquer tree is an important indigenous commercial source of natural lacquer.

In 1903, Majima was promoted to the position of associate professor. Soon the Ministry of Education sent him to Europe for further study. He conducted research in Kiel under Carl Dietrich Harries (1866-1923) and in Zurich under Richard Willstätter (1872-1942). While in Europe, besides his research on the top-

ics provided by his European supervisors, he continued to study urushiol, using advanced instruments available in the laboratories.

Majima returned to Japan in January 1911 and became a Professor of Organic Chemistry at Tohoku Imperial University, a newly established imperial university in Sendai in northern Japan, in March. After establishing his laboratory, he restarted his research on the structure of urushiol, using newly introduced advanced instruments and methods from Europe, such as highly reduced pressure distillation and a new method of catalytic reduction, Harries ozonolysis. Within six years, he succeeded in elucidating the structure of urushiol as a catechol (o-dihydrobenzene) derivative.

Majima's study of urushiol and local natural products using newly developed European methods was a major research strategy in Japan until the 1960s. Many of his students followed this line of research.

#### **4. Majima's Research School and Tetsuo Nozoe: From Natural Product Research to a New Field of Organic Chemistry**

Majima, as a leader of the first generation of organic research chemists, contributed greatly to the establishment of organic chemistry laboratories in higher education institutions as well as in research institutes in Japan including the Imperial Universities at Sendai and Osaka, and the Tokyo Institute of Technology, and a laboratory at the Research Institute of Physical and Chemical Sciences (RIKEN).

Tetsuo Nozoe was one of Majima's students at Tohoku Imperial University, and his research path showed how Majima's research line was developed by his students.<sup>8</sup> Nozoe was born in Sendai in 1902 as the sixth child of a lawyer and local politician. He entered the Department of Chemistry of the Faculty of Science at Tohoku Imperial University in 1923. At the university, Majima became Nozoe's mentor and was to play a decisive role in his life.

After his graduation in March 1926, Nozoe stayed on as Majima's assistant. However, at the end of June 1926, Nozoe left Sendai for Formosa (now Taiwan) to become a researcher at the Monopoly Bureau in Taipei, the capital of Formosa, with Majima's strong recommendation. Nozoe was a candidate for a Professorship at a planned new Imperial University in Formosa. In 1928, Taihoku Imperial University (Taihoku is the Japanese name for Taipei) was established. Young Nozoe was appointed an associate professor the following year. Taihoku Imperial



University was the second Japanese Imperial University in its colonies after Keijo Imperial University in Keijo (now Seoul), founded in 1925. Nozoe's main research interest was the study of natural products, especially those found in Formosa.

Nozoe's well-known work in Formosa concerned the chemical components of *taiwanhinoki* (*Chamaecyparis obtusa* var. *formosana*, now *C. taiwanensis*), a native conifer that grew in high mountainous areas. Nozoe obtained a new compound, named hinokitiol, from the components and reported it for the first time in 1936 in a special issue of *The Journal of the Chemical Society of Japan* to celebrate Professor Majima's sixtieth birthday.

By studying hinokitiol's structure, he arrived at the idea that it could be a new type of aromatic compound stabilised by resonance, involving an intramolecular hydrogen bond, after reading Linus Pauling's *The Nature of the Chemical Bond* (1939), which was probably one of the last academic publications imported into Formosa before World War II started. Even though it turned out later that hinokitiol existed not as a resonance hybrid, but as a pair of tautomers that interconverted through intramolecular hydrogen bonding, this idea was the first step in opening the new research area of non-benzenoid aromatic compounds. Nozoe presented his ideas for the first time at a local meeting of chemists in Taiwan in 1941, but the audience was skeptical about his seven-membered structure.

After World War II, Formosa was returned to the Republic of China and Taihoku Imperial University was renamed Taiwan National University. Most Japanese left Taiwan for Japan, but Nozoe, who had to stay in Taiwan, worked as a Professor of Chemistry at Taiwan National University under the orders of the Chinese government, which needed such specialists.

Since Nozoe regarded hinokitiol as a compound with a novel aromatic system, he examined various substitution reactions: halogenation, nitration, and azo coupling. He managed to return to Japan at the end of May 1948. His *alma mater*, Tohoku University, offered him a position. The results of his group's research on hinokitiol at National Taiwan University were first published in 1949 in a Japanese journal.<sup>9</sup> His group in Tohoku University began to produce many papers on chemistry of hinokitiol and its derivatives in English, first in the Proceedings of the Japan Academy and then other journals.<sup>10</sup>

In 1945, Michael J.S. Dewar (1918-97), later a leading theoretical chemist, proposed a new kind of aromatic structure with a seven-membered ring for which he coined the term "tropolone". At the end of 1948, Nozoe received a copy of a letter written by Holger Erdtman (1902-89), a Professor of Organic Chemistry at the Royal Institute of Technology in Stockholm, along with a reprint of Erdtman's

paper on the structure of thujaplicins, published in *Nature* in 1948, from his Japanese colleague at the Faculty of Pharmacy at the University of Tokyo, who had once worked under Professor Robert Robinson at Oxford along with Erdtman. Erdtman had isolated three isomeric monoterpenoids (named the  $\alpha$ -,  $\beta$ -,  $\gamma$ -thujaplicins) from the Western red cedar (*Thuja plicata*). Erdtman and Nozoe corresponded and discovered that hinokitiol was identical to  $\beta$ -thujaplicin.

When a symposium, “Tropolone and Allied Compounds”, was organized by the Chemical Society of London in November 1950, Erdtman mentioned Nozoe’s work on hinokitiol as a pioneering contribution to tropolone chemistry, thus helping Nozoe’s research to gain recognition in the West. Nozoe was able to publish his work on hinokitiol and its derivatives in *Nature* in 1951 thanks to J. W. Cook, the chairman of the symposium.<sup>11</sup>

Nozoe’s work, which started with natural products research in Taiwan and developed fully in Japan in the 1950s and 60s, opened a new field of organic chemistry, the chemistry of non-benzenoid aromatic compounds. His work was good example of how Majima’s students developed his line of research and demonstrated that organic chemistry research in Japan had reached a world-class level by the 1950s.

## Notes

<sup>1</sup> See, for example, Andrew Gordon, *A Modern History of Japan: From Tokugawa Times to the Present* (New York and Oxford: Oxford University Press, 2003).

<sup>2</sup> This was the Dutch translation in 1734 by Gerrit Dickten, a Dutch surgeon, from Johann Adam Kulmus’s small anatomy textbook, *Anatomische Tabellen*. Kulmus (1689-1745) was a teacher of medicine and physics in a Gynasium in Danzig (Gdańsk) and wrote the anatomy book in German in 1722 and his book ran several editions in German and was translated into Latin, French and Dutch. See Sumio Ishida, *Oranda ni okeru Rangaku Isho no Keisei* [in Japanese, The Formation of Books on Dutch Medicine in the Netherlands] (Kyoto: Shibun-kaku Shuppan, 2007).

<sup>3</sup> For an overview of the history of “Dutch learning,” see Jiro Numata, *Yogaku* [in Japanese, Western Learning in Japan] (Tokyo: Yoshikawa Kobunkan, 1989); Shosuke Sato, *Yogakushi no Kenkyu* [in Japanese, A Study of Western Learning in Japan] (Tokyo: Chuo Koron-sha, 1980); Tadashi Yoshida, “Tenbo: Rangakushi [in Japanese, Overview: History of Dutch Learning in Japan],” *Kagakushi Kenkyu* 23 (1984): 73-80; Grant K. Goodman, *Japan and the Dutch, 1600-1853* (Richmond, Surrey, England: Curzen Press, 2000).

<sup>4</sup> Masao Sakaguchi, Tatsumasa Doke, Toshihiko Kikuchi and Minoru Tanaka, *Seimi Kaiso Kenkyu* [in Japanese, Studies on *Seimi Kaiso*] (Tokyo: Kodan-sha, 1975); Togo Tsukahara, *Affinity and Shinwa Ryoku: Introduction of Western Chemical Concepts in Early Nineteenth-Century Japan* (Amsterdam: J.C. Gieben, 1993).

<sup>5</sup> On Joji Sakurai, see Yoshiyuki Kikuchi, “Redefining Academic Chemistry: Joji Sakurai and the Introduction of Physical Chemistry into Maiji Japan,” *Historia Scientiarum* 9 (2000): 215-56;

idem, "Sakurai Jōji to Igrisu jin Kagakusha Konekushon [in Japanese, Joji Sakurai and His Connections with British Chemists]", *Kagakushi Kenkyu (the Journal of the Japanese Society for the History of Chemistry)* 31 (2004): 239-67; idem, "The English Model of Chemical Education in Meiji Japan: Transfer and Acculturation." PhD diss., The Open University, 2006.

<sup>6</sup> On Riko Majima, see Takashi Kubota, "*Hihon no Yuki-Kagaku no Kaitaku-sha Majima Riko* [in Japanese, Riko Majima: Founder of Organic Chemistry in Japan]," *Kagakushi Kenkyu (Journal of the Japanese Society for the History of Chemistry)* 30 (2003): 36-51, 93-105, 159-176, 231-255; Majima Riko Sensei Iko Kanko Inkaikai, ed., *Majima Riko Sensei no Iko to Tsuioku* [in Japanese, Professor Riko Majima's posthumous manuscript and Remembrance of Him] (Tokyo: Tokyo Kagaku-Dojin, 1970).

<sup>7</sup> Riko Majima, "Waga Shogai no Kaiko [in Japanese, Reminiscences of My Life]," *Kagaku no ryoiki* 8 (1) (1954): 1-11; (3): 137-146, p. 6.

<sup>8</sup> On Tetsuo Nozoe, see T. Nozoe, *Seventy Years in Organic Chemistry* (Washington, D. C.: American Chemical Society, 1991); Nozoe Tetsuo Kyoju Go-taikan Kinen-kai, ed., *Nozoe Tetsuo Kyoju Ronbun Mokuroku-shu* [in Japanese, Bibliography of Professor Tetsuo Nozoe's Papers] ([Sendai, Japan], 1966); Nozoe Tetsuo Sensei Tsuito Jigyo-kai, ed., *Hitosuji no Michi: Tsuioku Nozoe Tetsuo Sensei* [in Japanese, One way: For the memory of Professor Tetsuo Nozoe] ([Sendai, Japan], 1997); *Nozoe Tetsuo Sensei Kenkyu Ronbun-Chosaku Mokuroku* [in Japanese, Bibliography of Professor Tetsuo Nozoe's Work] ([Sendai, Japan], 1999); Shinji Takashima and Public Relations Department of *Hinoki Shinyaku, Ltd.*, *Hinokitiol Monogatari* [in Japanese, The Story of Hinokitiol] (Tokyo: Hinoki Shinyaku, Ltd., 1996); T. Asao, S. Ito, and I. Murata, "Tetsuo Nozoe (1902-1996)," *European Journal of Organic Chemistry* (2004): 899-928; Masanori Kaji, "Tetsuo Nozoe (1902-1996)", in *New Dictionary of Scientific Biography*, ed. Noretta Koertge (Detroit, New York et al.: Charles Scribner's Sons, 2008), vol. 5, 287-293.

<sup>9</sup> T. Nozoe, "Hinokitiol ni kansuru Kenkyu [in Japanese, Studies on Hinokitiol]," *Yakugaku* 3 (1949): 174-198.

<sup>10</sup> In the early 1950s, soon after the World War II, *the Proceedings of the Japan Academy* were one of a few journals in Japan, published in European languages. Riko Majima, a member of the Japan Academy, was communicated almost every month to the Academy for Nozoe (note 8, Nozoe, *Seventy Years in Organic Chemistry*, p.110). More than sixty papers in English were published in *the Proceedings of the Japan Academy* during 1950 and 1953.

<sup>11</sup> T. Nozoe, "On the Substitution Products of Tropolone and Allied Compounds," *Nature* 167 (1951): 1055-1060.

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## From Physical Chemistry to Molecular Biology: The Catalan Contributions to Nucleohistone Studies: 1965-1977

*Xavier Calvó-Monreal\**

During the 1960s, the Spanish Biochemistry and Molecular Biology received a decisive impulse for their development. The first generation of biochemists, trained abroad, had returned to Spain and set up their own research groups mainly with the support from the Spanish Science Council (CSIC). A new generation, their young graduates, completed their training following the same strategy, by postdoctoral research overseas.

A particular case, due to their initial training as chemists, was the group which became to be known as the “Catalan Structuralist School”, led by Jaume Palau and Joan Antoni Subirana. Their postdoctoral training abroad represented a change in their scientific interests, from organic chemistry to structural molecular biology, particularly the study of the nucleohistone. The aim of this communication is to show how it has been possible to reconstruct and study their early years, taking into account the disciplinary identity of chemistry and its changing relationships with other fields, such as Molecular Biology, mainly studying their correspondence, analysing their scientific papers in addition to interviews with the main characters, placing their School in the framework of the historiography of Biochemistry and Molecular Biology in Spain, and also in the wider, international, context.

### The Training Periods

Joan Antonio Subirana completed his degree in chemistry in Barcelona in 1958 and his PhD. in polymer chemistry and industrial engineering in Madrid in 1960. After a stay in Paris with Arnold Münster, the following year then moved to Harvard University to work with Paul Doty. More or less one year before his arri-

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val in Harvard, the denaturation and renaturation of DNA had been discovered by Doty and his co-workers. Denaturation is the process by which the two strands of DNA become separated, and renaturation, the opposite. It is important to stress that the understanding of both processes was crucial in order to provide data to validate Watson and Crick's 1953 DNA structure. Subirana's stay in Harvard produced four papers; the first of them co-authored with Doty, concerned phage DNA renaturation. Subirana became interested in the changes which could take place in DNA structure when interacting with other substances, such as proteins. As can be seen, Subirana's first interest was not in the biological field but in polymer chemistry. It was during his stay at Harvard when his shift to structural molecular biology took place.<sup>1</sup>

In 1963, after two years in Harvard, Subirana obtained a fellowship which allowed him to move to the polymer group at the Weizmann Institute in Rehovoth, to work under the leadership of Aaron Katchatsky. Subirana's work at Rehovoth was essentially theoretical, but it is important to stress his contribution, in what was his first paper, on the X-ray diffraction of biological polymers, mainly in the preparation of DNA-spermine fibres.<sup>2</sup>

Jaume Palau took his degree in chemistry in 1959 and his PhD in 1963 in Barcelona. In 1964, Palau attended a course given in Madrid by Alberto Sols at the "Centro de Investigaciones Biológicas del CSIC" (from the Spanish Science Council). Sols was one of the Spanish researchers who were developing own groups after their postdoctoral stages in foreign countries.

The same year, Palau went to London, funded by the British Council, to the Chester Beatty Research Institute, in the Physical Chemistry department, led by John Butler, and began his first work on histones. Simultaneously, Palau went to London King's College, to assist in a course given by Maurice Wilkins, in the biophysics department, began his research on oriented fibres from DNA-histone complexes, which had to be studied with X-ray diffraction techniques.

Palau's studies in London produced three papers on histone fractions. As well as the papers, both Subirana and Palau apprenticeships also consisted of acquiring knowledge about department organisation, and laboratory skills concerning the molecular biology techniques and instruments.<sup>3</sup>

Briefly, their return to Barcelona was to jobs as research collaborators of the CSIC, the Spanish Science Council and the beginnings of the biopolymer section within the genetics department of the Science Faculty in the University of Barcelona, under the leadership of Antoni Prevosti. In 1967, Subirana was appointed to the Chair of Organic Chemistry Technology in the Engineers School.

In 1969, Palau was immersed in activities which led to the creation of the Basic Biology Institute. It is important to stress that, in 1968, Subirana met Alexander Rich from the Massachusetts Institute of Technology (MIT), and the idea of an X-Ray laboratory in the Engineers School in Barcelona came from Rich's suggestions. The research field chosen by Palau and Subirana was the characterisation of the histones from marine invertebrates, echinoderms and molluscs, as well as the structure of the related nucleohistones, that are the associations between DNA and these proteins.

In 1963, while Subirana was in Harvard and Palau in Barcelona, waiting for the grant that would allow him to go to London, an event took place which is important to the account being related, this was, The First World Conference on Histones, organised and coordinated by James Bonner, from Caltech's Biology Department, and Paul Ts'o, from the Johns Hopkins University. After knowing about this conference and the subsequent review in *Science*, Subirana and Palau began to specify the research field in which they would like to work.<sup>4</sup>

A quick look at the list of the contributors to this conference permits one to appreciate the influence which it exerted on our main characters, not only during their postdoctoral training but in future joint ventures in research. Among others, it is important to stress the contributions of John Butler, Ernest Johns and Derek Phillips, from the Chester Beatty Research Institute, London, Edward Morton Bradbury and Colin Crane-Robinson, from Portsmouth, Brian Richards, from King's College London, and Geoffrey Zubay, former researcher with Paul Doty, but working at King's at the time of the conference.

Why their interest in histones? In spite of the publication in 1953 of the collection of papers in *Nature* concerning the structure of DNA and the available experimental evidences, the 1957 paper on the semi-conservative replication of DNA by Meselson and Stahl and in 1960 by Doty and co-workers on the denaturation-renaturation of DNA, in which Subirana had taken part, the Watson-Crick structure was no more than a theoretical speculation, especially in their suggested genetic implications. It is important to take note that the *cracking* of the genetic code did not occur until 1967. Moreover, the RNA structure was not yet solved and it is important to stress the pioneering works of Watson and Alexander Rich from 1953 to 1956. It is also important to take into account that the main issue at the time was the chromosome structure and the understanding of the genetic regulation and to remember that the chromosome is constituted not only of DNA, but of proteins, which had to play some role in this regulation and in its structure, a matter that had been in Subirana's mind since his time at Harvard.

In order to start the research it was essential for Palau to obtain a grant which would allow him to go to London, to the Chester Beatty, to be precise, to John Butler's laboratory, a leader in histone research. Why the Chester Beatty and Butler's group? Shortly before Palau's arrival, Ernest Johns, one of Butler's co-workers had discovered that, in no matter which kind of living organism, there were always the five same kinds of histones associated to DNA in the chromosome, and had published an account of the techniques which allowed them to be isolated. A grant obtained from the British Council and subsequently another from the Wellcome Trust, allowed Palau to stay in London during 1964, at the Chester Beatty and King's College.

### **The research project**

The next step was how to obtain funds for their research. Subirana's experience in the USA made him think in the Agriculture Department, which during these years was developing an aid program for underdeveloped countries called the Food for Peace Program, or Public Law 480. Their application was made during 1964 and approved in 1965, but the program was cancelled in the same year. These setbacks confirmed their decision to apply to the National Institutes of Health (NIH) extramural program. The grant was given in 1966 and allowed them to start their research group in Barcelona.

Apart from the fact that funding was guaranteed, the newborn group needed a physical space in which the research could be developed. While Palau was in London, Subirana had returned to Barcelona, to the Organic Chemistry Department, whilst waiting for the American grant, and applied for a collaborator post in the Spanish Science Council, insisting that Palau did the same.

Antoni Prevosti, Chair of Genetics At Barcelona University, who was interested in the setting up of a molecular genetics group in his department, was well disposed to host the new biopolymer's section, after applying for coordination with the Spanish Science Council. These circumstances allowed Palau and Subirana to begin with their research program on histones. Nevertheless, their economic situation was far from solved, because of their position in the Council structure. The "collaborator" was the lowest status. This potential instability caused Subirana's decision to compete for the Organic Chemistry Technology Chair in the Engineer's School in Barcelona, which he obtained in 1966.

It must be taken into account that Subirana, while studying chemistry, also took a degree in Industrial Engineering, which allowed him to obtain his second PhD



and that Subirana's first interests were not in the biological field but in the chemistry of polymers, mainly in plastics. Competing for a Chair was in Subirana's mind ever since he was at Harvard, according to him, in Spain, if anybody wanted to do research, the main requirements were independence and a global vision of the chosen research field, and a chair allowed the necessary independence. The fact of Subirana having the Chair and the NIH grant, allowed them to develop the biological research within the Engineer's School, at the same time dealing with the teaching and the industrial research.

In 1970 the group obtained a new coordination with the Council which allowed the formation of the Department of Macromolecular Chemistry. The grants, first from the NIH and, afterwards, from the Population Council, a Rockefeller Foundation subsidiary institution, allowed them since 1968 to buy the necessary instruments for the research program, to publish their papers and also to supervise their first Ph.D. students.<sup>5</sup> During their stay in the Genetics Department, Subirana and Palau published their first join paper on the histones of marine invertebrates, in 1966. From 1966 to 1973, Subirana published 23 papers, and Palau 10, five of which were co-authored. Their main interest was the study of these marine invertebrates, echinoderms and molluscs, because these organisms showed a wide range of basic proteins associated with DNA in the spermatozoid. They focused on these organisms after reading papers Maurice Wilkins and others from 1956 to 1960. Until the 1970s there were no papers published on the study of fish protamines, and little attention had been paid to other species' nucleoproteins which showed important differences in size and amino acid composition. Although the first research was on Sea urchin and Holothuria, a third kind of organism, molluscs, was included in the program.

Why did they study this kind of organism? Echinoderms have a special feature which is that their spermatozoids contents histones quite similar to those from somatic cells, but with some differences. One more advantage was the cell structure simplicity, related with its function, sperm cells are available and simple, because they contain the minimum components, among them the nucleoproteins, which allowed extrapolating the results to other species. Two important contributions were made by Palau and Subirana, firstly, the analysis of the proteins from invertebrate's sperm, particularly molluscs and echinoderms and, secondly, the structural studies of nucleohistone by X-Ray diffraction techniques. At this point, must be stressed that it was the methods developed by Johns, from the Chester Beatty Research Institute, which allowed the histone fractions to be isolated.<sup>6</sup>

## The Chromosome Structure

In the mid 1950s, while DNA structure and properties had been elucidated in some detail, the clarification of the nature of histones and how could be linked with DNA, remained unsolved. According to John Butler, the problem lay in the extraction methods by which the fractions of different composition were obtained. It was concluded that the native structures were destroyed during the extraction. Since it was known that nucleoprotein could be obtained as an aqueous solution by chromosome dispersion in water, it seemed possible to study its molecular characterisation and to obtain evidences concerning the chromosome constituent units. Modifications introduced by Doty and Zubay in the procedures led to the isolation of deoxyribonucleoprotein (DNP) solutions and use of a wide range of techniques, among them, chemical analysis, infrared spectrometry, light dispersion and electron microscopy.

But Doty and Zubay's research showed that the aqueous solutions of DNP had a complex behaviour that challenged a simple analysis and suggested a gel like system, consisting in subunits which could not be identified at the time. The interest in its study vanished and only a few works on this issue were developed during the 1950s. The alternative was to develop chemical techniques which allowed the extraction of the unknown number of histone fractions, and this was the approach developed by Johns during the first half of the 1960s. These methods were developed using calf thymus, then were applied to other materials such as invertebrate sperm nuclei. The method is based on the extraction of the whole tissue with acid in the presence of a high concentration of ethanol, in combination with a whole set of techniques which allowed to obtain the histone fractions for further studies, such as the structural ones.

Palau and Subirana used this method and the modifications required by their research in order to characterise the proteins from invertebrates sperm, previous to the development of the X-ray diffraction techniques. Their landmark paper of this period of 1973 was published in *Biochimica et Biophysica Acta*.<sup>7</sup>

In 1970, the biopolymers group was replaced by the Department of Macromolecular Chemistry, again with assistance of the Spanish Science Council. At the same time with the characterisation of the histones, the X-Ray laboratory was in its trial period. When the X-Ray diffraction techniques began to be applied to the studies of the nucleohistone, they had provided a small amount of data, mainly because the material's nature which usually had gel, mainly non-crystalline, characteristics. Nevertheless, some features suggested the presence of a repetition which could represent some kind of structural unit in the chromosome.

From this speculation, three research lines arose among the community, physical studies of the shape of the histone fractions and their behaviour, X-Ray diffraction techniques and spectroscopy studies. How to focus the study of this supposed nucleohistone superstructure consisted in the reconstruction of nucleoproteins from a mixture of DNA and histones, obtained separately.

In Barcelona, the X-Ray diffraction techniques were considered the best approach, which implied the setup and optimisation of the X-Ray lab, between 1968 and 1973, including the modification of the standard instruments and the design of their own prototypes. The decision of the setting up of the X-Ray laboratory came, mainly, from Subirana's experience in Israel, where he had collaborated in his first paper on this issue and from the advice from Alexander Rich, then at the MIT, during the summer of 1968. The apprenticeship on these techniques for Subirana took place during that summer at the Massachusetts General Hospital. This stay led to the connections with Alexander Rich, who, knowing the interests of Subirana, suggested that he set up the X-Ray facilities in Barcelona and, at the same time provided him with an instrument which could be useful in the study of fibres. This was a small X-Ray diffraction camera, which in Barcelona became to be known as "RICH". It is important to stress that, in 1968, Rich was working on RNA structure, but in crystals. The first tests took place in Barcelona at the end of 1969, and the first paper was published in 1973. From 1973 to 1977, Subirana's group published 9 papers using of X-ray diffraction techniques and it must be considered that the laboratory was fully optimised. Since 1968, Subirana followed the same approach for the study of the possible conformational changes in DNA when associated with proteins. First of all, proteins mainly from marine invertebrate's sperm were characterised. A great variety of those proteins and the differences in their composition were described and fibres of DNA-protein complexes were prepared. The results confirmed that the double helix was very stable and that its conformation did not change when associated with proteins.

Concerning the suggested presence of a repetition in the chromosome structure, taking into account the data provided by electron microscopy and with enzyme digestion techniques, in 1974, Roger Kornberg proposed the nucleosome model. There was a problem in that X-Ray diffraction techniques applied to fibre studies could only provide a global vision of DNA structure. As the DNA sequence changes along the genome the question was, what was the influence of the sequence in DNA structure? Could these differences influence the organisation and the activity of the genome in the cell nucleus? In order to answer these questions, the only way to approach the problem was to determine the structure of DNA of different sequences.

In the late 1970s, organic chemists discovered how to synthesise crystalline DNA fragments of known sequence. This fact allowed the start of the study of DNA structure at the atomic level, with more precise crystallographic methods, the *single crystal* one, which was successfully applied to RNA studies ten years ago. The first structures obtained represented at the same time a surprise but also a long expected confirmation. In 1979, Alexander Rich's group at the MIT obtained artificial DNA which crystallised in a different way to that of the standard Watson and Crick model, and was called Z-DNA. While the Watson and Crick helix was dextrorotatory, Rich's structure was levorotatory, and this led to the fact that DNA could present different conformations as well as the A and B forms. The Z-DNA structure rarely appears in biological systems and its importance in cell life still remains unknown.

Thanks to the obtaining the sequences of crystallised DNA and with the available techniques, the same year 1979, Richard Dickerson's group at the University of California at Los Angeles synthesised a double helix structure in the B form, which gave practically identical diffraction data to that obtained by Franklin and Wilkins two decades before. This fact constituted a step further in the confirmation of Watson and Crick's model.

Coinciding with these discoveries, Subirana's laboratory shifted from the study of fibres to those of crystals. During the following years, their contributions to this issue were confirmation of the results obtained with fibre studies, and the attempts to obtain DNA crystals with new shapes. Moreover, since 1977, the group have continued to expand their use of a wide range of techniques and instruments, including ultracentrifugation, electron microscopy and so on, for their structural studies.

## Conclusion

One of the issues raised in this communication has been the role of postdoctoral training in research centres abroad in the setting up of the new Spanish research groups in biochemistry and molecular biology as well as the maintenance of the established relationships. International recognition has been used as a legitimisation strategy. Thus, Palau and Subirana developed a research programme, including the necessary infrastructures, and the scientific policies which allowed them success. In summary, the study of the Barcelona's structuralist school reinforces the idea that successful scientists help to construct the scientific policy together with advancing their disciplines, experiments and techniques.

## Notes

<sup>1</sup> Juan Antonio Subirana, "The irreversible denaturation of bacteriophage deoxyribonucleic acid". *Biochim. Biophys. Acta* 103(1965):13-24; Juan Antonio Subirana and Paul Doty, "Kinetics of Renaturation of Denatured DNA. I- Spectrophotometric Results". *Biopolymers* 4 (1966): 171- 187; Juan Antonio Subirana, "Kinetics of Renaturation of Denatured DNA. II- Products of the Reaction". *Biopolymers* 4 (1966): 189-200; Juan Antonio Subirana, "La renaturalización de ácido desoxirribonucleico (ADN) desnaturalizado: aplicaciones al estudio del genoma de los bacteriófagos". *Anales de la Real Sociedad Española de Física y Química. Serie B-QUÍMICA*, 62 (1966): 521-526.

<sup>2</sup> Mario Suwalsky, Wolfie Traub, Uli Schmueli & Juan Antonio Subirana, "An X-Ray Study of the Interaction of DNA with Spermine". *J. Mol. Biol.* 42 (1969): 363-373.

<sup>3</sup> Jaime Palau & John A.V. Butler, "Trout-Liver Histones" *Biochem. J.* 100 (1966): 779-783; John Butler, David Power, & Jaime Palau, "Countercurrent-Distribution Studies on Histones". *Biochem. J.* 102 (1967): 539-547; Jaime Palau, Pardon & Brian Richards, "The reversibility of the dissociation of nucleohistone by salt". *Biochim. Biophys. Acta*, 129 (1967): 633-636.

<sup>4</sup> James Bonner & Paul T'so (eds), *The Nucleohistones* (San Francisco and London: Holden-Day, Inc, 1964).

<sup>5</sup> Jaime Palau & Juan Antonio Subirana, "Histones of Marine Invertebrates". *Biochem. J., Proceedings of the Biochemical Society* 101 (1966): 34-35.

<sup>6</sup> Ernest W. Johns, "Studies on Histones. 7. Preparative Methods for Histone Fractions from Calf Thymus". *Biochem. J.* 92 (1964): 55-60.

<sup>7</sup> Juan Antonio Subirana, Carmen Cozcolluela, Jaime Palau & Mercedes Unzeta, "Protamines and other basic proteins from spermatozoa of molluscs". *Biochim. Biophys. Acta* 317 (1973): 364-379.

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## What Kind of Scientist is a Physical Chemist or a Biochemist? Reflections on Scientific Identity and Institutionalisation in Science

*Anders Lundgren\**

These thoughts are the result of a need to reflect on the concepts of “institutionalisation in science” and “scientific identity”, which the author’s earlier study of the development of science in Sweden (mostly on chemistry) raised.

A suitable starting point is the well known member of “das wilde Heer der Ionier” (“the wild horde of the Ionians”), Svante Arrhenius. He received the Nobel Prize in chemistry, was Professor of Physics, and Director of the Nobel Institute for Physical Chemistry in Stockholm. In Swedish encyclopaedias he has not only been called, chemist, physicist, physical chemist, but also scientist, natural scientist etc. So, what was his disciplinary identity? Is this at all a meaningful question, in his case and in the history of science in general? Is it meaningful to ask, what are the differences between “molecular biology” and “microbiology”, or between “physiology and medical physics” and “pharmaceutical biochemistry”, or between “scientific biochemistry” and “bioorganic chemistry”, and thereafter use these concepts to describe what was going on in the history of science? And if it is not, which seems to be the implicit answer to the question posed, why are they used at all?

As the example of Arrhenius shows, “discipline” is a problematic concept. Frontiers between disciplines are always vague and changing, today as well as in historical times. What is called physics today is not what it was called yesterday, and the same goes for physical chemistry or biochemistry. This is well known, but people still write books, articles, arrange conferences on the question of disciplinary identity and on the histories of disciplines. On the scholarly map one can place oneself with the help of disciplines. For example, “I am a historian of chemistry /physics/biology/geochemistry”. Earlier writings in history of science have usually used the concept of discipline unreflectingly: we have “History of physiological chemistry/biochemistry” etc. It seems as if we can’t do without disciplinary titles.

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The same seems to be valid for other vague terms often connected to it, such as research schools, research traditions, and not least institutionalisation and specialisation.

These reflections on discipline, institutionalisation and identity are also a modest attempt to create some kind of tool by which we could discuss the emergence of new disciplines, their institutionalisation, and how that could be related to ideas on the scientist's disciplinary identity. The hope is at least that these reflections will allow the arrangement of what we already know in a little different way.

### **The Organisation of Science**

Some kind of organisation, sometimes pejoratively called bureaucracy, is needed for any science at all to be done. There has to be a structure, by which research is organised, money allocated, and teaching carried out. The basic unit in such an organisation is usually a University institution, an administrative unit which nearly always carries a disciplinary name.

In the following, such an administrative unit will be considered. It is a unit, which has to be officially approved and the Director should have the title of Professor, or the equivalent. It should have economical support from outside, its own budget, and not the least, a sign above the door, put there by someone else than those working at the institution. At Universities administrative units take part in the decision making process. They are, in general, respected members of the scientific community. The advantage for science is obvious, it gives a stronger position to the scientist from which he or she could buttress the kind of science they think is important. But, however necessary, research is not only a question of organisation.

What is called institutionalisation herein, that is, the creation of an independent institution of the kind just described, has its prerequisite in the emergence of new research areas. Institutionalisation is the whole process from the first budding signs of such a new research area to the complete and approved institution accepted by the scientific community. This process it is suggested consists of two main parts. The first part is the *inner institutionalisation*, which means the emergence of a new research field, vaguely characterised by a common goal, and common practices and theories around a central question. It should be distinguishable from other kinds of research, and slowly identified as a separate and more or less specific field. The second part is called *outer institutionalisation* which means the



establishment of formal institutions, positions, laboratories, and journals, all dedicated to the new research field.

The two phases of institutionalisation are of course related to each other. The rise of new institutions cannot be explained by the use of only *inner-* or *outer- institutionalisation*, or by letting one of them become the only cause. Both factors are needed and they interplay constantly during the whole process of institutionalisation, but with the basic relation that *inner institutionalisation* precedes *outer-institutionalisation* chronologically. There must be in existence a fairly well defined research area before a formal institution can be established. A new institution does not, of course, start by someone inventing a disciplinary name such as “physical chemistry”, or “biochemistry”, and thereafter decide what kind of scientific research to do, and as the next step asks for money to build an institution, to create positions, etc. There has to be something which can be institutionalised. First comes a scientific content, thereafter an institution. This might seem to be a rather old-fashioned internal way of looking at the emergence of new disciplines and institutions. But it only seems old-fashioned. First: that the *inner institutionalisation* in time precedes the *outer* one, does not mean that it determines it. Second: internalism cannot explain why only certain areas become institutionalised, and others do not. Third: the development of the cognitive content of a new research area still depends on cultural, social, economic and other factors. Fourth: for a new discipline to be established a lot of lobbying and fund-raising is necessary, activities that are not of a scientific nature.

There are two very obvious consequences of separating inner and outer institutionalisation. The first is so obvious that there is a risk that we neglect it. When studying institutionalisation in science, it is not enough to count, Chairs, positions, journals etc. A sociological approach of that kind is necessary but it is not sufficient. One has to study what the scientists are doing in their laboratory, during the phase when direct political pressure is relatively weak.

The second consequence is just as obvious: namely that institutionalisation of new disciplines take time. Even if this is known, it is often hidden behind words which imply sudden changes, such as, “break-through”, “revolution”, “a new era” and the like. The symbolic act of “cutting the ribbon” is a confirmation that a new discipline and/or a new institution has come into existence, not a sign that institutionalisation has begun. The cut ribbon rather initiates the beginning of a second stage of institutional development, the enthusiastic time, during which a new research field, which already has passed *inner institutionalisation* is given the administrative possibilities to mature into a full-fledged science.

## The Institutionalisation of Physical Chemistry

The following two sections are an attempt to apply the above ideas on the institutionalisation of physical chemistry and more specifically, of biochemistry in Sweden in the beginning of the 20<sup>th</sup> century.

When did physical chemistry begin? The traditional view is it started with “das wilde Heer der Ionier”, Ostwald’s *Institut für physikalische Chemie* in Leipzig and *Journal für physikalische Chemie*, 1887. All these events are certainly historically important. But the line between physics and chemistry has always been blurred, and the use of physical methods in chemistry is an old phenomenon. For example, Johan Gottschalk Wallerius published in 1759-1768 the impressive *Chemia Physica* (in Latin but also translated into German). Does that makes him a physical chemist? Did English chemists during the eighteenth century, for example Joseph Black, do physical chemistry when applying Newtonian physics to chemistry? And Lavoisier, who called himself experimental physicist, was he doing physical chemistry? The present author considers it would be a misleading use of words to call Lavoisier a physical chemist, and has doubts that any historical analysis would profit from so doing. To try to answer questions of that kind appears to be more like scholastics, where a definite meaning of every word is taken for granted, than nowadays scholarly works.

But it is perhaps more than just a scholastic question when one learns that the Swedish Docent Otto Pettersson in 1860’s lectured in Uppsala on “physical chemistry”, especially since he was one of the few scientists in Sweden who, twenty years later, supported Arrhenius when the latter published his theory of dissociation. How much physical chemistry was around when Arrhenius and the others appeared on the scene? To answer such questions by defining physical chemistry seems meaningless. A better understanding of the relation between Pettersson and Arrhenius can be reached by examining their sciences and the links between them: what did Pettersson do, what did Arrhenius do? To quarrel about what we shall call it or describe what they “really” were doing seems to lead nowhere.

In 1884 Arrhenius defended his thesis and many stories have been told about the event, usually with Arrhenius as the prime witness. His thesis was only just approved by the members of the committee, and it did not receive a high grade, but as the myth-makers like to say, twenty years later it gave him the Nobel Prize. Regardless this and the many other picturesque details around his dissertation, it is true that Arrhenius experienced problems in Uppsala. The fundamental reason for this conflict was that the field of research that he had chosen, electrical properties of electrolytes, did not exist in Uppsala, and did not fit into the existing research traditions. Arrhenius rather drew on the well developed

research tradition in Germany, in which Kohlrausch, Hittorf and others, had long before studied the electrical properties of electrolytes, and to whom Arrhenius frequently referred. However, there were virtually no references to Swedish scientists in Arrhenius', almost 150 pages long, thesis. In Uppsala his thesis fell outside existing research traditions both in chemistry and in physics. When the young student Arrhenius came up to the Professors in Chemistry and Physics in Uppsala and explained that sodium ions existed free in water, their reaction is more than understandable.

But less than six months after the public defence of the thesis, Arrhenius was appointed *Docent* in Physical Chemistry in Uppsala. Even if *Docent* was just an honorary title, one reason for this quick recognition was that Arrhenius had found support from an international research field. Especially from Wilhelm Ostwald, chemist, physicist, Nobel laureate, monist, philosopher etc, at this time Professor in Chemistry in Riga, and later from Jacobus van't Hoff, Professor of Chemistry, Mineralogy and Geology, and later Honorary Professor and Member of the Royal Prussian Academy. They were all working in the same research tradition, but as seen, under a host of different institutional names. Under all circumstances it is considered that with the initiated cooperation between these three men the study of the electrical properties of electrolytes in the middle of the 1880s had reached the stage of *inner institutionalisation*, but that it happened within many different institutional contexts.

So let us turn to *outer institutionalisation*. The proverbial ribbon was cut when Ostwald's Institute was opened in 1887, and the name used for the new research area was also used as a name for a new institution. However, *outer institutionalisation* is a long process and not by necessity connected to a specific disciplinary name. In Stockholm the research field became institutionalised at Stockholm University when Arrhenius was appointed Professor of Physics in 1895. From this position Arrhenius could propagate his new science and continue to expand the new research area, electrical properties in electrolyte solutions, into new fields like immunochemistry and cosmic physics. The name above the door could thus differ when it came to outer institutionalisation. It was not until 1908 that the name Physical Chemistry was used for an institute headed by Arrhenius, namely, the Nobel Institute for Physical Chemistry, Stockholm.

The establishment of the first Chair formally designated as Physical Chemistry in Sweden was in Uppsala in 1912. The Chair was especially created for The Svedberg. Arrhenius supported the idea to designate a Chair for Svedberg, but there were no collaborations at all between the two scientists. One reason was that Arrhenius did not do much innovative science during this epoch. His institute was declining, and can be called one the greatest scientific failures in the his-

tory of science in Sweden. This statement deserves a more thorough study, not in order to dethrone Arrhenius, but since failures are often more interesting and tell more about how science functions in society than do the successful cases. How come that the successful “wild horde” failed here? This is a serious question however relative is the concept of failure.

Perhaps a better explanation for the non-existence of collaborations between Svedberg and Arrhenius is that their research areas differed. When matriculating at Uppsala University Svedberg immediately took up the study of colloid chemistry, a field with no research tradition in Sweden, but one that was internationally strong and growing. Svedberg chose colloid chemistry because he wanted to prove the existence of the atoms. Therefore he constructed an ultra-microscope by which he thought it would be possible to see colloid particles the size of an atom by direct observation. By this time Svedberg considered himself a colloid chemist, and as such he became an important member of an international colloid network.

Svedberg and Arrhenius thus worked in two different research traditions, both of which received their outer institutionalisation under the same name: physical chemistry, in Uppsala, as a Department of the University, and in Stockholm, as the Nobel Institute of Physical Chemistry.

After having become Chair-holder in physical chemistry in 1912, and thus a “physical chemist”, Svedberg concentrated on the study of variation in particle size in colloids. For this reason he turned to proteins, each protein by then was considered to consist of very small particles (molecules) with varying size. Svedberg constructed the ultra-centrifuge, in order to measure sedimentation speeds, and thus molecular weights. With the decline of colloid chemistry in the 1920’s the research field changed into the field, “study of the physical properties of large chemical molecules”, but all the time Svedberg retained his position as Professor of Physical Chemistry. It was in this research tradition that Svedberg’s most well-known student, Arne Tiselius, worked. In line with the object of the research area, to determine physical properties of large chemical molecules, Svedberg suggested Tiselius to try measuring their electric characteristics. This was the beginning of the development of electrophoresis.

### **The Institutionalisation of Biochemistry in Sweden**

By the middle of the 1930s Tiselius seemed to be without any possibilities to continue in science, due to the curious Swedish University system. Lobbying started and after a private donation, Tiselius in 1939 was appointed Professor in

Biochemistry at Uppsala University. Under this name he continued to work, more or less in the same way, as he had been doing as an assistant to Svedberg in the Department of Physical Chemistry.

Hans von Euler had been Professor of Organic Chemistry at Stockholm University since 1905. He worked in the new research field of enzyme chemistry, where he belonged to an international network with close contacts especially with Germany. From his position as Professor in Organic Chemistry, he created the Biochemical Institute at the University around 1930, placing himself as its Head. The Institute was established to support his studies in the research field of enzyme chemistry. Enzyme chemistry thus received its outer institutionalisation in the form of a Biochemical Institute, headed by a Professor of Organic Chemistry.

Within the research tradition at Euler's institute, fermentation chemistry soon became a research area in itself, and Euler's pupil Karl Myrbäck was in 1932 given a Personal Chair in Fermentation Chemistry, donated by the brewer's association in Stockholm. In 1947 the chair was turned into a tenured position and renamed Organic Chemistry and Biochemistry. Finally, in 1963, Myrbäck became Professor of Biochemistry but all the time he continued to work on fermentation.

In the 1930s another Chair in Biochemistry was established at the Karolinian Institute, where the physician Hugo Theorell had created a Department of Biochemistry. This department's background was medical, and it had few, if any, similarities with the different research traditions in which Tiselius and Euler had been trained.

From these two cases, physical chemistry and biochemistry, it is obvious that the sign above the entrance to a department does not automatically tell what kind of research is going on behind that door. The difference in types of work behind the same name can be immense, and behind different names we can also find similar research. The scientists themselves did not seem to bother what to call their research areas. Arrhenius was satisfied by being Professor in Physics, and Euler did not mind keeping his position as Professor of Organic Chemistry at the same time that he was Head of a Biochemical Institute. In the published correspondence between Emil Fischer and Arrhenius the editors (Horst Remane and Levi Tansjö) point out that to both scientists, there was practically no difference between "allgemeine Chemie" and "physikalische Chemie". If someone would have called Arrhenius immunochemist or cosmic physicist, he would not have argued. Tiselius, as a pupil of Svedberg, all his life considered himself more of a

physical chemist than a biochemist, but accepted without problems to be called Professor of Biochemistry.

### Some Uses of Disciplinary Names

The advantages with an *outer institutionalisation* of a research area have been mentioned: the scientists reach a secure position from which to carry out the kind of scientific work they want to do. The positions give the possibilities to encourage their own scientific specialities. That was enough, even if Euler certainly must have been pleased to hear Frederick Hopkins call him, “a biochemist in all but name”. If a scientist’s research area can profit by a formal change of disciplinary identity then every scientist will gladly do so. This is more important than to keep, or to continue to use, a certain disciplinary name. From his secure position as Professor of Organic chemistry, Euler supported the study of enzymes under the name of biochemistry; and Tiselius supported the study of large biochemically important substances under the name of physical chemistry from his position as Professor in Biochemistry.

For tactical reasons disciplinary names were often loaded with non-scientific meaning. When approaching donors, governments and other possible economical benefactors, a new disciplinary name is an argument that the donors will be supporting something new and something modern. This is also an argument by which the presumptive donor can be honoured as a modern progressive member of society, if he donates the money.

In the case of Tiselius, biochemistry was explicitly associated to the question of the origin of life, which made it even more tempting for donors-to-be. Donors were also certainly impressed by Tiselius’ and Svedberg’s intense cooperation with the Rockefeller Foundation in its programme for studying life with the help of chemistry and physics. The name biochemistry was easily associated with this research programme, but the choice of name was still fundamentally tactical, in this case it was a successful tactic.

Another use of disciplinary names should be mentioned. They can be used against competitors or to stop the career progression of other scientists. A referee or a committee member can state that this is not “physical chemistry”, thereby preventing another applicant from a competing research area from getting a position. Statements like that often tell what a referee or member means by a discipline, but many times they are used to stop a competitor one cannot stop by referring to competence in science. In this sense, once *outer institution-*

*alisation* has been achieved, disciplinary names may function conservatively since they can be used to prevent the emergence of new ideas. This happened for example with Arrhenius in Uppsala, when he tried to break into an old research tradition with new concepts and a new outlook on the properties of chemical solutions.

## **Scientist's Identity**

Finally we come to the question of a scientist's identity. From what has been said it is clear that the identity as expressed in disciplinary names does not say much. So let us look at what some scientists themselves saw as their identity.

As mentioned, Tiselius had no tenured position by the middle of the thirties. The only Chair open for competition at the Universities was a Chair in Inorganic Chemistry. He decided to apply and in order to stand a better chance in the competition he decided to do some work in the field. He chose zeolites, something he never had worked with before. The choice of zeolites is understandable, since zeolites are inorganic substances with comparatively complex structure, and therefore in a sense closer to the research field of large molecules and complicated structures that Tiselius had been working with in biochemistry/physical chemistry.

By applying for the position, he wanted to promote himself and his own research, not any particular discipline. Contra factual questions are of dubitable interest in history, but the thought arises, that if Tiselius had been appointed Professor in Inorganic Chemistry, the way he would have developed the discipline would be in line with the traditions from Svedberg, as did his work in biochemistry, and that Tiselius would still consider himself basically belonging to the research tradition of Svedberg, a tradition called Physical Chemistry. The identity he wanted to keep was the identity of being a "scientist".

If a scientist wants to promote himself, to build an institution, raise money for a certain research area, etc, it is better to stress an identity as a "famous scientist", as a "Professor", or much better as a "Nobel laureate", than any disciplinary identity. Everyone knows that a Nobel laureate is a genius, whereas not everyone during the 1930's knew that the property of large biologically active chemical substances was an interesting research area. But for donors and benefactors to learn, or to be given an impression, which sciences are interesting they have to trust someone and such trust was created by fame, by the title of Professor, and particularly by the Nobel Prize. Nobel laureates in chemistry often consider themselves



physicists; Arrhenius, and not least Ernest Rutherford are not the only cases, but they gladly accepted the prize. The idea that someone would say no to the prize just because it was the wrong discipline is absurd. The possibilities that the prestige of the prize gave were enormous, and it is certainly not by chance that the most important institution builders in Sweden during this epoch, Manne Siegbahn in physics, Hugo Theorell in medicine and Svedberg, Euler and Tiselius, in chemistry, were all Nobel laureates.

When in the middle of their careers, both Arrhenius and Svedberg received, during their scientific heydays, calls from abroad. This caused scientists in Sweden to act in order to “save” for Sweden, not a “physical chemist” or a “colloid chemist”, but above all a “famous scientist”. With that identity it is easier to raise money, and therefore also to succeed in *outer institutionalisation* in order to guarantee the continuity of a research field that already has passed the stage of *inner institutionalisation*.

But if money could be raised and status received by the scientist being a “famous scientist”, instead of being “physical chemist” or “biochemist”, and if disciplinary identity is of minor interest when it comes to *inner institutionalisation*, one key question to put is how really interesting is the question of disciplinary identity? Is belonging to a discipline or being famous the most important, when it comes to the institutionalisation of scientific work? Should we not, when studying institutionalisation look at what scientists are doing, rather than take for granted that what they say they are doing is what they are doing? I think we all agree on this, but the author also believes that our habit to think of disciplines as cognitive categories still can prevent us from asking some relevant questions. To think of disciplines, institutionalisation and identity in terms of *inner-* and *outer institutionalisation* might help to understand the complicated processes by which scientists create an identity, new institutions come into existence, and new disciplines emerge.

## Notes

1. This paper has not been discussing the use of disciplinary names as analytical tools. The interest has been in the actual use of such names. To use them as analytical tools in order to organise and to explain historical material is of course a possibility. However, in that case the use the historian makes of such a name automatically differs from how the actors use it; this makes them less suitable for this purpose.

2. The empirical material in this essay is mainly based on my article “Naturvetenskaplig institutionalisering: The Svedberg, Arne Tiselius och biokemin” [Institutionalisation of science: The

Svedberg, Arne Tiselius and biochemistry], in Sven Widmalm (red.), *Vetenskapsbärarna. Naturvetenskapen i det svenska samhället, 1880-1950* (Stockholm, 1999), 117-143, where empirical details and sources can be found.

### *Bibliographical postscript*

The classical work on the development of physical chemistry is Johns Servos, *Physical Chemistry from Ostwald to Pauling: The Making of a Science in America* (Princeton, 1990). For Arrhenius see Elisabeth Crawford, *Arrhenius: From the Ionic Theory to the Greenhouse Effect* (Canton: Mass., 1996). There are no good full length biographies of Svedberg, Tiselius and Euler. Valuable information on the Swedish scientific scene around 1900 can be found in Sven Widmalm, *Det öppna laboratoriet: Uppsalafysiken och dess nätverk 1853-1910* [The open laboratory: Physics in Uppsala and its network 1853-1910] (Stockholm, 2001), and in Svante Lindqvist (ed), *Center on the Periphery: Historical Aspects of 20<sup>th</sup> Century Swedish Physics* (Canton: Mass., 1993).

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## The “Stiffness” of Ions – Ostwald as Anti-Atomistic Ionist

Klaus Ruthenberg\*

### Introduction<sup>1</sup>

Most of the important scientists of the 19th Century working in electrochemistry and related fields held certain views regarding the interrelations between stuff and electricity. Michael Faraday for instance (1791-1867), the inventor of the notion “ion”, in his *Experimental Researches in Electricity*, expressed scepticism when referring to atomism. Contrasting to that, in 1881 Hermann von Helmholtz (1821-1894) speculated about the particular character of electricity, *Elektrizitäts-atome*, and held the latter to be the ultimate driving force in chemistry. Svante Arrhenius (1859-1927) who introduced his theory of electrolytic dissociation in 1887 considered the pertinent mechanisms to be atomistic. Arrhenius, his friend Wilhelm Ostwald (1853-1932), and Jacobus Henricus van’t Hoff (1852-1911) formed a group called “The Ionists” which developed and brought forward the ionic theory through its research and thinking.<sup>2</sup>

Wilhelm Ostwald is renowned for his *energetical* and *anti-atomistic* attitudes. Yet he was honoured with the Nobel Prize in 1909 for his pioneer work on catalysis. In addition, he was – together with van’t Hoff and Arrhenius – the inventor and multiplier of modern chemical ionic theory. Both catalysis and ionic theory are strongly connected with a corpuscular, if not atomistic picture, at least in modern chemistry. Figure 1 shows a graphic representation of this point of view: In it the most fundamental entities are considered to be the elementary particles of the sub-microscopic world (white boxes). The entities of the manifest world (grey boxes) usually are thought of as being reducible, at least in principle, to these elementary particles, and the properties of the former to be reducible, at least in principle, to the properties of the latter. Ostwald’s own concept had just the opposite direction: Starting with things and bodies, and then considering Gibbsean phases, he ended up with elements which he considered to be particular energy

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forms. Whereas the co-ionists considered themselves to be atomists,<sup>3</sup> his (later) philosophy of chemistry offered no place for “hypothetic” sub-microscopic particles,<sup>4</sup> at least these were not needed conceptually. Hence, as to the on-going philosophical discussions about the interpretation of the term “stuff”,<sup>5</sup> and because this point has been almost neglected in otherwise very useful historical studies,<sup>6</sup> it is intriguing which position the anti-atomist but “ionist” Ostwald took, that is which position the box *ions* would have in schemes like that in Figure 1.

### Stuff in Ostwald’s Philosophy of Chemistry

The main source for this investigation is the most fundamental non-atomistic if not anti-atomistic treatise within 20th century chemistry, namely the “*Prinzipien der Chemie*”, published 1907.<sup>7</sup> Surprisingly, this programmatic “introduction to all chemistry textbooks” has only rarely been referred to in accounts of the history and philosophy of chemistry.<sup>8</sup>

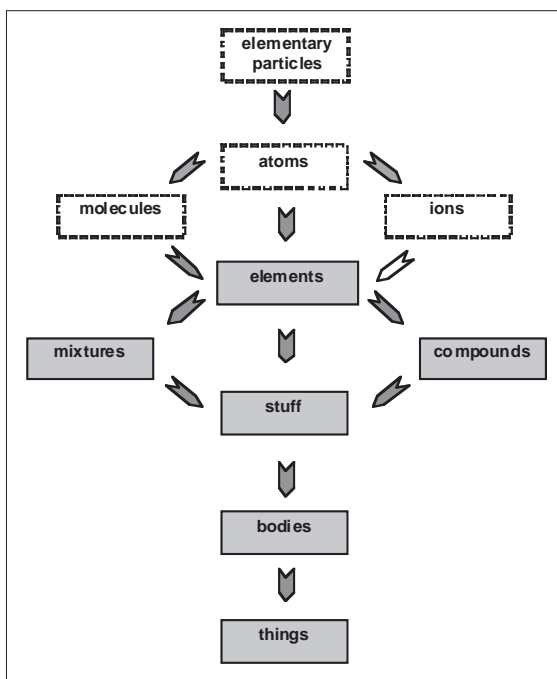


Figure 1. Scheme of the prevailing modern interpretation of the interrelations of central chemical concepts and entities. For explanations see text.

Ostwald considered chemistry as part of a natural philosophy which should be free of what he judged to be metaphysical assumptions and tried to present the fundamentals of chemistry from a phenomenological point of view. To him, there was no need of “hypothetic” atomistic language. Consequently, he made no use of atomistic vocabulary throughout the *Prinzipien*, and he strove for a theoretical closure of chemistry, as in his opinion mathematics, geometry, and mechanics had achieved already.

Repeatedly observed coexistence of certain properties was called a natural law by Ostwald. Some of these properties are arbitrary, others specific.

“The distinction is so important that it is the basis for the separation of two of the sciences: *Chemistry* has to do with specific properties, while the arbitrary properties are the province of *Physics*.”<sup>9</sup>

Ostwald formulated a *stuff law*, a precursor of which is the following statement:

“Substances are said to be *alike* chemically when they have similar specific properties.”<sup>10</sup>

From this stuff law an operational recipe (which can be turned into a requirement for investigations on purity) for the identification of substances can be inferred which still is applied in the characterisation of substances by, for instance, measuring the melting point or an infrared spectrum.

Ostwald recognised problems that occur when the laws he develops in the *Prinzipien* are applied to salt solutions:

“There is, however, a large and important group of substances in which regular contradictions to those laws appear, and these are not pure substances but a definite set of solutions. Especially among the aqueous solutions of salts we will find it necessary to extend our general ideas.”<sup>11</sup>

The extension of general ideas and their epistemological status is the subject of the following main section of the present contribution.

### **The last Chapter of the *Prinzipien***

Chapter XI, which is the very last chapter of the *Prinzipien*, comprises the following parts: Salt solutions and ions; Faraday’s law; The concept of ions considered chemically; Univalent and polyvalent ions; The molar weight of salts; The application of the phase law; Electrolytic dissociation.

Ostwald gave two separate definitions of chemical salts which are put here into Table 1, together with a modern one, for comparison. During the entire “introduction to all text-books of chemistry” Ostwald meticulously avoided atomistic and molecularistic vocabulary and he indeed made no exception when the text eventually came to electrochemistry. “Conductance of electricity” consists of nothing that refers to corpuscles, and his interpretation of the electrolytic dissociation is likewise held in the language of a substance-related chemistry.

Both of Ostwald’s attempts to define a chemical salt, however, lack generality. The “experimental” one, for instance, neglected that many substances which otherwise should be called salts are not soluble in water, thus cannot exhibit their

second class conductor property (Tab. 1/1). The “chemical” definition did not take into account more complex stuff such as double salts and complex ions, which comprise of more than one elemental constituent (Tab. 1/2). Moreover, in a slightly altered version this statement holds for melted salts, too. Obviously, for both definitions the application of electrical energy is necessary. Hence, using these definitions means to step away from the specific chemical realm comprising typical chemical energy. Nevertheless, Ostwald’s attempts are considerably nearer to laboratory experience than the typical modern version (Tab. 1/3).

Type	Definition
(1) experimental	A salt is a substance whose solutions act as conductors of electricity of the second class. <sup>12</sup>
(2) chemical	A salt is a substance which has the properties of a pure substance in the undissolved condition, while it exhibits the properties of two different substances while it is in solution. <sup>13</sup>
(3) modern	A salt consists of particles that have different charge and that build up a crystalline solid at standard temperature.

Table 1. Definitions of chemical salts (1 and 2 from Ostwald, 1909, 3 by the author)

As to his phenomenalist claims, Ostwald stepped on very thin ice in the following passage in which he referred to the mechanism of conductance in salt solutions:

“A reasonable description of the facts may be based upon the assumption that the passage of the electric current is accompanied by the simultaneous motion of certain constituents of the solution. These are the *ions*, and the action takes place in such a way that negative electricity moves with one of the constituents of the salt, while positive electricity moves with the other.”<sup>14</sup>

At first glance there seems to be no exit from this statement other than a particularistic interpretation. However, taken literally, “constituents” do not denote small charged particles in a modern, sub-microscopic sense. Rather, these two constituents are the “cation” and the “anion” (both in the singular). Unfortunately, Ostwald was not stressing the discussion of that problem at this part of his book.

The miraculous observation that different elements could be formed at the electrodes during electrolysis was met by Ostwald with the following explanation:

“These ions are of equal chemical composition but of different energetic value with the respective elements, thus they are “electrolytic” isomers.”<sup>15</sup>

The original salt has no impact on the chemical behaviour of the cation or anion (taken for granted its solubility, of course). Although this original interpretation has not become canonized within chemistry, the phenomenalist substance definition holds at least at that level of investigation. If complex ions are taken into account the situation becomes problematic, however. The sulphate-ion ( $\text{SO}_4^{2-}$ ), for instance, is discharged during electrolysis yielding oxygen and other species, but certainly not a simple corresponding isomeric substance like “ $\text{SO}_4$ ” or the like. Another questionable point is the operational requirement of the prepareability of chemical elements. Already the electrolytic transformation of the sodium-cation to its isomer metallic sodium from aqueous solutions does not work, because (in modern terms) the reduced sodium-ions instantaneously react with water to yield sodium hydroxide.

As to Faraday’s law,<sup>16</sup> Ostwald claimed that this can be applied without atomic hypothesis.<sup>17</sup> The equivalent amount  $M/z$ , and with this, the Faraday constant were subjects of convention, as far as he was concerned. Ions, at least in small concentration do behave independently of each other, and the two already mentioned definitions can be homogenized:

“Substances which conduct electrolytically contain constituents which react independently and *vice versa*.”<sup>18</sup>

There are no electrolytes that consist only of one constituent which, like Faraday’s law, can be simply deduced from the requirement of electroneutrality.<sup>19</sup> According to Ostwald, this was another aspect that requires a similar interpretation. These are the results of the determination of the molar weights of salts by measuring the osmotic pressure of dilute solutions of these salts.<sup>20</sup> As to these measurements, salts by the formula  $\text{K}^{\text{I}}\text{A}^{\text{I}}$  yield the half of the expected molar weight, those by the formula  $\text{K}^{\text{II}}\text{A}^{\text{I}}_2$  only one third. Ostwald’s explanation was as follows:

“These facts may be explained with the aid of the assumption which we made in considering the electrical and chemical properties of salt solutions. This was that ions exist as independent substances in salt solutions.”<sup>21</sup>

Again, the modern reader has to be careful not to interpret expressions like “salts consist of ions” in a modernistic sense, that is ions as sub-microscopic charged particles. In contrast, Ostwald considered “ions” to be substantial and stuff-like, and he used “cation” and “anion” in the singular throughout. Thus he found it natural to apply the Gibbsean phase rule<sup>22</sup> to salt solutions. According to the latter,



the degrees of freedom for (only) a solution of a simple salt would come up to  $F = 4$ . That result is in contrast to observation, because the real number of freedom degrees is  $F = 3$  (which becomes smaller if the number of phases is increased). Similar to the other aspects, Ostwald explained this smaller number of freedom degrees with the restrictive role of electroneutrality. The amounts of both ion sorts cannot be chosen independently:

“The various ions are to be considered as independent constituents, and one is to be subtracted from the sum of freedoms and phases resulting from the corresponding enumeration.”<sup>23</sup>

In the last part of the chapter analyzed here –*Electrolytic Dissociation*– Ostwald described the transformation of salts into ions as chemical reactions. These reactions were reversible and thus equilibrated:

“When a salt breaks up into its ions, a corresponding increase in osmotic pressure accompanies the increase in the molar concentration of the solution, and if the osmotic pressure is forcibly changed by dilution or concentration, reactions will be set up which resist the change.”<sup>24</sup>

Dilution fosters dissociation, concentration diminishes it. In the cases of high concentrations the dissociation grade should be taken into account. The derivation of the latter –which he himself introduced in 1888– Ostwald gave in the present passage. Thus he considered the applicability of the law of mass action as unproblematic.

## Conclusion – The Stuffiness of Ions

Wilhelm Ostwald conceded that a considerable large field of chemical appearances –he meant the solution of ions– are not easy to describe in terms of (his) phenomenological chemistry. Nevertheless, he made an attempt to present such description. Within the latter a picture of the isomeric nature of elements was applied to dissociated ions in aqueous solutions. Although the concept of isomery may have its merits referring to simple salts like sodium chloride (the chloride-ion is considered to be the isomer of chlorine, the sodium-ion is considered to be the isomer of sodium metal), the situation becomes more complicated when complex ions (“molecular ions” in modern terminology) like  $\text{NO}_3^-$  or  $\text{NH}_4^+$  are taken into consideration. These ions would have needed an extension of the concept such that corresponding uncharged stuff compounds were given. Moreover, even for simple cases it should be possible to prepare the kind of isomers Ostwald was

claiming. This preparation, however, is not possible. Stuff samples of pure chloride-ions, for instance, cannot be produced chemically. It follows from this that we are similarly not able to measure usual stuff properties of the electrolytic isomers. Accordingly, in customary stuff characterizations the electroneutrality was introduced quite early.

Wilhelm Ostwald could only maintain his phenomenological and operational point of view with additional, merely metaphysical assumptions. Hence, his attempt to toss the concept *ion* away from the theoretical or submicroscopic area (see the white arrow in Fig. 1) to the manifest world was questionable and, moreover, jeopardized his phenomenological project.

## Notes

<sup>1</sup> Revised version of the paper “To be both ionist and anti-atomist: a contradiction?” presented at the International Conference on the History of Chemistry at Leuven. The author thanks his chairman Carsten Reinhardt and all colleagues for their valuable contributions to the discussion, in particular Pierre Laszlo and Anders Lundgren. The core of this work was developed during a stay as visiting scholar at the Institute of Philosophy of the Catholic University of Leuven (HIW) in summer 2007. The author is greatly indebted to Prof. Dr. Jaap van Brakel of the HIW, who made this visit possible.

<sup>2</sup> John W. Servos, *Physical Chemistry from Ostwald to Pauling* (New Jersey: Princeton University Press, 1990), 39-45.

<sup>3</sup> Arrhenius, for example, left no doubt about this from the very beginning of his writings: Svante Arrhenius, “Über die Dissociation der im Wasser gelösten Stoffe”, *Zeitschrift für physikalische Chemie* 1 (1887): 631-648. See, for the relation between Arrhenius and Ostwald: Elisabeth Crawford, *Arrhenius. From Ionic Theory to the Greenhouse Effect* (Canton: Science History Publications, 1996).

<sup>4</sup> A pertinent early counter-example from the pre-energetic phase of his scientific work is: Wilhelm Ostwald and Walther Nernst, “Über freie Ionen”, *Zeitschrift für physikalische Chemie* 3 (1889): 121-130.

<sup>5</sup> Klaus Ruthenberg and Jaap van Brakel (Eds.), *Stuff – The Nature of Chemical Substances* (Würzburg: Königshausen&Neumann, 2008, in press).

<sup>6</sup> Robert S. Root-Bernstein, *The Ionists*, Ph.D. Thesis Princeton University, 1980; Alan Rocke, *Chemical Atomism in the Nineteenth Century* (Columbus: Ohio State University Press, 1984); Britta Görs, *Chemischer Atomismus* (Berlin: ERS Verlag, 1999).

<sup>7</sup> The book (Wilhelm Ostwald, *Prinzipien der Chemie*, Leipzig: Akademische Verlagsgesellschaft, 1907) is referred to here as *Prinzipien*, although the English translation (Wilhelm Ostwald, *The fundamental principles of chemistry*, New York: Longmans, Green, and Co., 1909) is used as reference.

<sup>8</sup> Even more recent publications do not consider this central work, e.g., Britta Görs, Nikos Psarros, Paul Ziche (Eds.), *Wilhelm Ostwald at the Crossroads between Chemistry, Philosophy and Media Culture* (Leipzig: Leipziger Universitätsverlag, 2005).

- <sup>9</sup> Ostwald, *Principles*, 4, emphases original.
- <sup>10</sup> Ostwald, *Principles*, 4, emphasis original.
- <sup>11</sup> Ostwald, *Principles*, 331, emphasis original.
- <sup>12</sup> Ostwald, *Principles*, 331.
- <sup>13</sup> Ostwald, *Principles*, 331.
- <sup>14</sup> Ostwald, *Principles*, 332, emphasis original.
- <sup>15</sup> Ostwald, *Principles*, 333.
- <sup>16</sup> In modern nomenclature:  $m = M/z L/F$  ( $m$  = obtained mass;  $z$  = exchanged charge;  $L$  = amount of electricity;  $F$  = Faraday's constant = 96485 C/mol).
- <sup>17</sup> Ostwald, *Principles*, 333-334.
- <sup>18</sup> Ostwald, *Principles*, 336, emphasis original.
- <sup>19</sup> Ostwald, *Principles*, 334.
- <sup>20</sup> Ostwald, *Principles*, 337-338.
- <sup>21</sup> Ostwald, *Principles*, 337.
- <sup>22</sup> For the present cases the phase rule is  $F = C - P + 2$  (the number of constituents, water, anion, and cation, is  $C = 3$ ).

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## Physical Chemistry Crossed the Boarder: Influences of Physical Chemistry in the German Chemical Industry, 1900-1950

Heinrich Kahlert\*

Before the turn of 20<sup>th</sup> century the German chemical industry was heavily influenced by the knowledge and the modelling approaches of organic chemists.<sup>1</sup> Although there were some chemical engineers in the industry, their influence regarding a more quantitative approach in industrial design was limited by the predominance of organic chemists. Originally caused by organic chemical constraints to enable various chemical reactions to obtain different dyes, the very important “oleum” (sulphuric acid) was produced by the so called “Röst-Process”. One decisive feature to control this process was the knowledge of the catalyst behaviour. At that time, BASF (Caro,<sup>2</sup> Knietzsch) was the leading company in the field to patent catalyst. These patents strongly influenced the style of physical chemical argumentation and modelling approaches, in contrast to the more common patents in organic chemistry.

With the technical introduction of the Haber-Bosch process before World War I (a “Pull-Factor”) and the theoretical understanding of the laws of thermodynamics and the kinetics of heterogeneous systems (a “Push-Factor”), the both parties, industry and academic, were coming together. Certainly also influenced by the multi-discipline genius Wilhelm Ostwald and his “Bridge-Function” (Hapke) capabilities. This congruence was reflected by the increasing number of physical chemical journals<sup>3</sup> (*Zeitschrift für physikalische Chemie* founded 1887, *Zeitschrift für Elektrochemie und angewandte physikalische Chemie* 1894, *Journal of Physical Chemistry*, 1896 etc), and the increasing number of papers as function of time in relation to organic papers.<sup>4</sup> Thus, physical chemistry had crossed the academic boarder. An important mentor and supporter of physical chemistry was Carl Bosch (1878-1940) as opposed to Carl Duisberg (1867-1930), who disliked the mathematical approach of the “modern chemist” preferring a more “holistic approach”.<sup>5</sup>

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The most important protagonists of this branch of the sciences (e.g. Haber, Nernst, Polanyi, Loewenstein, Epstein, Nikodem Caro) had to leave Germany and/or were prosecuted during the National Socialist-regime because of their Jewish origin.<sup>6</sup> It was not surprising that some organic NS-chemists declared that physical chemistry was not “clear” (“*anschaulich*”, Thiel)<sup>7</sup> enough or was an “intellectual science” (Hansen),<sup>8</sup> like it happened with Heisenberg, Sommerfeld, Bohr, Einstein in physics (“*Deutsche Physik*”). At this time there were some “dubious papers” (Jost) published in the journal (*Z. f. phys. Chem.*) which were caused by political pressure (Mentzel<sup>9</sup>), so that the three editors (Bodenstein, Bonhoeffer, Jost) had voluntarily resigned their editorship to protest against this hectoring.<sup>10</sup> One IG-Farben chemist, Heinrich Bütetisch, certainly the most highest ranked NS-member of the IG-Farben board members, defended the merits of the physical chemistry and its success in the German economy e.g. in the synthesis, coal to gasoline. Also the notorious opportunist Peter Adolf Thiessen, recommended the merits of physical chemistry as “decisive to obtain autarchy”.<sup>11</sup> This indicates that the physical chemistry was as well established in industry as in the National Socialist political system.

## Notes

<sup>1</sup> Carsten Reinhardt, Hans-Werner Schütt, “Christian Friedrich Schönbein und die Frühgeschichte der Katalyseforschung”, *Mitteilungen der Fachgruppe Geschichte der Chemie der GDCh* 6 (1991): 18-28.

<sup>2</sup> Carsten Reinhardt, Anthony S. Travis: *Heinrich Caro and the Creation of Modern Chemical Industry*. (Dordrecht: Kluwer Academic Publishers, 2000), 470 pp.

<sup>3</sup> Hans Schmitz, “Zur Entwicklung der chemischen Zeitschriftenliteratur”, *Laboratoriumspraxis* 19 (1967): 140-142;

Thomas Hapke, *Die Zeitschrift für physikalische Chemie: 100 Jahre Wechselwirkung zwischen Fachwissenschaft, Kommunikationsmedium und Gesellschaft*, (Herzberg: Bautz, 1990), 23

<sup>4</sup> The *Zeitschrift für physikalische Chemie* was very successful. The number of volumes increased from 1 to 6 in 16 years, the numbers of pages per volume varied between 600-800 pages.

<sup>5</sup> Heinrich Kahlert, *Chemiker unter Hitler – Wirtschaft, Wissenschaft und Technik der deutschen Chemie von 1914-1945*, (Grevembroich: Bernardus, 2001), 46.

<sup>6</sup> Ute Deichmann, “Chemists and Biochemists During the National Socialist Era” *Angewandte Chemie*, Int. Ed. 41, (2002): 3000-18; Ute Deichmann, “Dem Duce, dem Tenno und unserem Führer ein dreifaches Sieg Heil! Die Deutsche Chemische Gesellschaft und der Verein deutscher Chemiker in der NS-Zeit”, in *Zwischen Autonomie und Anpassung. Die Deutsche Physikalische Gesellschaft im Dritten Reich*, ed. Dieter Hoffmann and Mark Walker (Weinheim: Wiley- VCH, 2007), 459-498

<sup>7</sup> A. Thiel, “Zur Frage nach einer ‘anschaulichen’ Deutung der Osmose und des osmotischen Druckgesetzes” *Z. phys. Chem.* Vol. A178 (1937): 15-17; Steffen Richter, *Die “Deutsche Physik”*

in: *Naturwissenschaft, Technik und NS-Ideologie* ed. Herbert Mehrtens, Steffen Richter (Frankfurt a.M.: Suhrkamp, 1980), 116-141.

<sup>8</sup> Kahlert, "Chemiker unter Hitler" 45.

Christian Hansen, *Völkischen Beobachter*, March 19. 1936.

<sup>9</sup> Manfred. Rasch, 'Rudolf Mentzel', in *Neue Deutsche Biographie* (Berlin: Verlag Duncker & Humblot, 1994), 96-98.

<sup>10</sup> Wilhelm Jost, "The first 45 years of physical chemistry in Germany." *Annual Review of Physical Chemistry*, 17 (1966): 1-14;

Max Bodenstein, "50 Jahre Chemische Kinetik". *Zeitschrift für Elektrochemie und angewandte physikalische Chemie*, 47 (1941): 667-672.

<sup>11</sup> Heinrich Bütefisch, "Die Bedeutung der physikalischen Chemie für die chemische Grossindustrie", *Die Chemische Fabrik*, 8 (1935): 227-235; P. A. Thiessen, "Beruf und Stand", *Angewandten Chemie*, , 28 (1936): 19-20.

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## Another Early Root of Physical Organic Chemistry

Pierre Laszlo\*

This paper deals with building an identity for the new sub-discipline of physical organic chemistry. It addresses its historiography: a consequence of Whig history, as pervasive as it is implicit, is the standard account of the rise of a field. It is ascribed in this case to Hughes and Ingold in the 1930s and to Louis P. Hammett's *Physical Organic Chemistry* (1940). The standard account often masks or omits earlier and nevertheless seminal publications. To turn to the historical long haul –the *longue durée* dear to Fernand Braudel– offers a better perspective. I have drawn attention already to Sidgwick's contribution at the very beginning of the twentieth century with *Organic Chemistry of Nitrogen*.

Here, I turn up an even earlier milestone. Linear free energy relationships come close to a defining feature of physical organic chemistry, ever since Hammett's monograph. But when did they originate? Indeed the historical long haul provides the answer.

To evaluate the relative strengths of organic acids was a priority during the second half of the nineteenth century, even before the contributions by Arrhenius and Sørensen. A key episode was Wilhelm Ostwald's 1884 paper, introducing a novel measurement of acidity. It did so by first mining the history of chemistry and rediscovering Wilhelmy's work from 1850. It gave Ostwald the clue he needed to devise a measurement of the strength of acids, from their ability at catalyzing sucrose inversion. Thus, he correlated thermodynamics (acid strength) with kinetics (reaction rates). Hence the importance of this breakthrough as an antecedent, *avant la lettre*, to physical organic chemistry.

### Introduction

Fragmentation of the scientific enterprise into sub-disciplines is also a structuring process. It involves defining a more or less virgin field of study, an innovative set of methodologies for studying that field. Most important, the new subdisci-

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pline gives itself both principles and aporias, that is to say, its blind spots: assumptions which, as a rule, are manifestly untrue but turn out to be necessary simplifications.

In retracing the origins of a subdiscipline, some of the main relevant questions concern the pioneers, their social and disciplinar context, and of the time of occurrence of the key or supposedly key events. Other important and related questions are those of possible influences from nearby disciplines, such as for chemistry, physics or biology; the relevant forces, whether individuals were involved or whether it was collective activity, the result of some dominant intellectual configuration or fashion; quantitative evidence, such as crystallization of a number of subdisciplines when a discipline becomes heavily populated; the attendant question of clans with their respective leaders, who may introduce differing and sometimes conflicting research styles.

The subdiscipline focussed upon here is physical organic chemistry. The standard account of its coming into existence stresses Hughes and Ingold's work in England during the 1930s; <sup>1,2</sup> publication by the American Louis P. Hammett of a book entitled precisely *Physical Organic Chemistry* in 1940; and frequent statements about work by lone pioneers, especially Arthur Lapworth and Arthur Michael at the end of the nineteenth-beginning of the twentieth century.

For full disclosure, I should make it clear that I was a physical organic chemist for the whole duration of my career as an active scientist, from 1960 until 1999. Moreover, I have already published a companion paper on another early root of physical organic chemistry, dealing with publication in 1910 of Sidgwick's book, *Organic Chemistry of Nitrogen*.<sup>3</sup>

### **The approach chosen**

Linear free energy relationships will serve as our guides. They were a standard and a most important tool in physical organic chemistry.<sup>4,5,6,7</sup> They embody a correlation between thermodynamics and kinetics. Those are not necessary correlations, logically speaking. They are empirical, part and parcel of an aporia which indeed is one of the foundations of physical organic chemistry. Such correlations were rooted in a study by Wilhelm Ostwald, who himself founded physical chemistry, the predecessor to physical organic chemistry: that is the point I wish to bring up and to stress.

The paper involved<sup>8</sup> was published when Ostwald was 30 years old. He had been appointed a professor at the Riga Polytechnic three years earlier. At that time,

Ostwald was very much outside the mainstream of organic chemistry. He was an outsider geographically, he had studied at the University of Dorpat, under Russian rule, at a big distance from the great German centers of chemistry. Dorpat is a city to the northeast of Riga. Both towns were already part of the same Livonian Confederation from the Late Middle Ages.

Moreover, whereas his contemporaries concerned themselves with novel substances and reactions, Ostwald's concerns were more fundamental. He strove to understand the forces at work in holding molecules and in determining reactivity.

The French physicist Biot, the inventor of polarimetry, had discovered sucrose inversion, *i.e.*, the hydrolysis into glucose plus fructose. He also found that the transformation is catalysed by acids.<sup>9</sup> Several decades later, Berthelot would report that the same process is also catalyzed by enzymes, such as invertase, as he termed it because of its action.<sup>10,11</sup> The 1870s and 1880s were a period when organic chemists were striving to find an objective means for measuring the relative strength of acids. Ostwald's idea was to exploit the hydrolysis of sucrose, to measure its rates under catalysis by various acids and thus to gauge their strengths.

His was a meticulous study, indeed one finds abundant reference in the subsequent literature to the data he gathered. Ostwald studied the catalytic activity of no fewer than 32 different acids, of various structural types.

He used polarimetry for this purpose. Ostwald chose this technique simply because this was how Biot had discovered sucrose inversion. The technique is well-suited for monitoring the phenomenon. The reaction is slow enough, at room temperature, that it can be followed for several hours. Moreover, the observation technique not only is non-destructive, it does not interfere in any way with the progress of the reaction.<sup>12</sup> In addition, the intervening decades, since the first polarimeters were built in Paris by Soleil and his son-in-law Duboscq, had seen major improvements in design.<sup>13</sup> The instrumentation available to Ostwald was easy to use, quite accurate and furthermore recourse to the sodium D line made it monochromatic.<sup>14</sup> Ostwald used a Laurent-type polarimeter,<sup>15</sup> built in Berlin by Franz Schmidt and Herrmann Haensch, who had set-up their instrument-making company in 1864.

### **Ostwald's historical erudition**

Let me call attention to a most interesting feature of Ostwald's personality. He was acutely interested in the history of the discipline. He knew its bibliography

inside out. Thus, there were at least two antecedents to his work which he was well aware of, and which he refers to explicitly in his paper. Let us now consider them. His paper starts with a quotation from Biot's paper, which Ostwald reproduces in the original French.

The text of the Biot quote reads: "*Des observations comparatives, faites ainsi sur divers acides employés à des doses connues, liées à leurs poids atomiques, présenteraient vraisemblablement des résultats très dignes d'intérêt; mais je n'avais ni les moyens ni les connaissances nécessaires pour les suivre fructueusement sous ce rapport.*" Ostwald chose to read these lines from the French physicist in a prescriptive and programmatic sense: his own work will fulfill the void, glimpsed by Biot and which he had been unable to fill.

The second paper that Ostwald quotes had another physicist for its author. Ludwig Wilhelmy was working in Berlin. This young scientist was independently wealthy, which enabled him to purchase one of the early research polarimeters.<sup>16</sup> He applied it to the phenomenon of sucrose inversion.<sup>17</sup> In so doing, Wilhelmy discovered mass action law. However, his groundbreaking study fell into oblivion, he was not a chemist and in the 1850s and 1860s chemical science had other, major interests, at a distance from kinetics. This was the period for the blossoming of structural theory. Mass action law was rediscovered by Guldberg and Waage in 1864 which further pushed Wilhelmy's contribution into oblivion.<sup>18</sup> When van't Hoff was about to publish in 1884, unbeknownst to him, a near duplicate of Wilhelmy's paper, Ostwald, who knew his chemical history, had to warn him off.

## The aftermath

The contemporary context was ionic dissociation theory: after Arrhenius wrote his doctoral thesis on the conductivity of electrolytes. The significance of van't Hoff's *i*-factor, the multiplier necessary to make the gas law extendable to solutions, was pointed out by Arrhenius, but Ostwald made it much clearer than Arrhenius had been able to do.

Ostwald contributed significantly to ionic dissociation theory. He would point out in 1887 that osmotic pressure is proportional to the number of the dissolved particles, and he would formulate the same year 1887 the dilution law, which he showed to be applicable to several hundreds water-soluble acids and bases. In so doing, Ostwald again helped to clarify and extend the concepts of both Arrhenius and van't Hoff.

When Ostwald launched the discipline of physical chemistry and the attendant journal, *Zeitschrift für Physikalische Chemie* the same epochal year 1887,<sup>19</sup> there

was no move of his to set-up a sister or a daughter sub-discipline of physical organic chemistry.

When he turned to kinetic data for measuring the relative strengths of organic acids, this was an astute means for answering a then burning question. In so doing, Ostwald was bridging thermodynamics and kinetics in a way which was rediscovered only half-a-century later, with the first instalments of formal physical organic chemistry. Linear free energy relationships tap into the same vein that Ostwald had pioneered.

Why then has Ostwald's role as an effective founder of physical organic chemistry not been properly recognized? There are two answers to this question. The first, already alluded to, is that his founding physical chemistry just a few years later has totally eclipsed his 1884 contribution linking thermodynamics and kinetics. The second is the embedding in textbooks of Ostwald's study of sucrose inversion as catalyzed by acids: as a classic of chemical kinetics already in the years to follow.<sup>20,21,22,23,24,25</sup> Moreover, it entered the teaching laboratory as an experiment which most students of organic and / or physical chemistry –the latter especially, for a reason too obvious to be commented upon– repeated for many generations. It was inexpensive in materials. It served as an initiation into a powerful physical method, polarimetry, and it taught students how to use a polarimeter. Sucrose inversion was slow enough that the transformation could be monitored over several hours. Last but not least, it served implicitly as a celebration of one of the giants of chemical science. As a selection of just a sampling of textbooks and of articles in chemical education shows, Ostwald's study of sucrose inversion became statufied as a landmark in the training ritual for students of chemistry.<sup>26,27,28,29,30,31,32,33,34,35,36,37,38,39,40,41</sup> Its very enshrining as a milestone of chemical kinetics shrouded it; it was not viewed (as it might have) as bridging thermodynamics and kinetics and thus as a forerunner of physical organic chemistry. The year 1887, with Ostwald's founding physical chemistry in that year, came too close to the year 1884: the kinetics of sucrose inversion 1884 paper became assimilated into physical chemistry, instead of being identified as one of the earliest interfaces between organic and physical chemistry.

## **Conclusion**

This particular example brings up a first point, sometimes history serves as a pointer to the advancement of science. The past may be the best guide on how to orient one's research in the future.

Another point is the masking of Ostwald's role as a pioneer of physical organic chemistry. Since, in the late 1880s, he was the inventor of physical chemistry as a discipline, it eclipsed his early contributions in defining physical organic chemistry in its aims and methods. A roadsign used to be ubiquitous at railroad crossings in France: beware, a train can hide another. Likewise, the birth of a subdiscipline can hide another!

My final point, one that I have tried repeatedly to stress, is that often chemical history makes sense only in the long haul, the *longue durée* of Fernand Braudel.<sup>42</sup> Only by enlarging the temporal scope can one understand properly the events and the attendant evolution.

## References

- <sup>1</sup> K. Leffek, *Sir Christopher Ingold: A Major Prophet of Organic Chemistry*, (Victoria BC: Nova Lion Press, 1996).
- <sup>2</sup> J. Shorter, "Electronic theories of organic chemistry: Robinson and Ingold," *Natural Products Research* 4 (1987): 61-66.
- <sup>3</sup> P. Laszlo, "Features of mechanistic organic chemistry already present in 1910," *Ambix* 50 (2003): 261-73.
- <sup>4</sup> J. Shorter, *Correlation Analysis in Organic Chemistry: An Introduction to Linear Free-Energy Relationships*, (Oxford: Clarendon Press, 1973).
- <sup>5</sup> N. B. Chapman and J. Shorter, *Correlation Analysis in Chemistry*, Plenum Press, 1978).
- <sup>6</sup> R. I. Zalewski, T. M. Krygowski and J. Shorter, eds. *Similarity Models in Organic Chemistry, Biochemistry and Related Fields*. (Amsterdam: Elsevier, 1991).
- <sup>7</sup> A. Williams, *Free Energy Relationships in Organic and Bioorganic Chemistry*, (London: Royal Society of Chemistry, 2003).
- <sup>8</sup> W. Ostwald, "Studien zur chemischen Dynamik. III. Die Inversion des Rohrzuckers." *J. Prakt. Chemie* 29 (1884): 385-408.
- <sup>9</sup> J.-B. Biot, *Mém. Acad. Sci.* 13 (1835): 39-175.
- <sup>10</sup> M. Berthelot, "Sur la fermentation glucosique du sucre de canne," *Compt. Rend. Ac. Sci. Paris* 50 (1860): 980-84; see also C. O'Sullivan and F.W. Thompson. "Invertase: A Contribution to the History of an Enzyme or Unorganized Ferment." *J. Chem. Soc.* 57 (1890): 834-931.
- <sup>11</sup> L. Michaelis and M. Menten, "Die Kinetik der Invertinwirkung," *Biochem. Z.* 49 (1913): 333-69.
- <sup>12</sup> J. R. Partington, "Polarimeter," in *An Advanced Treatise on Physical Chemistry*, ed. J. R. Partington (London: Longmans, 1953), 347-53; S. F. Johnston, "Polarimeter, Chemical," in *Instruments of Science. An Historical Encyclopedia.*, ed. R. Bud and D. J. Warner (London: The Science Museum, 1998), 473-75.
- <sup>13</sup> P. Brenni, "Soleil-Duboscq-Pellin: A Dynasty of 19th century Scientific Instrument Makers," Eleventh International Scientific Instrument Symposium, Bologna University, 9-14 September; P. Brenni, "19th Century French Scientific Instrument Makers. XIII: Soleil, Duboscq, and Their Successors," *Bulletin of the Scientific Instrument Society* 51 (1996): 7-16.

- <sup>14</sup> Ward, R. "The Development of the Polarimeter in Relation to Problems in Pure and Applied Chemistry: An Aspect of 19th-Century Scientific Instrumentation." Ph. D. dissertation, London University, UK, 1979.
- <sup>15</sup> L. Laurent, "Sur le saccharimètre Laurent.," *Compt. Rend. Ac. Sci. Fr.* 89 (1879): 665-66.
- <sup>16</sup> Circumstances Ostwald was well aware of: see W. Ostwald, *L'évolution d'une science, la chimie*, (Paris: E. Flammarion, 1910), p. 255.
- <sup>17</sup> L. F. Wilhelmy, "Ueber das Gesetz, nach welchem die Einwirkung der Säuren auf den Rohrzucker stattfindet.," *Annalen der Physik und Chemie* 81 (1850): 413-28, 99-526, 27-32.
- <sup>18</sup> C. M. Guldberg and P. Waage, "Studier over affinitet. I-III.," *Forhandlinger I Videnskabs-selskabet I Christiania* (1864): 35-45, 92-94, 111-20.
- <sup>19</sup> J. W. Servos, *Physical Chemistry from Ostwald to Pauling: the Making of a Science in America*, (Princeton, NJ: Princeton University Press, 1990).
- <sup>20</sup> S. Arrhenius, "On the Reaction Velocity of the Inversion of Cane Sugar by Acids," *Zeitschrift für Physikalische Chemie* 4 (1889): 226.
- <sup>21</sup> J. Walker, *Introduction to Physical Chemistry*, (London: Macmillan, 1899).
- <sup>22</sup> F. P. Worley, "Studies of the processes operative in solution XXII.," *Proc. Royal Soc.* 87A (1912): 563-81.
- <sup>23</sup> E. W. Washburn, *An Introduction to the Principles of Physical Chemistry from the standpoint of modern atomistics and thermodynamics. A course of instruction for students intending to enter physics or chemistry as a profession.* (New York: McGraw-Hill, 1915).
- <sup>24</sup> L. J. Curtis, "Concept of the Exponential Decay Law Prior to 1900," *American Journal of Physics* 46 (1978): 896-905.
- <sup>25</sup> G. Rolfe, *The Polariscope in the Chemical Laboratory*, (New York: Macmillan, 1905).
- <sup>26</sup> E. B. R. Prideaux, *Problems in Physical Chemistry with Practical Applications*, (London: Constable & Company, 1920).
- <sup>27</sup> J. F. Spencer, *An Experimental Course of Physical Chemistry. Part II Dynamical Experiments.*, (London: G. Bell and Sons, 1921).
- <sup>28</sup> A. Findlay, *Practical Physical Chemistry*, (London: Longmans, Green and Co., 1925).
- <sup>29</sup> E. Mack and W. France, *A Laboratory Manual of Elementary Physical Chemistry*, (New York: Van Nostrand, 1928); E. Mack and W. G. France, *A Laboratory Manual of Elementary Physical Chemistry*, (New York: D. Van Nostrand, 1934).
- <sup>30</sup> F. Daniels, J. H. Mathews and J. W. Williams, *Experimental Physical Chemistry*, (New York: McGraw-Hill, 1929); F. Daniels, J. H. Mathews and J. W. Williams, *Experimental Physical Chemistry*, (New York: McGraw-Hill, 1934); F. Daniels, R. A. Alberty, J. W. Williams, C. D. Cornwell, P. Bender and J. E. Harriman, *Experimental Physical Chemistry*, (New York: McGraw-Hill, 1970), Experiment 24, "Inversion of Sucrose".
- <sup>31</sup> J. R. Mohr and D. C. Neckers, *Laboratory Experiments in Organic Chemistry*, (New York: D. Van Nostrand, 1973).
- <sup>32</sup> J. G. Dawber, R. R. Brown and R. A. Reed, "Acid-catalyzed hydrolysis of sucrose. A student study of a reaction mechanism," *Journal of Chemical Education* 43 (1966): 34.
- <sup>33</sup> T. J. Zielinski and R. W. Schwenz, "Physical Chemistry: A Curriculum for 2004 and Beyond," *The Chemical Educator* 9 (2004):
- <sup>34</sup> W. J. Wienen and R. S. Shallenberger, "Influence of acid and temperature on the rate of inversion of sucrose," *Food Chemistry* 29 (1988): 51-55.
- <sup>35</sup> R. S. Dordick and G. A. Clarke, "Salt effects on the hydrolysis of sucrose," *Journal of Chemical Education* 56 (1979): 352.

- <sup>36</sup> E. Y. Shalaev, Q. Lu, M. Shalaeva and G. Zografi, "Acid-Catalyzed Inversion of Sucrose in the Amorphous State at Very Low Levels of Residual Water," *Pharmaceutical Research* 17 (2000): 366-70.
- <sup>37</sup> M. A. Gibas, "A polarimeter experiment for introductory courses," *Journal of Chemical Education* 53 (1976): 462.
- <sup>38</sup> M. D. Mosher, C. O. Kelly and M. W. Mosher, "Examination of a Reaction Mechanism by Polarimetry: An Experiment for the Undergraduate Organic Chemistry Laboratory," *Journal of Chemical Education* 73 (1996): 567-68.
- <sup>39</sup> H. Klostergaard, "Inversion of sucrose and fructose structure," *Journal of Chemical Education* 53 (1976): 298.
- <sup>40</sup> S. M. Mahurin, R. N. Compton and R. N. Zare, "Demonstration of optical rotatory dispersion of sucrose," *Journal of Chemical Education* 76 (1999): 1234.
- <sup>41</sup> D. P. Shoemaker, C. W. Garland and J. W. Nibler, *Experiments in Physical Chemistry*, (Boston: McGraw-Hill, 1996), 264-75.
- <sup>42</sup> F. Braudel, "History and the Social Sciences: The *longue durée*," in *Economy and Society in Early Modern Europe*, ed. P. Burke (London: Routledge and Kegan Paul, 1972), 11-42.



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## Robert Boyle's Experiments on Cold: A Study of the Role of Chemical Experiments

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Cold was one of the few qualities of matter which formed part of Boyle's<sup>1</sup> early experimental agenda at the beginning of his experimental activities in 1649.<sup>2</sup> Boyle had initiated his experimental studies with experiments of chemical, alchemical and pharmaceutical interest. During this early period of experimentation he also designed schemes to investigate other qualities of matter such as heat, fluidity, firmness, porosity and colour which he developed in parallel with his first ideas on atomism and the composition of matter. During the first half of the 1660's, Boyle published *The Sceptical Chymist*<sup>3</sup> (1661) and the essay 'Essay on Nitre'.<sup>4</sup> In the first, he presented experiments on the analysis of bodies through chemical processes, with which he attempted to refute the predominant theories on the constitution of matter. In the second, experiments on the analysis and composition of nitre were used to support his views on the corpuscular structure of matter. During the same period, Boyle published two treatises on qualities of matter which he considered the most comprehensive in their subject, *The History of Fluidity and Firmness* (1662)<sup>5</sup> and *New Experiments and Observations Touching Cold or an Experimental History of Cold, Begun* (1665)<sup>6</sup> (which from here is referred to as *Cold*). In this paper some of the categories of experiments presented in the treatise *Cold*<sup>7</sup> will be used in a case study to examine the role of chemical and alchemical practices as well as the knowledge produced by them, in the development of certain experimental inquiries and experimental practices on cold. Firstly, it will be illustrated how the phenomena of cold and particularly the process of freezing were shown by Boyle to provide new ways of separating substances and of producing knowledge on the chemical qualities of bodies and the classification of various substances. Secondly, it will be shown that knowledge on the categories of substances produced through the use of chemical processes played an important role in the formation of certain experimental inquiries on cold. Thirdly, it will be argued that some of the experimental issues treated in *Cold* supported and corroborated Boyle's arguments against the chemical theories

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of the constitution of matter as treated in the *Sceptical Chymist*. It will be suggested that Boyle's experimental examination of the quality of cold constituted an effort to form a new experimental field in which chemical and alchemical practices played an important role.

### Experimental History of Cold, Begun

Boyle's treatise on *Cold* was structured after the Baconian principles of a "history". Boyle claimed to be presenting experimental results and observations on phenomena of cold without engaging into a discussion of a theory for the explanation of the nature of cold,<sup>8</sup> nevertheless, *Cold* was not a pure Baconian history. In various investigations, his trials focused on the refutation of predominant theories of his time such as the Aristotelian theory of the four principles of matter and Pierre Gassendi's (1592-1655) theory on the existence of "frigorific corpuscles" of cold. *Cold* consists of a vast range of experimental trials that can be categorised in five general thematic groups. The first and largest group contains experiments on the phenomena of freezing. Central to Boyle's experimental scheme on cold was the examination of the ability of certain categories of liquids and solid bodies to freeze, and the various aspects of the expansion of freezing water and certain related liquids. The verification of the expansion of water by freezing had opened up new experimental themes, such as the measurement of its expansion, the investigation of the expansive force of freezing, and the examination of the content of bubbles produced in ice. In the second group of trials Boyle examined the phenomena related to ice such as the cohesion of its parts and its duration in various liquids and in the air. In the third group, Boyle examined various degrees of cold by the use of a liquid thermoscope [In the secondary bibliography on the history of the thermometer, the term "thermoscope" is used to signify an instrument that indicates changes in temperature but does not have a standard scale]<sup>9</sup>. He tried to establish the difference between a degree of cold capable to freeze water and that produced by the freezing mixture of salt and snow, as well as the difference between a freezing degree of cold and a lower degree which was unable to cause freezing. The fourth group contained experiments on the ability of cold to diffuse and its transmission through various mediums. In the fifth group of trials Boyle examined those bodies that could either, alone or when mixed with ice or snow, propagate and intensify the quality of cold. The most important trials and observations presented here were aimed at the refutation of the various theories that argued for the existence of a single body which was the primary cause of cold in all other bodies.

A close study of the questions posed and the phenomena examined in the treatise *Cold* shows that part of Boyle's experimental inquiries were developed on the basis of his efforts to refute phenomena and explanations established on Aristotelian and Scholastic principles. The diversity of the phenomena examined as well as thematic categories such as the degrees of cold showed that Boyle was also relying on the Baconian rules of investigating the nature of qualities as presented in the *Novum Organum* (1620)<sup>10</sup> in the form of the three tables of inquiry into the quality of heat. Moreover, some of his trials stemmed from the examination of certain "traditions" on the effects of cold supported by the "chymists"<sup>11</sup> of his time and others contributing to common belief. Boyle had collected the description of phenomena and the explanations of the nature of cold predominant in his time and had converted them into specific inquiries. In contemplating the existing traditions and opinions he had also developed new inquiries into phenomena of cold which allow us to argue that the experimental scheme on cold was opening new pathways to the examination of the quality of cold. Furthermore, the treatise on *Cold* was unique in its time, in terms of its focus on the experimental examination of the phenomena and effects of the quality of cold. Until then, the quality of cold was mainly treated as part of a general theory of matter which was used to explain the qualities of bodies such as heat, cold, fluidity, firmness and others as well as part of the treatment of meteorological phenomena. Some examples are the original Aristotelian works,<sup>12</sup> the scholastic treatises on phenomena of freezing discussed by Aristotle,<sup>13</sup> and the treatises presenting the new atomic and corpuscular theories of matter.<sup>14</sup> The topic of cold was also discussed as part of the observations contained in the writings of navigator's travels to "gelid"[very cold] climates as well as in laymans' books on meteorological observations. It cannot be denied that the examination of cold formed part of Boyle's scheme to construct a corpuscular theory of matter supported by experimental phenomena. Nevertheless, it is evident by the structure and the content of the treatise that at the same time he was trying to form an experimental inquiry that focused primarily on the phenomena, it also considered some questions not directly aiming to support a corpuscular theory of matter.

There are three examples that illustrate the way chemical practices and the knowledge acquired through them was used in the treatise, *Cold*. The first shows the use of cold as a process of chemical separation instead of distillation. The second illustrates the role played by the categories of substances according to their chemical qualities in the examination of the phenomena of freezing and the contribution of experiments on freezing to the classification of substances. The third example, presents how the experiments on cold supported the argument put forward in the *Sceptical Chymist* against the explanation of the qualities of bodies

on the basis of the Aristotelian elementary theory, and the three Paracelcian, or chemical, principles.

On the basis of his experiments on the capacity of various categories of liquids to freeze, such as salts, spirits, oils and various kinds of solutions and *lixiviums*, Boyle was led to the conclusion that only liquids and bodies that contained aqueous and phlegmatic parts could be frozen.<sup>15</sup> According to Boyle only the bodies that properly froze were expanded by the effect of cold. In the cases of frozen spirituous liquids, such as beer as also saline liquids such as seawater it was observed, particularly in “gelid” climates, that the aqueous or phlegmatic part was separated from the spirituous or saline parts by being frozen, whereas the latter remained fluid.<sup>16</sup> Furthermore, in the case of seawater it was observed that once the ice was melted it produced water fresh to the taste. In the *Sceptical Chymist* Boyle had used such observations in order to prove that cold did not have the effect to congregate homogeneous and heterogeneous bodies as was held by the Aristotelian theory.<sup>17</sup> Whereas in the *Sceptical Chymist* Boyle held reservations on reproducing the effects of analysis by freezing in the climate of England, as most of his observations stemmed from phenomena in the “gelid” climates, in the treatise of *Cold* he presented a series of phenomena which showed that he had managed to reproduce some separations as those of beer and solutions of salt in water. Boyle argued that the specific phenomenon caused by cold could be used as a process replacing distillation for practical purposes, in the case of sea and salt water, the separation of water from salt through freezing could be more cost-efficient than the use of heat. The process of freezing was also used to reveal two other important characteristics of the constituents of certain bodies. Firstly, Boyle used the expansion of aqueous liquids as a method to define the proportion of phlegm in them.<sup>18</sup> Secondly, he argued that the process of freezing in solids such as vegetables and fruits revealed the juicy and aqueous parts contained in them which were in some cases invisible.<sup>19</sup> On the basis of this observation and the already verified phenomenon of the expansion of aqueous liquids by freezing,<sup>20</sup> Boyle formed an explanation of the destruction of their texture after being frozen. He proposed that the juicy parts, being expanded by freezing would push hard against the cavities of the solid bodies that contained them, and thus would destroy its texture, in the same way as a fruit gets bruised and rots. Cold could be used to replace in some cases some chemical processes, such as distillations and it could be used also in order to investigate some characteristics of matter.

The experimental inquiries on the ability of liquids and bodies to freeze,<sup>21</sup> their ability to propagate the degree of cold produced by ice and snow,<sup>22</sup> and the examination of the duration of ice in various liquids,<sup>23</sup> were all focused primarily on

categories of liquids and bodies and their chemically produced solutions, distillates, decoctions, and *lixiviums*. In particular, Boyle examined groups of liquids, such as salts, oils, and spirits and used his results to deduce general conclusions on each category of bodies. According to Boyle, solutions of salts, spirits of gross salts as well as other aqueous liquids such as beer, milk, vinegar could be frozen whereas chemical oils and fermented liquids and strong spirits could not. The ability to freeze was related to the amount of aqueous and phlegmatic parts in the liquids. Those that could not freeze were shown to contract by a high degree of cold. Boyle also examined whether the ability to freeze applied to all kinds of liquids belonging to the same category and whether it was preserved when they were processed together to produce a solution. He also examined the differences in the degree of cold and time interval required for the liquids of the same category to freeze or condense. In the case of the experiments on the ability of various chemically processed liquids to intensify the freezing effect of ice and snow, Boyle focused on the examination of the various salts in order to examine whether the effect could be attributed to the saline part. He also examined liquids that varied in acidity. In each trial attention was given to the intensity of the freezing effect which was used as evidence of the degree of cold produced. The ability of a liquid to cause freezing by its mixture with snow was related to its ability to melt the snow more rapidly than it would in its pure form. This characteristic varied even among liquids of the same category. Moreover, Boyle examined whether liquids that were shown to intensify the freezing effect of ice would retain this characteristic after their mixture with other liquids.

The above experiments had multiple purposes. Firstly, they led Boyle to important conclusions on the identification of those parts of liquids responsible for their freezing. Their practical use, in chemical experimentation and in constructing explanations in various phenomena, as for example, the corruption of the texture of alimentary bodies by freezing were discussed earlier. Secondly, the attribution of the ability to freeze or be frozen to certain liquids were used by Boyle to support his views on the classification of certain substances which differed, at least in some cases, to that of physicians and chemists.<sup>24</sup> Thirdly, the experimental results contributed to the examination of the nature of cold. Defining whether the ability to freeze and be frozen was a characteristic attribute of one or multiple substances could be used in Boyle's effort to refute hypotheses like the one held by Gassendi on the nature of nitre. Boyle had executed a wide number of experiments to show that nitre and its products could not be considered the primary cause of cold.<sup>25</sup> He argued that Gassendi's experiments aiming to show that cold was produced in bodies through the participation of nitre were based on experimentation by some chemists of his time who attributed to nitre the capacity to

cause cold on its own without being mixed with snow.

One of the central issues dealt with in the treatise on cold concerns the general view held on various theories of matter, according to which cold can be attributed primarily to an elemental body, by the participation of which it is transmitted to all other bodies.<sup>26</sup> Boyle used experiments and observations from “gelid” climates to refute four hypotheses on the nature of such a body. Aristotelians and scholastic philosophers proposed water, the philosopher of the Hellenistic period, Plutarch suggested earth, the Stoics suggested air and Gassendi suggested nitre. Boyle showed that none of the four bodies could be considered to cause and accept the effects of cold in a more intense degree than the others. Nitre was the only case where he argued using his own experimental results. Boyle showed that although nitre had a freezing effect when mixed with snow, there were other substances that caused more intense effects. Boyle’s arguments against the other three elemental bodies were based on the macroscopic phenomena of freezing in the sea, on earth and its subterranean areas and the atmospheric air. Boyle explained that his argument against the attribution of cold as a primary quality to a single body, was based on his more general belief that such a body does not exist. He pointed to the *Sceptical Chymist* where he had attempted to refute the attribution of the cause of the qualities of bodies, such as odours, colours, and gravity, to the four elements by the Aristotelians and to the three or five principles, tenets of “chymical” theories.<sup>27</sup> Boyle’s arguments in *Cold* was presented as corroborating the arguments put forth in the *Sceptical Chymist* against the existence of primary forms in certain substances considered elemental to matter. Experimental results pertaining to a treatise against various tenets of “chymical” theories seemed to work, hand in hand, with experiments on cold, particularly those that treated the macroscopic effects of the quality of cold.

## Conclusions

It has been argued that in setting up his experimental agenda on cold Boyle collected the various established phenomena and theoretical hypotheses on the nature of the quality that had been presented by the, still predominant, Aristotelian Theory, the emerging atomic theories of matter and various treatises on the meteorological and geological phenomena of cold in “gelid” climates. Boyle, guided by his own evolving ideas on the corpuscular theory of matter and explanation of its qualities, structured and organised a set of inquiries on cold that provided not only refutations to the established theories on cold but also initiated new investigations that could possibly lead to new explanatory hypotheses.

The discussion of the experiments performed on the ability of bodies to freeze and be frozen shows that knowledge produced by chemical practices about the categories of substances as well as Boyle's interest in chemical analysis as an investigative tool played an important role in the formation of the particular set of questions. It has also been shown that experiments on freezing produced knowledge useful to the practise of classification of substances according to their "chemical qualities". Moreover the use of freezing as a tool to separate bodies and investigate their constitutive parts was based on important conclusions on the characteristics of freezing as was for instance the expansion of aqueous liquids during the process and the separation of their parts. Therefore, we can argue that there was close collaboration of the knowledge produced by chemical practises and experiments on cold, what seemed for the time a natural philosophical topic. Furthermore it is made evident that the attempt to refute the attribution of the cause of cold to elemental bodies as primary agents of cold, as well as Boyle's general aim to refute the Aristotelian theory of the four elements and the chemical theories of three or five principles, created a context where results from both the experimental practice of cold and the chemical practises were used for a common purpose.

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## Notes

<sup>1</sup> Robert Boyle (1627-1691)-

<sup>2</sup> Boyle's list of unpublished writings: Robert Boyle, "Materialls & Addenda Desing'd towards the Structure & Compleating of Treatises already begun or written. January the 25<sup>th</sup> 1649/1650," in *Works of Robert Boyle*, eds Michael Hunter, Edward Davis, (London: Pickering and Chatto Publishers, 1999-2000), vol.14, 329.

<sup>3</sup> Boyle, *Works of Robert Boyle*, vol. 2, 205-378.

<sup>4</sup> Short title for "A Physico-Chymical Essay, containing an Experiment with some Considerations touching the differing Parts and Redintegration of Salt-Petre," published in the treatise *Certain Physiological Essays*, (1661). Boyle, *Works of Robert Boyle*, vol. 2, 93-113.



<sup>5</sup> One of the five essays published in *Certain Physiological Essays* (1661). Boyle, *Works of Robert Boyle*, vol. 2, 117-203.

<sup>6</sup> Boyle, *Cold* in Boyle, *Works of Robert Boyle*, vol. 4, 203-575.

<sup>7</sup> Some studies on Boyle's experimental treatise on cold E. Williams, "Some Experiments on the Expansive Force of Freezing Water," *Annals of Science* 10 (1954): 166-171; Marie B. Hall, "What happened to the Latin Edition of Boyle's *History of Cold*?" *Notes and Records of the Royal Society of London* 17 (1962): 32-35, Muriel A. Bentham, "Some Seventeenth Century Views concerning the Nature of Heat and Cold," *Annals of Science* 2, issue 4, (1937): 431-450 on 445-450; Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago/London: Chicago University Press, 1994), 243-266.

<sup>8</sup> Boyle, *Cold* in Boyle, *Works of Robert Boyle*, vol. 4, 221.

<sup>9</sup> Knowles W.E. Middleton, *History of the Thermometer and its uses in Meteorology*, (Baltimore/Maryland: The Johns Hopkins Press, 1966), 4.

<sup>10</sup> Francis Bacon, *The New Organon*, ed. Fulton H. Anderson (Indianapolis: The Library of Liberal Arts/Bobbs-Merrill Educational Publishing, 1960).

<sup>11</sup> For the use of the term "chymists" see Lawrence Principe, *The Aspiring Adept: Robert Boyle and His Alchemical Quest* (New Jersey: Princeton Press, 1998), 30-35.

<sup>12</sup> *Meteorology* for instance.

<sup>13</sup> One of the treatises Boyle discussed was Claude De Berigard's (1578-1663) *Circulus Pisanus*.

<sup>14</sup> For instance Descartes' *Meteorology* (1637) and *Principles of Philosophy* (1644), Gassendi's *Animadversiones in decimum Librum Diogeniis Laertiis* (1649) and *Syntagma Philosophicum* (1658) in *Opera Omnia* and Walter Charleton's *Physiologia Epicuro-Gassendo-Charltoniana* (1654).

<sup>15</sup> Boyle, *Cold* in Boyle, *Works of Robert Boyle*, vol. 4, 275.

<sup>16</sup> Boyle, *Cold* in Boyle, *Works of Robert Boyle*, vol. 4, 443-444.

<sup>17</sup> Boyle, *Sceptical Chymist* in Boyle, *Works of Robert Boyle*, vol. 2, 251-253.

<sup>18</sup> Boyle, *Cold* in Boyle, *Works of Robert Boyle*, vol. 4, 324.

<sup>19</sup> Boyle, *Cold* in Boyle, *Works of Robert Boyle*, vol. 4, 446-447.

<sup>20</sup> Boyle, *Cold* in Boyle, *Works of Robert Boyle*, vol. 4, 301-305.

<sup>21</sup> Boyle, *Cold* in Boyle, *Works of Robert Boyle*, vol. 4, 273-275, 275-278.

<sup>22</sup> Boyle, *Cold* in Boyle, *Works of Robert Boyle*, vol. 4, 265-273.

<sup>23</sup> Boyle, *Cold* in Boyle, *Works of Robert Boyle*, vol. 4, 357-360.

<sup>24</sup> Boyle, *Cold* in Boyle, *Works of Robert Boyle*, vol. 4, 269.

<sup>25</sup> Boyle, *Cold* in Boyle, *Works of Robert Boyle*, vol. 4, 376-381.

<sup>26</sup> Boyle, *Cold* in Boyle, *Works of Robert Boyle*, vol. 4, 364-381.

<sup>27</sup> Boyle, *Sceptical Chymist* in Boyle, *Works of Robert Boyle*, vol. 2, 330-331, 336-339.





Figure 1. The liquid sealed thermoscope depicted in "Fig. 3" of the 1683 edition of Boyle's *Cold*, Photos by Douglas A. Lockard. Roy G. Neville Historical Chemical Library, Chemical Heritage Foundation, Philadelphia, PA.

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## Between Physics and Chemistry: Early-Low-Temperature Research, 1877-1908

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The early period of low-temperature research, that is from 1877 to 1908, when all the so-called permanent gases were finally liquefied, is commonly considered to be the period during which another sub-branch of physics, that of low-temperature physics, emerged. Indeed, it is the case that, after the discovery of superconductivity in 1911 and of the various properties of liquid helium below 2.9 K in the early 1930s, research at the low temperatures was almost completely dominated by physicists. However, this was not so for the period during which the liquefaction of the so-called permanent gases dominated low temperature research. The most important developments in gas liquefaction took place in a period characterised by the application of thermodynamics on physical and chemical research, the reappraisal of chemical theory and the elaboration and acquisition of increasingly complex experimental apparatus and skills. It appears that chemists were as much as physicists actively involved in early low-temperature research. What, then, has been the role of the chemists in the development of a series of practices, which eventually formed a branch of physics?

In this paper an attempt is made to show some aspects of a more general thesis that is that the history of low-temperature research, and especially the period between the liquefaction of oxygen in 1877 and the liquefaction of helium in 1908, has been an integral part of the history of physical chemistry. The period to be examined is the period when physical chemistry was also in search of its own identity. It was a period when physical chemistry was articulating its own autonomous language with respect to both physics and chemistry, when it was charting its own research agendas and formulating its own theoretical framework. It was also a time when the sub-disciplinary boundaries were drawn and re-drawn, and when these processes were deeply influenced by the different cultures of physicists and chemists.

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The inclusion of early-low-temperature research in the history of physical chemistry has not been previously discussed in the literature of the field. Similarly, historians of low-temperature physics have paid almost no attention to the ways in which low-temperature research has been influenced by the attempts to establish physical chemistry as a separate discipline. The first section of this paper reviews some of the basic assumptions made and some of the main historiographical issues raised among historians of physical chemistry. It will be argued that one of the main preoccupations of historians has been the examination of the emergence of physical chemistry as a new discipline under the influence of Wilhelm Ostwald, Svante Arrhenius and Jacobus Van't Hoff. However, such an approach has overshadowed the complex procedures related to the development of physical chemistry. Evidence from primary sources is provided in order to show that the links between early-low-temperature research and physical chemistry were also acknowledged by contemporary workers.

The second section deals with some aspects of the history of early-low-temperature research. Starting with the liquefaction of oxygen in 1877 by Raoul Pictet and Louis Cailletet, it will be argued that the interplay of both physics and chemistry, physicists and chemists, held an important role in the production, maintenance and use of low temperatures. However, physical chemistry is not characterised by the mere application of physical methods into chemistry and vice versa. Although this reductionist view has been sustained by the contemporary scientists, the liquefaction of helium by Kamerling Onnes in 1908 demonstrates that the success of his project did not depend only on the development of techniques for lowering the temperature and for purifying gases. The liquefaction of helium was the result of a research programme built upon a solid theoretical knowledge on the behaviour of gases, and combined with systematic research in instrumentation and thermometry, issues that have been intrinsically connected to the agenda of physical chemistry.

### **Historians and the emergence of physical chemistry**

Historians traditionally identify the emergence of physical chemistry as a new discipline with the spectacular take-off of the work of Wilhelm Ostwald (1853-1932), Svante Arrhenius (1859-1927) and Jacobus Van't Hoff (1852-1911) in the 1880's.

Wilhelm Ostwald initiated his experimental work in the 1870s. He studied the problem of chemical affinity, paying special emphasis to electrochemistry and che-

mical dynamics. Critical of the descriptive character of chemistry, he aimed at redirecting the focus of chemists onto questions related to chemical change rather than on the nature of chemical compounds. His work was characterised by his determination to look for the general laws of chemical change, using physical techniques and presenting these laws in a mathematical language. Around the same period, in 1884, Svante Arrhenius submitted his doctoral thesis on the galvanic conductivity of electrolytes. His thesis demonstrated that electrolytes, when dissolved in water, dissociated into electrically positive and negative ions. The ions were supposed to be carriers of electricity and chemical activity. Arrhenius's thesis, although it almost failed to fully convince the faculty staff of the University of Uppsala, where it was submitted, gained quickly the support of Ostwald and Jacobus Van't Hoff. In fact, Van't Hoff used it in order to validate his own research on the extension of the laws of thermodynamics from gases to dilute solutions. This powerful ensemble of ideas and techniques –the new theory of solutions, the theory of electric dissociation and their approach through thermodynamics– that was created by the work of the three ionists (as Ostwald, Arrhenius and Van't Hoff were called) is considered to be the foundation stone of the discipline of physical chemistry.

The consolidation of the discipline was achieved through some well-orchestrated activities towards the institutionalisation of the field. From 1884, Wilhelm Ostwald wrote textbooks in which he attempted to promulgate his vision for a reformed chemistry, and numerous other scientific works on analytical chemistry, electrochemistry, and inorganic chemistry. Along with his writing of textbooks, he founded, in 1887, the first *Journal of Physical Chemistry* (the *Zeitschrift für physikalische Chemie*), and around the same time he was put in charge of the organisation of the Department of Physical Chemistry at the University of Leipzig. Similar developments took place in Great Britain and France, and contributed to the further establishment of physical chemistry. By the end of the nineteenth-century, the new discipline had gained important institutional space. Physical chemistry grew rapidly also in the United States, after some of Ostwald's American students gained positions and power in America's expanding University system.

The founders of the discipline used a carefully crafted rhetoric in order to promote and consolidate their field. As Diana Kormos Barkan has shown, the programmatic texts of the prime movers of physical chemistry stressed the links between physical chemistry and its ancestors.<sup>1</sup> In these works, physical chemistry was presented as the natural outcome of the 'progress' in both sciences of physics and che-

mistry, and their interaction through the systematic approach of research questions laying between the two sciences.<sup>2</sup>

Some historians, such as Erwin Hiebert and R.G.A. Dolby, have paid attention to the creation of the discipline of physical chemistry within the context of a growing professionalisation and the emergence of other sub-disciplines (such as pharmaceutical and medical chemistry, experimental psychology, etc.) during the second half of the 19<sup>th</sup> century.<sup>3</sup> Their approach was characterised by a careful examination of the scientific work and the network activities of the three ionists. Similarly, John Servos' seminal book on physical chemistry focuses almost exclusively on the growth of physical chemistry in the US.<sup>4</sup> In his introductory chapter, Servos briefly discusses the emergence of the discipline, placing emphasis on the discontinuities rather than the continuities between older scientific traditions and the physical chemistry of the 1880s. For Servos the physical chemists of the 1880s altered the balance between theory and experiment, affirmed the role and use of mathematics in chemistry, revealed the relations of chemistry with other sciences, and changed the topography of chemistry itself.<sup>5</sup>

Most of the secondary literature treats physical chemistry from either the point of view of its emergence as a new discipline, or the point of view of individual contributions to its main research topics such as the theory of solutions, chemical kinetics and chemical thermodynamics.<sup>6</sup> The story seems a straightforward and a rather unproblematic. Despite the analysis of various debates that inevitably sprung from the enforcement of the new theories to the scientific community, there is little reflection on the process of legitimisation of the language of physical chemistry and the praxis of its practitioners.<sup>7</sup>

The task to narrate the emergence of physical chemistry as a new discipline with respect to the works of the three ionists seems to have been accomplished. What is still missing is, perhaps, a better understanding of the rich ontological, epistemic and conceptual commitments that underpinned the emergence of physical chemistry. The emergence of physical chemistry as a new scientific field cannot be identified with the mere application of physical theory and techniques to chemistry. This reductionist view sounds far too simplistic to justify the emergence, consolidation and endurance of an autonomous discipline. Even the founders of the discipline did not give a clear answer to whether physical chemistry emerged as an attempt to unify chemistry under general principles, or whether they wanted to develop a new specialty that would stand apart from chemistry and would make use of physical methods in order to better understand chemical processes. As John Servos has pointed out, Ostwald's use of both the terms 'General chemistry' and 'Physical chemistry' is indicative of his ambivalence of whether physi-

cal chemistry could be conceived as part of chemistry, or reduced to the domain of physics.<sup>8</sup>

During the period under examination, there were plenty of investigations that fell into the borderland between physics and chemistry, such as the study of the physical properties of compounds: the measurements of specific heats, the index of refractivity, the rotation of polarised light etc. During the same period we have the emergence of a new research field –the low temperatures– created by the liquefaction of the permanent gases. Both chemists and physicists, employing chemical and physical techniques, became interested in using the low temperatures for probing into the properties of matter and extending their theoretical understanding of these properties. Despite the importance of the development of highly complex physical and chemical techniques for lowering the temperature and for purifying gases, early-low-temperature researchers seems to have participated in, and contributed to the articulation of a new theoretical framework that was to accommodate the latest developments in the theory of thermodynamics and its various applications.

### **Low-temperature research and physical chemistry in primary sources**

An obvious place to look for connections between low-temperature research and physical chemistry would be in the newly founded journals of Physical Chemistry. However, it is not surprising if such information is not readily available. We must not forget that the early journals on physical chemistry were instrumental in the consolidation of the emerging discipline, but also for the promotion of the research agendas of its editors. Therefore, Ostwald's journal of physical chemistry founded in 1887 was mainly concerned with the main topics dealt by his research school. Similarly, Wilder Bancroft's *Journal of Physical Chemistry*, founded in 1896 at Cornell University, reflected its editor's rather idiosyncratic conception of physical chemistry, which stressed the need for a qualitative understanding of its principles and its usefulness in industry and medicine.<sup>9</sup> However, papers on liquefaction and the thermodynamic properties of gases and gaseous mixtures were occasionally accommodated in both journals.<sup>10</sup>

Such papers were also included in the Swiss *Journal de Chimie Physique*. Correspondence between the editor of the journal, the chemist Philippe Guye (1862-1922) and the Dutch physicist Heike Kamerlingh Onnes (1853-1926), is again indicative that research in the low temperatures was not considered unre-



lated to physical chemistry. Philippe Guye had asked Kamerlingh Onnes to lend his support to his journal not only by allowing him to include his name among the principal collaborators of the journal, but most importantly by sending him articles on the work conducted at the Leiden Cryogenic Laboratory.<sup>11</sup> Guye wanted to include in his journal articles related to physical chemistry, especially articles on the equation of state, a 'question that he wished to treat extensively in the journal'.<sup>12</sup>

Perhaps the most convincing evidence is to be found in general journals. The fact that both physicists and chemists were involved in early-low-temperature research is made evident from an examination of articles that were related to the low temperatures published in the *Comptes Rendus (CR)* of the Academy of Sciences in Paris. From 1877 to 1908 low-temperatures related articles were classified under both the categories of 'chemistry' and 'physics'. These articles referred to the liquefaction of gases, the use of special instrumentation and thermometric methods, the study of the properties of matter at low temperatures, and the application of the low temperatures in industry. Although most of these articles fell under the headings of 'chemistry' and 'physics', it is hard to identify them as 'purely' chemical or 'purely' physical research. This apparent difficulty is partially resolved in 1901, when the category 'physical chemistry' appears for the first time in the *CR*, and under which some of these articles (especially articles dealing with the measurement of the densities of liquid gases at low temperatures as well as the measurement of atomic volumes and molecular weights) were thereafter classified.<sup>13</sup>

Interesting information is derived when one attempts to classify the authors of the scientific papers inserted in the *CR* according to their background. Although one can single out a greater number of chemists compared to physicists working in the low temperatures, the difficulty of identifying someone as a physicist or a chemist reinforces the hypothesis that the development of research in the low temperatures, at least for sometime, could have played an important role in the making of the culture of physical chemistry. What remains to be seen is how research in the low temperatures has contributed to the emergence of physical chemistry. A brief examination of some aspects of the history of low temperature research may shed light on the way in which the challenge of the liquefaction of the permanent gases led to the articulation of a theoretical framework, a theoretical agenda and an autonomous language with respect to both physics and chemistry.

## **Aspects of early low-temperature research and the emergence of physical chemistry**

The liquefaction of oxygen by the French Louis Cailletet (1823-1913) and the Swiss Raoul Pictet (1846-1926), simultaneously in 1877, is often considered to be the date of the birth of low-temperature research. The first systematic attempts to liquefy gases are traced back to Michael Faraday's liquefaction of chlorine in 1823. Until 1845, Faraday had managed to liquefy almost all known gases, apart from six that were then named the 'permanent gases'. These were the oxygen, nitrogen, hydrogen, nitrogen dioxide, methane, and carbon monoxide. Until 1877, attempts to liquefy the so-called permanent gases had all failed. The Austrian physician Johannes Natterer (1821-1901) had attempted liquefaction by the exertion of pressures reaching up to the enormous number of 3,000 atmospheres. Similarly, the French chemist Marcelin Berthelot (1827-1907) had attempted to liquefy some of the permanent gases by applying pressures of over 800 atm without success.

The key to the liquefaction of the permanent gases was given by the experimental work of the Irish chemist, Thomas Andrews (1813-1885). Andrews's work was mainly concerned with the calorimetry of chemical reactions, and the properties and constitution of ozone. However, around the late 1850s and probably through the influence of James Thomson, he became interested in the liquefaction of the permanent gases.<sup>14</sup> He undertook the study of the gas-liquid equilibrium using carbon dioxide. In this study, Andrews pointed to the existence of a critical point, which corresponds to a critical volume, a critical pressure and most importantly a critical temperature, at which point the phases of the substance are not distinguishable. Above the critical point, even the highest pressure cannot yield liquefaction. In 1869 Andrews submitted a paper for the Bakerian lecture of the Royal Society, where he reported his experiments on carbon dioxide and stated his belief on the continuity of the gaseous and liquid states of matter.<sup>15</sup>

Andrews's experimental results were interpreted in terms of molecular physics in Johannes Van der Waal's thesis, defended in 1872 under the same title as Andrews's Bakerian lecture. Van der Waal's worked substantially on the law of the ideal gas. The reformulation he provided gave a fairly adequate explanation of Andrew's experimental results, and demonstrated the continuity of the transition from the gaseous to the liquid state. The great virtue of Van der Waals's equation was that it applied to every substance. Some years later, in 1880, Van der Waals was able to show not only the continuity of the transition from one state of aggregation to another, but also their identity. His formulation of the law of corresponding states implied a similarity among all gases and liquids.<sup>16</sup>



Various experimentalists, especially in France, played an important role in consolidating the significance of the critical point after Andrew's and Van der Waal's work.<sup>17</sup> Emile Amagat, perhaps the most systematic among them, started his experimental work examining the influence of temperature on the deviations from Boyle's law. His work was purely experimental, and he did not proceed to any theoretical claims. By the early 1890s he had compiled his data in 70 comprehensive tables, which were widely used by his contemporaries.<sup>18</sup>

Before going into the implications of Van der Waals's work for low-temperature research, it is useful to first take a look into the liquefaction of oxygen. Upon the simultaneous announcement of the liquefaction of oxygen by Raoul Pictet and Louis Cailletet at the Academy of Sciences in Paris, it was primarily chemists who stepped in to applaud the achievement. Jean-Baptiste Dumas, reminded them of the prediction of Lavoisier, the 'immortal creator of modern chemistry', that air, or at least part of its constituents, could be liquefied at very low temperatures.<sup>19</sup> Sainte-Claire Deville testified that he was present at Cailletet's experiments, conducted in his chemical laboratory at the Ecole Normale.<sup>20</sup> Marcelin Berthelot praised Cailletet for his systematic work that led to the liquefaction of the permanent gases, and used Pictet's results in order to lend support to some of his own views on high-pressure chemistry.<sup>21</sup> But how did research in the low temperatures relate to both physics and chemistry for these early experimentalists? Both Raoul Pictet and Louis Cailletet, although they followed different paths to achieve the liquefaction of oxygen, had clear understandings of the importance of the critical point. Let us not forget that in the minds of people Van der Waals's equation is a reformulation of an equation that is known by the name of the doyen of chemistry, Robert Boyle.

After studying physics and chemistry in Geneva and Paris, Pictet returned to his hometown and started experimenting in the low temperatures, while at the same time he followed closely the lucrative refrigeration industry.<sup>22</sup> His interest in the liquefaction of gases stemmed also from a wider theoretical concern about the constitution of bodies. His extended memoir on the liquefaction of oxygen, published in the *Annales de chimie et physique* in 1878, includes a rather long exposition of his microscopic approach to the laws of nature, within the context of the mechanical theory of heat and Clausius's kinetic theory of gases.<sup>23</sup>

For Pictet, the identification of heat with motion required an understanding of the laws that govern the behaviour of the ultimate particles of matter. His approach was non-mathematical, mostly qualitative and highly speculative. He often complained that the use of mathematics obscured the physical meaning of the problems under examination, and sought to present his work with a minimum use of

mathematics and in simple terminology.<sup>24</sup> He resorted to a mechanical explanation of physical theory, making use of a molecular model with a specific configuration of the particles and the forces that acted between them, very close to the one conceived by Van der Waal's. Pictet considered bodies to be constituted of impermeable molecules, submitted to the force of cohesion, and set in motion. The liquefaction of gases was usually achieved by mere compression, during which the particles of the gas were brought close enough for the force of cohesion to act upon them. Brought to a certain distance, and upon the influence of the force of cohesion, the molecules of the gas precipitated one upon another and formed a liquid. In order to explain the impossibility of liquefying all gases by mere pressure, Pictet argued that it was the motion of the particles of the body, which counteracted the force of cohesion. Since temperature was directly related to the motion of the particles, he reasoned that the lowering of the temperature of the body would reduce the motion of the particles. Two conditions had, therefore, to be fulfilled for the liquefaction of gases: the exertion of great pressures, and the obtaining of great cold.<sup>25</sup> Although, Pictet must have been aware of Andrew's and probably Van der Waal's work, he never mentioned them explicitly. He only once referred to the term 'critical point'; a concept, he believed, that was logically deduced from his own theory.<sup>26</sup>

The liquefaction of oxygen was presented by Pictet as a proof of the validity of his microscopic theory of matter. Despite his non-rigorous theoretical exposition, Pictet provided an exhaustive description of the experimental apparatus and set-ups employed. His virtuosity in experimentation and dexterity in the construction and manipulation of his experimental apparatus was highly appraised. Even Victor Regnault was highly impressed by Pictet's remarkable experimental apparatus.<sup>27</sup> In fact, it was his liquefaction of oxygen, the experimental apparatus and the method employed, that seemed to be of interest at least to the French scientific community rather than his speculations about the nature of bodies and the action of the force of cohesion.

After the liquefaction of oxygen, and his unsuccessful attempts to liquefy hydrogen, Pictet used his skills in the obtaining of low temperatures in order to probe into the properties of matter and further his theoretical investigations.<sup>28</sup> His interests lay in the domains of both physics and chemistry, and rested on the hypothesis that all physical and chemical phenomena result from the interplay of two attractions, namely, the attraction of matter to matter, and the attraction of matter to the ether. His investigations were characterised by strong ontological and methodological commitments. He believed that it was possible to account for all physical phenomena on a celestial, terrestrial, and molecular scale in terms of

central forces between particles. His main concern was to give thermodynamics a physical imagery, making a minimal use of mathematics, but seconding his reasoning by empirical data.<sup>29</sup>

Although Pictet's theories had no impact on his contemporaries, his preoccupations may be considered as relevant to the emergence of physical chemistry. In Pictet's mind, research in the low temperatures clarified issues about the constitution of bodies, gave a physical imagery to the laws of thermodynamics, and submitted the study of physical and chemical phenomena under the same principles. It was not a matter of reducing chemistry to the laws of physics, rather than articulating a framework within which both physical and chemical phenomena were treated in a similar fashion.

Let us now pass to the second experimentalist, Louis Cailletet, who had liquefied oxygen independently and almost simultaneously with Pictet. Contrary to Pictet, Cailletet's initial interest in the liquefaction of gases seems to be almost exclusively due to the experimental challenge imposed, rather than for any theoretical concerns. His interest in gases had its roots in the training he received by the chemist Henri Sainte-Deville, and his first professional steps as an industrialist.

Cailletet attended courses at the École de Mines as an 'auditeur libre', and frequented the chemical laboratory of Henri Saint-Claire Deville. However, he soon returned to his birthplace, Châtillon-sur-Seine, to work at his father's ironworks. There, he continued to pursue his scientific interests that were closely connected to Saint-Claire Deville's work on the phenomenon of dissociation.<sup>30</sup> Work on dissociation had revealed that there was a strict analogy between chemical decomposition and the change of the physical state of matter, when it passed into the solid, liquid and gaseous condition.<sup>31</sup> Cailletet provided supportive evidence for his mentor's theory by having the gases suddenly cooled at the moment of their collection, and demonstrated that at very high temperatures these gases were indeed dissociated to their elements.<sup>32</sup>

His interest in the liquefaction of gases began from his research on liquid carbon dioxide.<sup>33</sup> A few years later, in 1877, he attempted and succeeded the liquefaction of acetylene by pure pressure.<sup>34</sup> During his work with acetylene an accidental leak of the pressurised gas from a tap caused a sudden cooling of the gas, which led to momentary condensation. The production of abrupt cold by the expansion of a compressed gas, led Cailletet to contemplate upon the possibility of using such a technique in the liquefaction of the permanent gases.<sup>35</sup>

Cailletet's research on chemical phenomena at high pressures, and the compression of gases led him gradually to get interested in the liquefaction of gases. His

work on chemistry, the dexterity he obtained with his involvement in high-pressure chemistry, as well as his expertise in building apparatus for the manipulation of such phenomena, were all useful experience for his later work. Contrary to Pictet, Cailletet's experimental work was followed neither by theoretical claims, nor by ontological commitments.<sup>36</sup> Although he was aware and made use of the notion of the critical point, he never made any claims about the underlying ontology in Van der Waal's work.

As already mentioned, the estimation of the critical points was crucial for the liquefaction of gases, and many experimentalists were drawn into the study of isotherms in order to calculate the critical temperatures under which liquefaction was achievable. The liquefaction of hydrogen posed an even greater experimental challenge since its critical temperature was estimated to be around  $-243^{\circ}\text{C}$ . It was finally the British chemist James Dewar who succeeded its liquefaction in 1898. For this, Dewar made extensive use of Zygmunt Wroblewski's deductions regarding the critical point of hydrogen following from a study of the isothermals of the gas. In his paper "Liquid Hydrogen", Dewar stated that Wroblewski's results were "a signal of triumph for the theory of Van der Waals and a monument to the genius of the Cracow physicist".<sup>37</sup>

Despite the occasional references to the importance of Van der Waals's work in Dewar's writings, Kostas Gavroglu has argued that Van der Waals's considerations played only a very small part in Dewar's experimental work. The British chemist had not published a single theoretical work, nor was there any reference to the measurement of isotherms despite the reporting of the values of a large number of physical parameters at low temperatures.<sup>38</sup> In order to demonstrate the decisive role of Van der Waals's early work in the development of the field of the low temperatures, Gavroglu used as an example, Dewar's failure to liquefy helium and compared it to the research programme of the Dutch physicist, Heike Kamerlingh Onnes.

Just after his appointment to the Chair of Experimental Physics at the University of Leiden in 1882, Heike Kamerlingh Onnes established the first cryogenic laboratory and initiated his work on the low temperatures with an aim to liquefy hydrogen. When Dewar managed to liquefy hydrogen, Kamerlingh Onnes turned to the liquefaction of the newly-discovered gas helium. Contrary to Dewar's lack of programmatic claims, Kamerlingh Onnes's main motivation for the liquefaction of helium was not the experimental challenge imposed but the provision of supporting evidence to Van der Waal's law of corresponding states, and the generalisation he himself provided in 1881.<sup>39</sup> Kamerlingh Onnes did not ascribe only to the phenomenological implications of Van der Waals's theory but also to its onto-

logical and methodological commitments. In his paper 'General theory of the fluid state' where he discussed the law of corresponding states, he suggested that corresponding states could be visualised as states of similar molecular movement. In other words, the motion of the molecules of substances at the corresponding states, was considered to be dynamically similar.<sup>40</sup> Like Van der Waals, Kamerlingh Onnes was interested in drawing conclusions from physical-mechanical-arguments rather than sophisticated mathematics.<sup>41</sup>

In practice this meant that one could explore the possibilities of liquefying gases by applying the law of corresponding states to the results of working with other substances in higher temperatures. The liquefaction of helium in 1908 was achieved through the systematic research of Kamerlingh Onnes based on the work started by Van der Waals. Around 1900 Kamerlingh Onnes had started investigating the isotherms of monoatomic, diatomic gases and their binary mixtures. These investigations, along with considerable improvement of the cryogenic apparatus, led the Dutch physicist to arrive at a fairly good estimate of the critical point of helium, which was necessary prior to any attempt to liquefy it.<sup>42</sup>

As Kostas Gavroglu has shown, the measurements of isotherms had been decisive for the liquefaction of helium. For Kamerlingh Onnes the liquefaction of helium was not an aim by itself, but a proof of the validity of the law of corresponding states. On the contrary, Dewar's work on the liquefaction of gases seems not to have been guided by strong theoretical considerations. Dewar did not explore the possibilities offered by the law of corresponding states. In the case of helium, his 'brute force' approach did not prevail. Dewar's approach to low-temperature research is delineated in his presidential address to the Society of Chemical industry in Glasgow. There, he referred to the two schools of chemistry: one of organic chemistry, and another in which physical chemistry was predominant. According to Dewar, physical chemistry examined the physical relations of chemical action, as well as the effect of the physical action of the constituents taking part in chemical changes.<sup>43</sup> For Dewar, chemical changes were considered to be parallel and similar to physical operations, and therefore measurable with physical accuracy.<sup>44</sup> The liquefaction of gases was a good example of the analogy between chemical and physical transitions, while research in the low temperatures followed strictly the consideration of the physical side of chemistry.<sup>45</sup> However, Dewar never went beyond the notion of physical chemistry as a way of adopting physical techniques for chemistry.<sup>46</sup>

On the contrary, for Kamerlingh Onnes the law of corresponding states was not only a useful tool for the estimation of the critical points, but provided also a completely novel way of dealing with phenomena lying between physics and che-

mistry.<sup>47</sup> Like Pictet's rather crude considerations, Kamerlingh Onnes's work was not characterised by the mere application of physical laws in the molecular scale, but by the articulation of a framework within which both physical and chemical phenomena were treated in a similar fashion.

### **Concluding remarks**

This paper attempts to present some preliminary results concerning the emergence of low-temperature research within the context of physical chemistry. Traditionally historians identify the emergence of physical chemistry with the work of Wilhelm Ostwald, Svante Arrhenius and Jacobus Van't Hoff on the theory of solutions, chemical kinetics and chemical thermodynamics in the 1880s. The institutionalisation of the field was achieved through the establishment of specialised journals, and research departments. Current historiography on physical chemistry focuses mostly on the emergence of this new research field as a new discipline, and on individual contributions to its main research areas.

It has been shown that other research areas, and especially low-temperature research, have also contributed to the establishment of physical chemistry as an autonomous field in respect to both physics and chemists. Such a thesis questions also the commonly assumed position that the various developments in early low temperature led in a straightforward manner to the establishment of a new branch of physics, that of low-temperature physics.

The involvement of both chemists and physicists in low-temperature research concerning the low temperatures is indicative of a continuous negotiation about the identity of this emerging field. Although the first successful attempts to liquefy the permanent gases were mostly due to the employment of complex experimental apparatus and skills, the liquefaction of helium by Kamerlingh Onnes in 1908 was not simply a matter of improving the existing techniques of liquefaction. The liquefaction of helium was the outcome of the study of a series of theoretical issues explicitly expressed or implied by the work of Johannes Diderik Van der Waals on the equation of state and the law of corresponding states. Kamerlingh Onnes and his collaborators in the cryogenic laboratory of the University of Leiden developed a rather characteristic culture and characteristic laboratory practices in which theoretical research was in direct relationship to experimental studies and vice versa.

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## Notes

<sup>1</sup> Diana Kormos Barkan, "A Usable Past: Creating Disciplinary Space for Physical Chemistry," in *The Invention of Physical Chemistry*, eds Mary Joe Nye et al. (Dordrecht: Kluwer, 1992), 175-202.

<sup>2</sup> It is interesting to note that the only recent accounts of a general history of physical chemistry, written by authors trained as physical chemists, trace the origins of the field in past scientific activity dating back to Robert Boyles's natural philosophy. See Keith Laidler, *The World of Physical Chemistry* (Oxford, New York: Oxford University Press, 1993) and Cathy Cobb, *Magick, Mayhem and Mavericks. The Spirited History of Physical Chemistry* (Amherst, NY: Prometheus books, 2002).

<sup>3</sup> Erwin Hiebert, "Developments in Physical Chemistry at the Turn of the Century," in *Science, Technology and Society in the Time of Alfred Nobel*, eds Carl Gustav Bernhard et al. (Oxford: Pergamon Press, 1982), 92-115; R.G.A. Dolby, "The case of physical chemistry," in *Perspectives on the Emergence of Scientific Disciplines*, eds G. Lemaine et al. (Hague: Mouton, 1976), 63-73.

<sup>4</sup> John Servos, *Physical Chemistry from Oswald to Pauling: The Making of Science in America* (Princeton: Princeton University Press, 1990).

<sup>5</sup> Servos, *Physical Chemistry*, 40.

<sup>6</sup> See for example H.A.M. Snelders, "The Dutch Physical Chemist J.J. van Laar (1860-1938) versus J.H. van't Hoff's 'osmotic school'," *Centaurus* 29 (1986): 63-71 and Helge Kragh, "Before Quantum Chemistry: Erich Hückel and the Physics-Chemistry interface," *Centaurus* 43 (2001): 1-16.

<sup>7</sup> See for example: R.G.A. Dolby, "Debates over the theory of solutions," *HSPS* 7 (1976): 297-404 and "Thermochemistry vs Thermodynamics: the 19<sup>th</sup> century controversy," *History of Science* 22 (1984): 375-400.

<sup>8</sup> John Servos, "A Disciplinary Program that Failed: Wilder D. Bancroft and the Journal of Physical Chemistry, 1896-1933," *ISIS* 73 (1982): 230.

<sup>9</sup> Servos, "A disciplinary program", 3.

<sup>10</sup> See for example: Pierre Duhem, "On the liquefaction of a mixture of two gases," *Journal of Physical Chemistry* 5 (1901): 91-112; F.G. Cottrell, "On air liquefiers," *Journal of Physical Chemistry* 10 (1906): 264-274.

<sup>11</sup> Letter 30 avril 1903 from Philippe Guye to Heike Kamerling Onnes. Museum of Boerhaave, archives of Kamerlingh Onnes, inv. no 297.

<sup>12</sup> Letter 9 October 1903 from Philippe Guye to Heike Kamerling Onnes. Museum of Boerhaave, archives of Kamerlingh Onnes, inv. no 297 (my translation from French).

<sup>13</sup> See for example: Ph.-A. Guye, Ed. Mallet, "Constantes critiques et complexité moléculaire d'hydrocarbures élevés," *CR* 133 (1901): 1287; A. Ponsot, "Chaleur spécifique des corps au zéro absolu," *CR* 134 (1902): 703; Ph.-A. Guye, "Application de la méthode des densités limites aux gaz liquéfiables," *CR* 147 (1907): 1360; Joseph de Kowalski, "La phosphorescence progressive à basse température," *CR* 151 (1910): 810.



- <sup>14</sup> Rowlinson, J.S., "The work of Thomas Andrews and James Thomson on the liquefaction of gases," *Notes Rec. R. Soc. Lond.* 57 (2003): 143-159.
- <sup>15</sup> Thomas Andrews, "On the continuity of the gaseous and liquid states of matter," *Phil. Trans. R. Soc. Lond.* 16 (1869): 575-590.
- <sup>16</sup> For van der Waals's work see A. Ya. Kipnis, B. E. Yavelov and J.S. Rowlinson, *Van der Waals and Molecular Science* (Oxford: Clarendon Press, 1996).
- <sup>17</sup> On the debates over the existence of the critical point see Johanna Levelt Sengers, *How fluids unmix. Discoveries by the School of Van der Waals and Kamerlingh Onnes* (Amsterdam: Koninklijke Nederlandse Akademie van Wetenschappen, 2002).
- <sup>18</sup> Mentioned in Louis Cailletet, "Prix La Caze (Physique)," *CR* 117 (1893): 901-2.
- <sup>19</sup> Jean-Baptiste Dumas, "Liquéfaction de l'Oxygène," *CR* 85 (1877): 1212.
- <sup>20</sup> Henri Sainte-Claire Deville, "Liquéfaction de l'Oxygène," *CR* 85 (1877): 1217.
- <sup>21</sup> Marcellin Berthelot, "Liquéfaction de l'Oxygène," *CR* 85 (1877): 1219 and "Nouvelles observations sur le rôle de la pression dans les phénomènes chimiques," *CR* 85 (1877): 1219.
- <sup>22</sup> "Pictet, Raoul," *DSB*, vol. 10, 603.
- <sup>23</sup> Raoul Pictet, "Mémoire sur la liquéfaction de l'oxygène," *ACP* 13 (1878): 145-229.
- <sup>24</sup> Raoul Pictet, "Application de la théorie mécanique de la chaleur à l'étude des liquides volatils. Relations simples entre les chaleurs latentes, les poids atomiques et les tensions des vapeurs," *Bibliothèque Universelle et Revue Suisse- Archives des Sciences Physiques et Naturelles* 55 (1876), 66-83.
- <sup>25</sup> Raoul Pictet, "Application de la théorie mécanique de la chaleur," 145-159.
- <sup>26</sup> Pictet, "Application de la théorie mécanique de la chaleur," 225.
- <sup>27</sup> Victor Regnault, "Observations sur la liquéfaction de l'oxygène," *CR* 85 (1877): 1219.
- <sup>28</sup> Raoul Pictet, "Études des phénomènes physiques et chimiques sous l'influence des très basses températures," *CR* 114 (1892): 1245-47.
- <sup>29</sup> Raoul Pictet, "Essai d'une méthode générale de synthèse chimique," *CR* 115 (1892): 711.
- <sup>30</sup> Louis Cailletet, "De la dissociation des gaz dans les foyers métallurgiques," *CR* 62 (1866): 891-95.
- <sup>31</sup> Many of Sainte-Claire Deville's contemporaries saw his work as part of physical chemistry. See James Dewar, "Presidential Address to the Society of Chemical Industry," in *Collected Papers of Sir James Dewar*, ed. Lady Dewar, (Cambridge: Cambridge University Press, 1927), vol.1, 312; Pierre Duhem, "Une Science Nouvelle: la Chimie Physique," *Revue philomathique de Bordeaux et du Sud-Ouest* (1899): 262; William Ramsay, "Introduction to the study of physical chemistry," in Sydney Young, *Stoichiometry* (London: Longmans, Green and Co, 1908), xxvi.
- <sup>32</sup> Louis Cailletet, "Sur la composition et l'emploi industriel des gaz sortant des foyers métallurgiques," *CR* 83 (1877): 955-57.
- <sup>33</sup> Louis Cailletet, "Recherches sur l'acide carbonique liquide," *CR* 75 (1872): 1271-1274.
- <sup>34</sup> Louis Cailletet, "Sur la liquéfaction de l'acétylène," *CR* 85 (1877): 851-852.
- <sup>35</sup> Louis Cailletet, "Recherches sur la liquéfaction des gaz," *ACP* 15 (1878): 142.
- <sup>36</sup> Cailletet, "Recherches sur la liquéfaction des gaz," 132-144.
- <sup>37</sup> James Dewar, "Liquid Hydrogen," *Proc. Roy. Inst.* xvi (1899): 4..
- <sup>38</sup> Kostas Gavroglu, "The reaction of British physicists and chemists to Van der Waals' early work and to the law of corresponding states," *HSPBS* 20 (1990), 229.
- <sup>39</sup> Heike Kamerlingh Onnes, "On the cryogenic laboratory at Leiden and on the Production of Very Low temperatures," *CPL* 14 (1894): 4.



<sup>40</sup> Heike Kamerling Onnes, “Théorie générale de l’état fluide,” in *Through Measurement to Knowledge. The selected papers of Heike Kamerlingh Onnes, 1853-1926*, eds Kostas Gavroglu et al., (Dordrecht: Kluwer Academic, 1991), 89-124.

<sup>41</sup> Kostas Gavroglu, Yorgos Goudaroulis, “The remarkable work of ‘Le gentleman du zero absolu,’” in *Through Measurement to Knowledge*, xl.

<sup>42</sup> Gavroglu, Goudaroulis, “The remarkable work of “Le gentleman du zéro absolu,” xlv.

<sup>43</sup> James Dewar, “Presidential Address to the society of Chemical Industry,” in *Collected Papers*, vol.1, 307.

<sup>44</sup> James Dewar, “Presidential Address,” 309.

<sup>45</sup> James Dewar, “Presidential Address,” 312.

<sup>46</sup> Kostas Gavroglu, “Controversies and the Becoming of Physical Chemistry,” in *Scientific Controversies. Philosophical and Historical Perspectives*, eds P. Machamer et al., (NY, Oxford: Oxford University Press, 2000), 193.

<sup>47</sup> Kostas Gavroglu, “The reaction of British physicists and chemists,” 237.

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## Thermochemistry: The Meeting Point of Physics, Chemistry and Mechanics. The Thermochemical Laboratory of Moscow University and W. F. Louguinine

*Elena A. Zaitseva,\* Galina I. Liubina\*\**

Already in XVIII<sup>th</sup> and the beginning of XIX<sup>th</sup> century there appeared the works which later lead to the formation of thermochemistry as a science. Chemical affinity, as a special ability of substances to have specific selective interactions, attracted the close attention of many scientists of XVIII<sup>th</sup> century. They persistently looked for methods of measurement of chemical affinity forces of various substances. Nearly at the same time was established a close connection between chemical and thermal phenomena: chemical transformations of substances were nearly always accompanied by thermal effects. The question, whether it was possible to measure chemical affinity forces by determining the thermal effects, suggested itself. Thus arose, the preconditions for appearance of thermochemistry, that is, the science of thermal effects accompanying chemical and physico-chemical phenomena.

### **Thermochemistry: the meeting point of physics and chemistry. The formation of the theoretical basis of thermochemistry and development of measuring methods in the XVIII<sup>th</sup> to the middle of XIX<sup>th</sup> century**

In the middle of XVIII<sup>th</sup> century Joseph Black (1728-1799), Scottish physicist and chemist, constructed one of the first calorimeters and used it to make measurements of the heat capacity of substances and heats of evaporation and of fusion.<sup>1</sup> He also conducted pioneer research that showed the presence of latent heat, which is absorbed or released by a substance in course of change from one aggregate state to another. From 1766 until 1799, Black was Professor of Chemistry at the University of Edinburgh. His lecture notes were published posthumously by John Robinson. They contain detailed description of Black's experiments in ther-

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mochemical research. In particular, experiments in course of which he discovered that when mixing ice (at temperature of 32 °F) with equal quantity of water warmed up to 172 °F, the ice began to melt, but temperature of ice and water mixture did not raise to 102 °F, as one could have supposed. The melting of ice, concluded Black, absorbs a lot of heat, but the action of this heat results only in transformation of ice into water, which is not warmer than ice, from which it appeared. These experiments led him to an idea of the existence of latent heat, and this was later confirmed by him and his pupils in course of conducting research with use of other substances (“spermaceti and bees-wax”, etc.).<sup>2</sup> These experiments of Black prepared the ground for rigid scientific calorimetric investigations that were first conducted at the end of XVIII<sup>th</sup> century.

Early research in this sphere also includes that of Antoine Laurent Lavoisier (1743-1794) and Pierre-Simon Laplace (1749-1827), set out in their *Mémoire* about heat (1780).<sup>3</sup> Extending his research on phenomena of combustion and breath of animals, Lavoisier aimed to measure the quantity of heat released as a result of various chemical phenomena, mostly for combustions. Together with Laplace he constructed an ice-calorimeter<sup>4</sup> with the help of which determined the heat capacity of solids and liquids, heats of reaction of mixing two liquids, for example, sulfuric acid with water and in particular, heats of combustion.<sup>5</sup> Of course, the data was not precise. But the historical importance of their work lies not in its experimental preciseness, but in proposing a new method of defining heat, released in the course of chemical interactions, that served as a basis for calorimetry. Certain modifications of this method were made in 1834 by a Moscow chemist, Hermann,<sup>6</sup> who designed a calorimeter that measured not only the weight of water, originating from melting of ice, as in the device of Lavoisier and Laplace, but the change in volume of the water and ice mixture. Melting of ice leads to decrease in volume that can be measured. Using his calorimeter Hermann measured thermal capacity of 47 various substances. As W. F. Louguinine (see below) later observed, the ice calorimeter became quite popular in thermochemical measurements as the device has “high sensitivity, allowing to measure quite small amounts of heat”.<sup>7</sup>

The works of Lavoisier and Laplace also have high theoretical importance. Their conclusion that the heat of a chemical reaction had a characteristic value for the formation of every compound was of exceptional importance for further development of thermochemistry. These scientists also formulated the first law of thermochemistry in the following format: *all changes in heat, which are experienced by a system of bodies in course of the transformation from one state to another, are made in reverse order when the system returns to its initial state.*

This general conclusion was indirectly based on the concept of energy conservation. As it is known, it took several decades more for the law of energy conservation to receive its theoretical and experimental proof. Thus, one may consider that thermochemists were in advance of physicists in solution of this cardinal physical problem.

From 1830 to 1850 a number of various, but based on a uniform system, calorimetric investigations were conducted by the Germain Henri (German Ivanovich) Hess (1802-1850),<sup>8</sup> who lived in St.-Petersburg.<sup>9</sup> Hess' thermochemical research was connected with certain theoretical idea, expressing his general view of the atomic structure of chemical compounds. He wanted to try to extend atomic notions to the sphere of thermochemical phenomena. The main idea was to discover the law of multiple heat relations, similar to the law of John Dalton, in new sphere of chemistry. He did not manage to do it, as the precondition was false. But in course of experiments he came to an idea of measuring the force of chemical affinity via the definition of the process' thermal effect. The importance of this idea is obvious. It introduced into chemistry an experimental measure of chemical affinity allowing to foresee both fundamental possibility of occurrence of chemical reactions and the directions of chemical processes. At the same time he formulated the *basic law of thermochemistry* (1840), in the following form: *when some chemical compound is formed, it is always connected with release of the same quantity of heat independent of the fact, whether such formation is made directly or indirectly in several stages.*<sup>10</sup> Today in all educational programmes for chemistry students in Universities the law is stated as follows: *thermal effect of chemical transformation is determined only by initial and final state and does not depend on intermediate states of a substance.* Hess finished his research on the substantiation of the law of constancy of amounts of heat in 1842, i.e. in the year that Julius Robert von Mayer formulated the law of the conservation of energy.<sup>11</sup> Five years later (1847) Hermann L. F. von Helmholtz showed that the law of Hess expressed the principle of conservation of energy as applied to chemical processes.

### **Thermochemistry: the meeting point of physics, chemistry and mechanics. Formation of the theoretical basis of thermochemistry and methods and new devices for measurement in the second half of XIX<sup>th</sup> century**

The history of thermochemistry in second half of XIX<sup>th</sup> century is connected mostly with works of Marcellin (or Marcelin) Pierre Eugène Berthelot (1827-1907) in Paris and Hans Peter Jørgen Julius Thomsen (1826-1909) in Copenhagen. The amount of research conducted by them was great.

Thomsen was the first who applied the mechanical theory of heat to chemical phenomena. In 1853-1854 he published a series of works under the title “Die Grundzüge eines thermochemischen Systems”, in which he suggested two most important provisions:

*Affinity of two bodies becomes apparent in their ability for direct compounding. When such compounding occurs, a certain quantity of heat is released, in proportion to affinity of two bodies.*

*Quantity of heat released in course of chemical reaction can serve as a value of chemical power (energy).<sup>12</sup>*

This means that the main idea in his works, as well as in works of Hess, was the concept that the thermal effect of chemical reaction was a measure of the affinity of the reacting substances. In particular, in papers of 1860s he grounded the principle, according to which every chemical alteration, happening without use of energy from outside and leading to formation of certain substance or system of substances, was accompanied by calorification. This conclusion, close to so-called principle of maximum work (discussed below), was the first attempt to predict the direction of chemical reactions on the basis of thermochemical data. Later Thomsen came across a number of facts that contradicted this principle. His publications on thermochemistry were collected afterwards in a multivolume edition “Thermochemische Untersuchungen”.<sup>13</sup> In this series of volumes one can clearly see the change in Thomsen’s views on the thermochemical theory of affinity, which happened under the influence of the achievements of thermodynamics of the 1880s.

The development of thermochemical aspects of studies of chemical affinity can also be traced in the works of Marcellin Berthelot. Suffice it to say that Berthelot published 152 sole-author papers on thermochemistry and 63 works together with his numerous students (altogether 215) and besides a two-volume monograph on thermochemistry, having united all the material by one theoretical concept. The main thesis which is considered in this part of the article: *Berthelot puts as aim, to lay, with his works, the foundation for a new science, intended to transform chemistry, by bringing it to rational basics, established on laws of mechanics.* His first thermochemical work appeared in 1865. In this Berthelot formulated the general theorem of thermochemistry based on the principle of equivalence of heat and mechanical work. This provision he called “the principle of calorific equivalent of chemical transformations”. The principle may be translated as follows: *if the system of plain or complicated bodies taken in known conditions, experiences physical or chemical transformations that are able to bring it to a new condition without any external mechanical action, then the quantity of heat released or*

*absorbed in course of these transformations depends only on initial and final state of the system. It will be the same regardless of the nature and order of intermediate stages.*<sup>14</sup> In its essence it is the same law of Hess (the law of constancy of amounts of heat), but only developed from the viewpoint of mechanics.

In 1875, Berthelot set out his concepts in three basic principles of thermochemistry:<sup>15</sup>

- 1) Principle of molecular works: the quantity of heat released during some reaction is a measure of amounts of physical and chemical works, taking place in course of such reaction;
- 2) Principle of calorific equivalent of chemical transformations, discussed above;
- 3) Principle of maximum work: every chemical transformation, happening without interference of outside energy strives for formation of body or system of bodies which release the largest quantity of heat.

The last provision, Berthelot assigned as the basic law of thermochemistry, the viewpoint from which he considered the whole vast amount of experimental material obtained by him and others.

In “Essays on Chemical Mechanics...”<sup>16</sup> (two volumes) he explained the new doctrine, developed by him, which he considered a fundamental point of transformation in chemistry. The first volume set out the major methods of thermochemistry with detailed descriptions of procedures for calorimetric measurements.<sup>17</sup> In the second volume the basic provisions of thermochemistry are formulated and commented upon and a number of chemical reactions reviewed from the principle of maximum work.<sup>18</sup> No doubt, this work of Berthelot was a “culmination revelation” for many of his contemporaries in view of the original statement of main concepts of thermochemistry at the meeting point of three sciences. Having united a number of physical data, he considered from the viewpoint of mechanics chemical interaction as a “game of opposite forces”, the competition of chemical energy and physical agents (which included the heat, electricity, light, pressure). He consecutively analysed, from this viewpoint, conditions of occurrence and the results of various chemical processes. It should be noticed that after the appearance of this work fierce disputes aroused in scientific community, connected with the discussion on appropriateness and limits of the applicability of this principle.

Developments in chemical thermodynamics during the next 20 years made Berthelot change his views on the importance of the principle of maximum work. In his two volume book of 1897<sup>19</sup> this principle did not play a dominating role. Thermodynamics and corresponding experiments by that time confirmed only the provision that chemical reactions, which take place only at a low temperature,

move towards heat release. In view of Van't Hoff's theory, at sufficiently high temperatures most of chemical equilibriums move towards systems which are formed with absorption of heat.

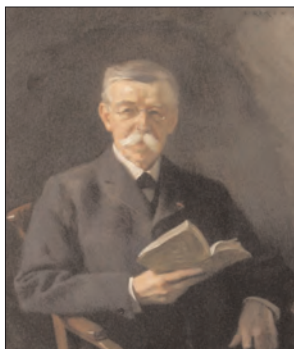
Development of thermochemistry largely depended on the development of its experimental method, calorimetry. The whole development of thermochemistry was accompanied by improvement of calorimeters and methods of determination of thermal capacities and heats of substance formation, for calculation of which it is necessary to know heats of combustion of these substances. As already mentioned the invention of first calorimeters was by Black, Lavoisier and Laplace. In the course of time there appeared new modifications of ice-calorimeters. This apparatus was precise and convenient for many thermal measurements, especially for the investigation of slow processes and reactions with little thermal effect. There were proposed also new types of calorimeters, in particular, the steam calorimeter of R. Bunsen, and the quicksilver calorimeter of P.-A. Favre and J.-T. Silbermann, etc.

Radical changes to the techniques of calorimetry were introduced in 1881 by M. Berthelot, by the creation of a bomb calorimeter. It was especially useful and widely used for determination of calorific values of combustible materials. The only impediment in that time was the high price of the apparatus, as its inner surface was covered with platinum. But ten years later this problem was eliminated by Malher, who replaced platinum by a special enamel.<sup>20</sup>

### **Thermochemistry: the meeting point of physics, chemistry and mechanics. W. F. Louguinine (1834-1911) and his scientific school (second half of XIX<sup>th</sup> century – beginning of XX<sup>th</sup> century)**

A substantial contribution to the development of thermochemistry and techniques of calorimetry in the second half of XIX<sup>th</sup> century was made by a Russian scientist, Wladimir Fedorovich Louguinine (for portrait see Fig. 1). His fundamental researches were based also on the interaction of physics, chemistry and mechanics.

Louguinine was a graduate of the Mikhailov Artillery Academy (1858) but decided in 1860 to resign and turn down a military career.<sup>21</sup> He left for Germany, going to Heidelberg University, to study chemistry in which he intended to perfect himself. It should be noticed that at the same time he paid much attention to studying physics and mechanics. About a year he studied in the Polytechnic School in Karlsruhe, attending the lectures on theoretical and practical mechanics of



**Figure 1. Portrait of W. F. Louguinine by Swiss painter C. Girone (1907). Private collection of prince Aleksandre Wolkonsky (St. Cloud).**

Professor F. Redtenbacher.<sup>22</sup> Afterwards he improved his knowledge of mathematical physics in the course led by Gustav Robert Kirchhoff (1824-1887).<sup>23</sup> At the same time he studied in the chemical laboratory<sup>24</sup> of Robert W.E. Bunsen (1811-1899).<sup>25</sup> In Zürich (1864/1865) he took a course of lectures on mechanical theory<sup>26</sup> of heat given by Rudolf I. Clausius (1822-1888).<sup>27</sup>

As can be seen from the names of his teachers, most were physicists and experts in sphere of mechanics. It is no coincidence that after several years of work in organic chemistry Louguinine still engaged in physical and chemical research. He was for several years an assistant of Henry V. Regnault (1810-1878) in the Collège de France. Later, having got acquainted with Berthelot, he undertook thermochemical research together with him in the Collège de France,<sup>28</sup> and also within the framework of the École pratique des hautes études.<sup>29</sup> Friendship and cooperation of Louguinine and Berthelot proceeded from 1867 and until the death of the later.<sup>30</sup> With Berthelot he published 11 papers on thermochemistry.

It should be noted that over the years Louguinine lived in various countries, France (in Paris), Switzerland (in his estate at Bex, La Pelouse) and his motherland (in Moscow, St.-Petersburg, in his estate in the Kostromskoy region). Hence, he had to often change the laboratories, where his experiments were conducted. Among them, the laboratories in the Collège de France and also the Sorbonne and private ones, organised by him specifically for his own research (in Paris, St.-Petersburg and Switzerland). From 1889, all the scientific and pedagogic activity of Louguinine was connected with Moscow University.<sup>31</sup> By 1892 he managed to equip (at his own expense) a specialised thermochemical laboratory in Moscow University, which at the beginning of xx<sup>th</sup> century was shifted to a newly constructed building of the Physical Institute related to the University.

The main contributions of W. F. Louguinine to thermochemistry are presented here under a series of topics.

- 1) *Development and improvement of techniques of determination combustion and evaporation heats of organic substances and also heat capacities of inorganic substances;*
- 2) *Determination of combustion heats of various classes of organic compounds, on the basis of which important thermochemical laws were ascertained. In course*



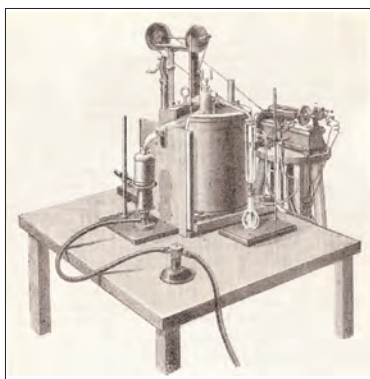
*of this research there was produced reliable experimental data on thermochemistry of organic substances which kept its importance for many years;*

- 3) *Creation in Moscow University of a reference thermochemical laboratory which won world wide recognition for its scientific achievements;*
- 4) *He assisted in training new Russian scientists – thermochemists, who, in their turn, created their own scientific schools.*

### **1) Development and improvement of techniques of determination combustion and evaporation heats of organic substances and also heat capacities of inorganic substances**

Louguinine, as mentioned, had received excellent training in physics and mechanics. In Regnault's laboratory he completed studies of practical design of devices for precise measurements. The years of work with Berthelot also allowed him to gain experience in the same direction. In the course of improving techniques of thermochemical measurements he constructed following devices:

- apparatus for determination of thermal capacity of bodies with a moveable calorimeter and a fixed heater;
- the same apparatus in a version with moveable heater;
- moveable heater for an ice calorimeter;
- apparatus for determination of latent evaporation heat of liquids, etc.<sup>32</sup>



**Figure 2. Louguinine's device for determination of hidden evaporation heat of liquids (Wladimir F. Louguinine, Aleksander N. Schukarev. *Rukovodstvo k kalorimetrii*, P. 105)**

Let us consider, in detail, the last mentioned apparatus, as an example (Fig. 2). The basis for this apparatus was the method of measuring latent heat of evaporations, developed by Regnault, but this required a substantial quantity of the liquid (2-3 kg) for the investigation and the time of the experiment was notable for its long duration. Louguinine's device allowed working with a small amount of liquid – about 100 g. Besides, he managed to exclude from the quantity of measured heat that from the direct action of steam injected to calorimeter. This method became common use in laboratory practice of 1890s.

## **2) Determination of combustion heat of various classes of organic compounds**

As already mentioned, in 1840 Hess had foreseen that with the help of thermochemical research one could come to a wider understanding of structure of organic substances. In one of his works he wrote, "When we will more precisely know those quantities of heat that are educed in course of interaction of several elements, then the quantity of heat educed in course of combustion of an organic substance will be an important factor, which will lead us to a more profound cognition of this substance's structure".<sup>33</sup>

One of the first to use this conclusion of Hess were P.-A. Favre and J.-T. Silbermann, who measured the heats of combustion of twenty hydrocarbons of the ethylenic series. Based on their experimental data on specific heats of combustion of these hydrocarbons<sup>34</sup> they discovered that as the number of CH<sub>2</sub> groups in a molecule increased, combustion heat reduced, on average by 37.5 calories for each CH<sub>2</sub> group. This principle was important for further development of thermochemistry. They showed that thermochemical calculations can be used for determining a compound's composition, having data on thermal effects of combustion reactions.

Many scientists of second half of XIX<sup>th</sup> century were involved in establishing structural-thermochemical relations. Favorite for study were compounds of homologous row of various alcohols and their isomers. Louguinine determined combustion heats of various ketones, alcohols, in search of the connection of substance's combustion heats with the phenomena of homology and isomeric composition. He profoundly theoretically substantiated his research in this direction. "Only in recent time, when thermochemistry due to classic works of Hess, Thomsen and Berthelot, basing on general laws of thermodynamics, developed into absolutely independent field of knowledge, the definition of combustion heats of organic compounds became one of the most effective tools with the help of which one can judge about the composition of the studied body", –wrote Louguinine in the beginning of 1890s–.<sup>35</sup> [...] Obviously, that heat educed in course of burning of every organic compound should be equal to the heat that would be educed in course of the combustion of separate atoms of the carbon, hydrogen etc., comprising the molecule of studied substance without that heat, which should be spent for break of link combining these atoms into a molecule. When defining combustion heats it is necessary to consider these forces and deeper penetrate into molecule's structure [...]".

He chose saturated hydrocarbons and unsaturated hydrocarbons (in particular heptane (C<sub>7</sub>H<sub>16</sub>) and heptylene (C<sub>7</sub>H<sub>14</sub>)) in one research. Based on heptane's experimental heat of combustion, Louguinine determined the homologous differ-

ence for higher homologues which differed substantially from the value for the lower homologues of the series. He also found that in a series of unsaturated hydrocarbons the homologous difference of combustion heat of the higher and the lower members of series also did not match. These experiments lead to the establishment of the following rule, *in case of the increase of molecular weight of hydrocarbon, the homologic difference of combustion heats decreases*. This disproved, in particular, the views of Thomsen, who gave constancy of this value for this homologous series.<sup>36</sup> Louguinine also tried to determine the thermochemical influence of substitution of a hydrogen atom for an hydroxyl group in organic compounds of various classes. He established other important rules from the viewpoint of chemistry, that showed clear connections between the constitution of a compound and the thermal effect on their combustion. For example, *“it is verified – polymerization is accompanied by heat release; ester’s combustion heat approximately equals the sum of combustion heats of acid and alcohol”, etc.*<sup>37</sup> Investigations in this direction were actively continued in works of his pupils, for example, Pavel V. Zubov, who researched on heats of combustion of isomeric compounds<sup>38</sup> and afterwards employees in his laboratory, for example, Wojciech Świątosławski, who studied of heats of atomic bonds formation.

Louguinine also proposed indirect methods of determination of heats of combustion of some acids that were difficult to prepare in pure form. He also made reliable thermochemical measurements of organic substances (ketones, aldehydes, alcohols, esters) that were of importance for many years. Louguinine’s experimental data was regarded as very reliable, in the middle of xx<sup>th</sup> century his data was cited in reference books of the thermodynamic properties of chemical substances,<sup>39</sup> for example, US National Bureau of Standards etc.<sup>40</sup>

Louguinine’s papers from before 1890s were republished in a separate volume (Moscow, 1917), prepared after the scientist’s death, jointly by his daughter, M. Wolkonsky, and a former pupil, Professor I. Kablukov. (Fig. 3). The publication of second volume comprising the works of the period 1892-1912 was planned, but the beginning of revolution in Russia did not allow the realisation of these plans.

### ***3)-4) Establishment of the reference laboratory and of a scientific school***

In 1888 Louguinine, who was already known in scientific world by his works on thermochemistry, decided to find a work space in Moscow. Professor V. V. Markovnikov who was the head chair of chemistry at that time, offered him an opportunity to work in his laboratory. Next year, on the application of the

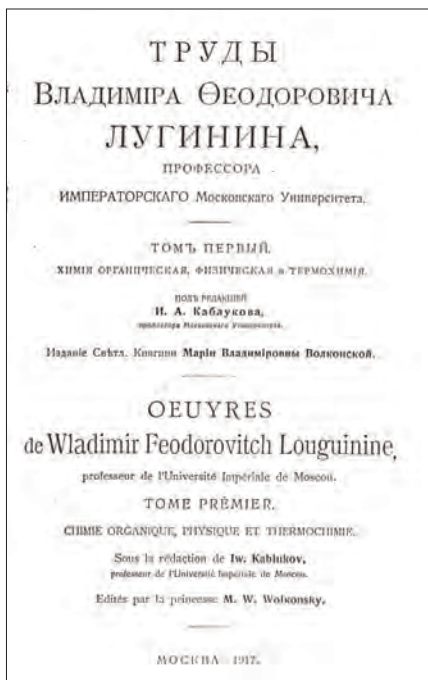


Figure 3. Title page of edition of W. F. Louguinine's works (includes chemical and physico-chemical publications of 1866-1891-s), published in 1917 (Moscow).



Figure 4. Thermochemical laboratory of Moscow University, the photography of the beginning of XX century (Archive of Moscow State University).

University Professors, Louguinine was awarded the title of the doctor of chemistry, without thesis defense, in recognition of his scientific merits<sup>41</sup>. After that he decided to move his thermochemical laboratory from Paris to Moscow (Fig. 4). In the laboratory established by him in the University a lecture course and workshop for students and trainees was organised on calorimetry and thermometry. In this connection Louguinine published various educational books, many of which were republished in foreign languages. In opinion of his contemporaries, the laboratory was equipped with such a large amount of good, expensive devices and also unique ones developed by Louguinine, that it was one of the leading places, not only in Russia but world-wide. In Moscow University, Louguinine worked at first in the position of privat-docent, then as extraordinary Professor (1899).<sup>42</sup> In 1904 Louguinine was appointed an honorable member of Moscow University.<sup>43</sup>

The tradition of Louguinine's school was first and foremost, preciseness and reliability of measurements, which indeed allowed the laboratory to rise to the fore. Apart from students, many respected scientists and Professors worked in the laboratory. Among these were Vladimir I. Vernadskii and Ivan A. Kablukov.<sup>44</sup> Scientists from Kharkov, Warsaw, Kazan' Universities came to research, among these were A. J. Bogorodskii,<sup>45</sup> Vladimir F. Timofeev, Georgii V. Vulf<sup>46</sup> and others. Due to the activity of this laboratory more and more scientists discovered new fields of research for themselves.

In particular, Kablukov, who conducted initially with Louguinine a number of investigations on the thermochemistry of bromine addition to unsaturated hydrocarbons later created his own school of thermochemistry, in which he made a series of measurements on heats of combustion of poorly studied halogen-containing compounds.<sup>47</sup> Professor Timofeev developed thermochemical research in the Ukraine. Starting in 1910 Świątosławski<sup>48</sup> worked in the laboratory, and was the first to construct an adiabatic calorimeter in Russia. Later he created a large school of thermochemistry in Poland.

In 1903, Louguinine presented his laboratory together with all its equipment to Moscow University. It became possible to preserve the laboratory in the framework of organisational system of Moscow University. Nowadays it forms a part of the chemical faculty located in a separate building on Lenin hills and bears the name of W. F. Louguinine.

From 1906 Louguinine lived abroad permanently because of health reasons, mostly in his estate in Switzerland and Paris. In 1911 he died in Paris, in his apartment at avenue des Champs Elysées, 146.<sup>49</sup> The French scientific community also highly appreciated the scientific activities of Louguinine. In 1885 the French government awarded Louguinine the order Officier de l'Instruction Publique.

Afterwards he received the title Chevalier de la Légion d'honneur (1896).<sup>50</sup> In 1909, Louguinine was one of candidates for the position of Corresponding Member of Chemistry Section in Académie des Sciences<sup>51</sup> In the beginning of 1911 he was elected an Honorary Member of Société Chimique de France.<sup>52</sup> In the beginning of 1912 famous physicist and metrologist Ch.-Ed. Guillaume has published a short biography of Louguinine in French as a separate brochure<sup>53</sup> (Fig. 5).

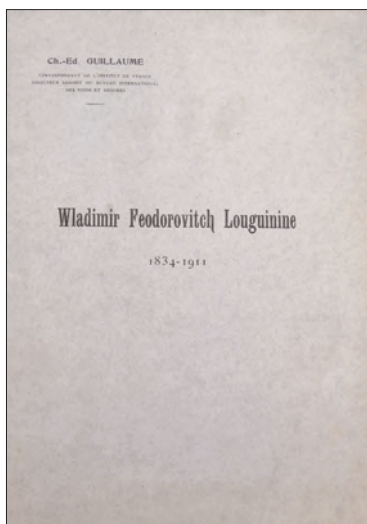


Figure 5. Title page of Ch.-Ed. Guillaume's brochure, dedicated to memory of W. F. Louguinine (Paris, 1912). Private archive of Mme. Livia Guerby.

## Acknowledgments

The present work was carried out with the financial support of Foundation Maison des Sciences de l'Homme, which allowed one of the authors of this paper to get acquainted with materials relating to the activity of Louguinine

in France, found in a number of both state archives of Paris and private ones. We are grateful for assistance the administrator of FMSH, Alain d'Iribarne and members of its staff, Dominique Richard and Sonia Colpart. We owe an immense debt to Bernadette Bensaude-Vencent, Natalie Pigeard, her former student, and Laurence Lestel for support in many ways.

## Notes and References

<sup>1</sup> Henry Guerlac, "Black, Joseph", *DSB* (New York: Charles Scribner's Sons. 1970-1980) vol.2.: 173-183

<sup>2</sup> Joseph Black, *Lectures on the elements of chemistry delivered in the University of Edinburgh*, publ. from his ms., by John Robinson (Edinburgh: W. Creech, 1803, 2 vol.) vol.1, on 137; see also 68-69.

<sup>3</sup> This Mémoire included 4 articles, dedicated to phenomena of burning and breathing, methods of measuring heats educed in course of burning, number of chemical processes etc. - "Mémoire sur la chaleur, par MM. Lavoisier et de Laplace", *Œuvres de Lavoisier*, éd. par J.-B. Dumas, É. Grimaux et F.-A. Fouqué (Paris: Impr. impériale, 1862-1893 , 6 vol.). vol. 2: 283-333.

<sup>4</sup> In its basis lay in the idea of the Swedish physicist, Johan Wilcke (1732-1796) – to define quantity of heat educed in course of some physical or chemical process by measuring the quantity of ice turned into the water by the action of this heat. (Wladimir F. Louguinine, Aleksander N. Schukarev. *Rukovodstvo k kalorimetrii na osnovanii metodov, vyrabotannykh ili ispytannykh v termicheskoi laboratorii pri Fizicheskom institute Moskovskogo universiteta* [A Guide to Calorimetry Basing on Methods Elaborated or Tested in Thermal Laboratory of Physical Institute of Moscow University] (Moskva: Tipolitografiya Tovarischestva I. N. Kushnerev i K, 1905), on 132.

<sup>5</sup> See "Mémoire contenant les expériences faites sur la chaleur pendant l'hiver de 1783 à 1784, par MM. de Laplace et Lavoisier (1792), pour servir de supplément au mémoire sur la chaleur publié en 1780", in *Œuvres de Lavoisier*, vol.2.: 724- 738.

<sup>6</sup> Hans R. Hermann, "Über die Proportionen, in den sich die Wärme mit den chemischen Elementen und ihren Verbindungen vereinigt und über die Mischungsgewichte, als Quotienten der specifischen Gewichte der Körper durch ihre Wärmecapazität betrachtet", *Nouveau mém. de la Société imper. des naturalistes de Moscou* 3 (1834): 153-170. (Hans Rudolf (Iosif Rudol'fovič) Hermann (1805-1879)-see Elena A. Zaitseva-Baum "Ferdinand Friedrich v. Reuss und die Mineralwässeruntersuchungen", in *Deutsch-russische Beziehungen in Medizin und Naturwissenschaften*, Hgg. D.v. Engelhardt u. I. Kästner, Bd. 12 (Aachen: Shaker Verlag, 2005): 137-150).

<sup>7</sup> Wladimir F. Louguinine, Aleksander N. Schukarev, *Rukovodstvo k kalorimetrii*, on 141.

<sup>8</sup> Foreign scientists who worked in Russia for a long time (for example Hess) always had a second name (in a Russian manner).

<sup>9</sup> About him and his works see: Iurii I. Solov'ev, *German Ivanovich Hess* (Moskva: Izdatel'stvo AN SSSR, 1962); Iurii I. Solov'ev, Viktor P. Kolesov "Termokhimiya" v *Istoriya ucheniya of khimicheskoy protsesse. Vseobshchaya istoriya khimii* ["Thermochemistry" in *History of Doctrine of Chemical Process. General History of Chemistry*] (Moskva: Nauka, 1981).

<sup>10</sup> The scientist's works are published in Ostwalds Klassiker der exakten Wissenschaften, 9 (Thermochemische Untersuchungen von G. H. Hess, Hrsg. von W. Ostwald (Leipzig: Engelmann, 1890)).



- <sup>11</sup> Julius R. von Mayer, "Remarks on the forces of inorganic nature", *Ann. Chemie und Pharmacie*, 43 (1842): 233.
- <sup>12</sup> In the first article of this series and the forth ( and last one), correspondingly, under titles: Julius Thomsen, "Die Grundzüge eines thermochemischen Systems. Entwicklung der allgemeinen Begriffe", *Ann. Physik und Chemie* Ser.2. 88 (1853): 349-362; Julius Thomsen, "Die Grundzüge eines thermochemischen Systems. Ueber die Affinität mit besonderer Rücksicht auf die chemischen Zersetzungen", *Ann. Physik und Chemie* Ser.2. 92 (1854): 34-57.
- <sup>13</sup> Julius Thomsen, *Thermochemische Untersuchungen*, 4 vol. (Leipzig : J. A. Barth, 1882-1886)
- <sup>14</sup> Marcellin Berthelot, "Sur la chaleur dégagée dans les réactions chimiques", *Ann. Chim. Phys.* 4 sér. 6 (1865) : 294.
- <sup>15</sup> Marcellin Berthelot, "Sur les principes généraux de la thermochimie", *Ann. Chim. Phys.* 5 sér. 4 (1875) : 5.
- <sup>16</sup> Marcellin Berthelot, *Essai de mécanique chimique fondée sur la thermochimie*, 2 vol. (Paris : Dunod, 1879).
- <sup>17</sup> See. vol. 1. Calorimétrie. Livre 1. "Affinité chimique et calorimétrie" (Marcellin Berthelot, *Essai de mécanique* vol. 1 : 1-6).
- <sup>18</sup> See vol. 2. Mécanique. Livre 5. "Statique chimique". Ch. 1<sup>er</sup>. "Principe du travail maximum" (Marcellin Berthelot, *Essai de mécanique* vol.2 : 417-472)
- <sup>19</sup> Marcellin Berthelot, *Thermochimie. Données et lois numériques*. 2 vol. (Paris: Gauthier-Villars et fils, 1897).
- <sup>20</sup> See: Marcellin Berthelot, *Traité pratique de calorimétrie chimique* (Paris : Gauthier-Villars et fils , G. Masson, 1893) : 132-133.
- <sup>21</sup> About life and activity of Louguinine see the book, Iurii I. Solov'ev, P. I. Starosel'skii, *Wladimir Fedorovich Louguinine 1834-1911* (Moskva : Izdatel'stvo AN SSSR, 1963) and also memoirs of the scientist, written by him and found in the archive of International Institute of Social History (Amsterdam) in the collection of documents of the Society of Protection of Russian Cultural Values in the Foundation of Maria Wladimirovna Wolkonsky (Coll. ACCR No 28). In accordance with the discovered memoirs the authors of this article will prepare a book about W.F.Louguinine.
- <sup>22</sup> *Die Grossherzogliche technische Hochschule Karlsruhe. Festschrift zur Einweihung der Neubauten in Mai 1899* (Stuttgart: Druck von Carl Hammer, 1899): 6-9; 37-41
- <sup>23</sup> Dagmar Drüll, *Heidelberger Gelehrtenlexikon. 1803- 1932* (Berlin-Heidelberg-New York-Tokyo: Springer-Verlag, 1986.): 135
- <sup>24</sup> "Having resided, - writes Louguinine in his memoirs (Archive of International Institute of Social History, ACCR No 28: 83), - I settled in Heidelberg and enrolled in this university. There I engaged in analytic chemistry in Bunsen's laboratory and heard courses of experimental physics and of heat theory by Kirchhoff. In the evenings I heard lectures of either Helmholtz, or lawyer Mittermaier, or historian Häusser". (See also: Dagmar Drüll, *Heidelberger Gelehrtenlexikon*, on 35, 98, 108-109, 181).
- <sup>25</sup> *Geschichte des Chemischen Universitäts – Laboratorium zu Heidelberg seit der Gründung durch Bunsen*, Hrsg. Theodor Curtius und Johannes Risson (Heidelberg: Verlag v. F.W. Rochow, 1908): 4-25.
- <sup>26</sup> According to Louguinine's memoirs, he became so interested with the subject, that translated into Russian the book of professor of Zurich Polytechnicum Gustav-Anton Zeuner "Grundzüge der mechanischen Wärmetheorie mit besonderer bücksicht auf das verhalten des Wasserdampfes"



(Freiberg : J. G. Engelhardt, 1860) (Archive of International Institute of Social History, ACCR No 28: 90-91).

<sup>27</sup> Eidgenössische Technische Hochschule Zürich. Festschrift zum 125 jährigen Bestehen, Hrsg. Jean-François Bergier und Werner Tobler ( Zürich: Verlag Neue Züricher Zeitung, 1980): 595-650.

<sup>28</sup> One can see Louguinine's diary of experimental research, conducted in the laboratory of Collège de France, in the department of manuscripts of Institut de France library (3954 (M. Berthelot): "Table des thermomètres employés pour les expériences calorimétriques", par Wladimir Louguinine (1862) Fol 1-71).

<sup>29</sup> Archives Nationales, Paris: F17/13614.

<sup>30</sup> In private archive of Madame Livia Guerby ( St.-Cloud), great-granddaughter of Louguinine, there is a rich collection of correspondence of Berthelot-Louguinine of these years, which was obviously prepared earlier for publication by the daughter of Russian scientist - Duchess Maria Wolkonsky. It awaits researchers and its publication.

<sup>31</sup> Central Historic Archive of Moscow. Fund 418 (Moscow University). Schedule 487, File 235, 10 pages (Formulary list of W. F. Louguinine).

<sup>32</sup> Specified devices and corresponding methods of measurement were presented in his books: Wladimir F. Louguinine, *Opisanie razlichnykh metodov opredeleniya teplot gorenija organicheskikh soedinenii* [Description of Various Methods of Determination Combustion Heats of Organic Compounds] (Moskva: Tovarišchestvo Skoropechatni A. A. Levenson, 1894) and Wladimir F. Louguinine i Alesander N. Schukarev, *Rukovodstvo k kalorimetrii*, mentioned above. Later, they were published in German and in French, correspondingly. Wladimir Louguinine, *Beschreibung der Hauptmethoden, welche bei der Bestimmung der Verbrennungswärme üblich sind* (Berlin: R. Friedländer & Sohn, 1897) and Wladimir Louguinine et A. Schukareff, *Méthodes de calorimétrie, usitées au laboratoire thermique de l'Université de Moscou*. Traduit du russe par G. Ter-Gasarian (Paris: A. Herrmann, 1908).

<sup>33</sup> German I. Hess, *Termokhimicheskie issledovaniya* [Thermochemical Researches] (Moskva: Izdatel'stvo AN SSSR, 1958), on 128.

<sup>34</sup> P.- A. Favre and J.-T. Silbermann, "Recherches sur la quantités de chaleur dégagées dans les actions chimiques et moléculaires", *Ann. Chim. Phys.* 3 sér. 34 (1852) : 357-450; 36 (1852) : 5-47.

<sup>35</sup> Wladimir F. Louguinine, *Opisanie razlichnykh metodov opredeleniya*, on I.

<sup>36</sup> Not only this research, but also other ones, with an aim of determination of a number of structural and thermochemical principles are described by him in detail in Wladimir Louguinine, "Sur la mesure de chaleur de combustion des matières organiques", *Ann. Chim. Phys.* 5 sér. 27 (1882): 347-374.

<sup>37</sup> Wladimir Louguinine, "Chaleur de combustion des éthers de quelques acides de la série grasse", *C. r. Acad. sci. Paris*, 99 (1884) : 118-1120 ; "Détermination de la chaleur de combustion de quelques acétones et deux éthers de l'acide carbonique", *C. r. Acad. sci. Paris*, 98 (1884) : 94-97; "Chaleur de combustion de quelques éthers d'acides organiques", *C. r. Acad. sci. Paris*, 101 (1885) : 1154-1156; "Sur la mesure des chaleurs de combustion des éthers de quelques acides organiques comme moyen de déterminer la chaleur de formation de ces acides", *Ann. Chim. Phys.* 6 sér. 8 (1886): 128-144 ; Marcellin Berthelot et Wladimir Louguinine "Chaleur de combustion", *C. r. Acad. sci. Paris*, 104 (1887) : 1574-1577; *Bull. Soc. Chim. Paris* 48 (1887) : 700-702 ; *Ann. Chim. Phys.* 6 sér. 13 (1887): 321-339, etc.

<sup>38</sup> Zubov has substantially corrected the early data of Louguinine on combustion heats of isomeric alcohols, as he introduced new ways in method of burning liquids with the help of a bomb

calorimeter. (Pavel Zubov, "Opredelenie teplot goreniya nekotorykh organicheskikh soedinenii", *Zhurnal Russkogo Fiziko-Khimicheskogo obschestva* 30 (1898): 948). ["Determination of Combustion Heats of Some Organic Compounds", *Journal of Russian Physical-Chemical Society* 30 (1898): 948].

<sup>39</sup> In his famous monograph, published in 1933, his former pupil Świętosławski several times mentions the importance of Louguinine's works on determination of combustion heats of various substances, regards the advantages of devices proposed by him for thermochemical measurements (W. Świętosławski *Thermochemie* (Paris: Librairie Félix Alcan, 1933) See pages: 6, 16, 20, 21, 35, 40, 41, 46, 68, 79, 92, 96, etc.).

<sup>40</sup> Selected Values of Chemical Thermodynamic Properties. Circular of the National Bureau of Standards, by D. Friederick, B. Rossini, D. Donald etc. (Washington, 1952). This work contains data from 24 works of Louguinine.

<sup>41</sup> Archive of Russian Academy of Sciences. Fund 474. Schedule 6. File 4. P. 1-2, 3verso.

<sup>42</sup> Archive of Russian Academy of Sciences. Fund 474. Schedule 6. File 9. P. 1-4verso.

<sup>43</sup> Archive of Moscow University. Fund 280 (P. N. Lebedev). P. 34.

<sup>44</sup> V. I. Vernadskii (1863-1945) – chemist, mineralogist, academician (1912), I. A. Kablukov (1857-1942) physico-chemist, honorable academician of USSR AS (1932).

<sup>45</sup> In future the Professor of the University of Kazan'.

<sup>46</sup> Georgii V. Vulf (1863-1925) – crystallographer, corresponding member of USSR AS (1921).

<sup>47</sup> Raisa M. Varuschenko, Elena A. Zaitseva "Zhizn', posvyaschennaya nauke. K 150-letiu so dnya rozhdeniya akademika I. A. Kablukova" [The life, Dedicated to Science. For 150<sup>th</sup> Anniversary from the Birthday of Academician I. A. Kablukov], *Vestnik RAN*, n° 10 (2007): 900-910.

<sup>48</sup> W. Świętosławski (1881-1968) – Polish physico-chemist, member of Polish AS, statesman. In the archive of Russian academy of sciences there are his memoirs on his work in Louguinine's laboratory. There he gave a highly professional estimation of scientific activity of Louguinine (Fund 474. Schedule 6. File 10. 7P).

<sup>49</sup> Obituary was published in Bull. Soc. Chim. France. 9 (1911): 1004.

<sup>50</sup> Archives Nationales, Paris: LH 1664 /16.

<sup>51</sup> Archives de l'Académie des Sciences, Paris: Dossier Louguinine; Comité Secret (1909): 291-292.

<sup>52</sup> Archive of International Institute of Social History, ACCR No 28: 447.

<sup>53</sup> Ch.-Ed. Guillaume, *Wladimir Feodorovitch Louguinine* (Paris: L. Maretheux, imprimeur, 1912).

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# The Development of Teaching of Applied Chemistry at Tokyo University, 1874-1900<sup>1</sup>

*Yoshiyuki Kikuchi\**

Japan underwent a process of institutionalisation of Western-style scientific and technological education in all fields during the Meiji period (1868-1912) as an integral part of the industrialisation policy of the Meiji government. Widely conceived throughout the nineteenth century as the most practical and utilitarian of many scientific disciplines by Europeans and Japanese alike,<sup>2</sup> chemistry was understandably one of the first subjects that gained a departmental status in 1874 together with engineering and law at Tokyo University, one of the major sites of higher education in Meiji Japan. It is also hardly surprising that chemical education there laid particular emphasis on ‘applied chemistry’ (*ôyô kagaku*).

However, the question of what constituted the teaching of applied chemistry was not at all a simple one, and it took almost a quarter of century for a prototype of the teaching of applied chemistry to emerge at Tokyo. This paper discusses the process and mechanism by which the education of applied chemistry at Tokyo University gradually accumulated three elements of teaching practice – analysis, fieldwork and engineering – in the course of these formative years. In conclusion, this event will be contextualised in the wider process of Japanese industrialisation as a question of knowledge or model transfer across cultural boundaries.

## Applied Chemist as ‘consulting analytical chemist’

Three chemists, one English chemist and his two Japanese students, were instrumental in the development of the teaching of applied chemistry at Tokyo University between the 1870s and 1890s: these were Robert William Atkinson (1850-1929), Toyokichi Takamatsu (1852-1937) and Iwata Nakazawa (1858-1943).

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Atkinson was born in 1850 in Newcastle-upon-Tyne, and studied chemistry at both University College London (UCL) under Alexander William Williamson (1824-1904) and the Royal School of Mines (RSM) under Edward Frankland (1825-1899) between 1867 and 1872. He worked for two years at UCL as assistant to Williamson and upon his recommendation Atkinson took up a Professorship of Analytical and Applied Chemistry at Tokyo University, in 1874. He was solely responsible for setting up a Chemistry Department there.

As Bud and Roberts have argued,<sup>3</sup> chemical education at UCL and the RSM between the 1850s and 1870s was formulated on the liberal science model, which rested on a dichotomy of ‘pure’ and ‘applied’ chemistry, in which only the ‘pure’ side of the subject could be taught at universities and colleges. Therefore, in his own teaching, Atkinson had to develop an independent course of applied chemistry single-handedly for Japanese officials and students, who were primarily interested in transplantation of Western-style technology and the exploitation of natural resources for the prosperity of their country.

Atkinson’s solution to the problem of delivering lectures on ‘manufacturing’ chemistry was rather conventional. With no prior experience of working in factories, Atkinson explained the scientific principles of well-known chemical industries such as coal-gas and alkali manufacture without mentioning any technical details and could hardly have satisfied Japanese audiences. It was after all what most English chemists did as a part of their lectures on chemistry. A more successful part of Atkinson’s curricula was his laboratory course. He could respond to their interest in the exploitation of natural resources by introducing the part of UCL’s chemical education designed for training of consulting analytical chemists by one of his teachers, Charles Graham (1836-1909), with commercial goods, foodstuffs and water from various sources as samples of chemical analysis.

For example, Atkinson used samples of domestic natural products such as milk, sugar and iron ore in his basic analytical training courses. Atkinson also assigned water analyses to his students to train their analytical skills, which resulted in papers published by Atkinson as well as his students and partly contributed to the establishment of the Japanese water supply system.<sup>4</sup> In the last case, Atkinson’s training at the RSM also possibly contributed to some extent, as his teacher there, Frankland, was famous for his consultancy in water analysis.<sup>5</sup>

In short, the first element of Tokyo’s teaching programme of applied chemistry was analytical training for consulting chemists. That was largely the result of Atkinson’s effort to make the most of his range of expertise and ideas of applied chemistry formed by his learning experiences in London for his new job in Tokyo.

### **Applied Chemist as ‘fieldworker’**

Atkinson’s teaching programme did end not at this point, but with a graduation work, whose purpose, according to him, was to ‘prepare students to improve the industries prospering in Japan’. As a logical consequence, Atkinson integrated into his course student excursions during summer vacation to traditional Japanese manufacturers. Atkinson’s students did fieldwork there and, using the analytical skill obtained in his laboratory course, embarked on laboratory work with samples collected under Atkinson’s supervision. These projects resulted in graduation theses, including ‘On Japanese pigments’ by Takamatsu, ‘On Shoyu’ [soy sauce], ‘The Chemistry of Copper Smelting in Japan’ by Nakazawa, and other titles such as ‘Japanese tea and tobacco’ and ‘lacquer’. Atkinson’s own research on sake brewing<sup>6</sup> during his Professorship at Tokyo University also originated in the student excursions and was aided significantly by Takamatsu and Nakazawa.

It is important to note that this ‘fieldwork’ aspect of Atkinson’s teaching programme was not just the gathering and examination of raw materials and end products in terms of chemical analysis, as natural product chemists do. Rather, it was comparable to ‘participatory observation’ that a cultural anthropologist does in his or her fieldwork, entailing long stay in site, intensive interaction with indigenous people and building rapport with them. In fact, apart from results of chemical analysis, the existing thesis copies exhibit knowledge of indigenous manufactures and their technological details, shown frequently using diagrams, which was available only from a close collaboration with local manufacturers as informants.

The comparison between cultural anthropologists and Meiji applied chemists may sound unusual, but it does highlight the essential role of Japanese students in the development of Atkinson’s teaching programme of applied chemistry. Just as cultural anthropologists often need informants or collaborators from indigenous societies, Atkinson’s project would simply not have been feasible without students’ participation as mediators, interpreters and practitioners. This comparison also explains why Takamatsu excelled in Atkinson’s teaching scheme and received particular praise from him. This was because most local manufacturers belonged to the same social class as Takamatsu, i.e. wealthy farmers who ran manufacturing businesses. Takamatsu was presumably well prepared for networking, communicating and building rapport with such local manufacturers.

Atkinson’s Japanese students also had recourse to *jitsugaku*, Chinese and Japanese indigenous scholarly traditions including *honzôgaku* (the studies of herbal medicine), *nôgaku* (agriculture studies) and *bussangaku* or studies of local

products. All of these were based on fieldwork in Chinese and Japanese localities and often resulted in encyclopaedic reference works, which was made widely available by the development of publishing culture in Japan since the early 18<sup>th</sup> century.<sup>7</sup> All in all, in this second ‘fieldwork’ element of Atkinson’s teaching programme, Atkinson was in many ways a pupil and his students were his teachers who brought Japanese scholarly traditions into Atkinson’s teaching.

### **Applied Chemist as ‘works chemist’**

Takamatsu, Nakazawa and other Japanese students respected Atkinson as a diligent teacher and researcher. However, it does not mean that they were completely satisfied with Atkinson’s approach to ‘applied’ or ‘manufacturing’ chemistry. Nakazawa in his later years claimed that Takamatsu, not Atkinson, was *the* founder of the teaching of applied chemistry in Japan. It is to be understood that Nakazawa’s words are not to be taken literally, but one can hear his, and probably other students’, dissatisfaction with Atkinson’s teaching.

The missing ingredient in Atkinson’s teaching was for them, the engineering or machine-operation aspect of the chemical industry. Atkinson’s own lectures on Western-style chemical industries lacked any information about the technical details of machine operations, and his curriculum for chemistry students did not include any engineering subject, even though one of his colleagues at Tokyo University was an engineering professor. This lack was indeed a serious drawback for Takamatsu and Nakazawa because they were expected by Japanese officials to supervise the establishment of Western-style chemical industries in Japan such as the alkali and coal-gas industries. Both Takamatsu and Nakazawa used their opportunity of doing overseas study in Europe to supplement their learning experiences at Tokyo, albeit in different ways.

Takamatsu attended the course of ‘technological chemistry’ at Owens College Manchester, for which the Manchester- and Zurich-trained industrial chemist, Watson Smith (1845-1920) designed an educational scheme mainly for training of works chemists.<sup>8</sup> Drawing on his previous experiences in both the alkali and coal tar industries, Smith devoted a large part of his lectures to day-to-day plant operations at chemical works and the engineering aspects of the chemical industry. Smith frequently organised industrial tours to chemical works, and he required students to submit drawings in his examination questions. Takamatsu was awarded the first prize in Smith’s course in 1881.

Nakazawa studied mainly in Germany and went beyond the ordinary academic sphere of universities and *Technische Hochschulen*. Nakazawa spent most of his time overseas visiting industrial exhibitions, local technology-related *Fachschulen*, that is German vocational schools and a wide variety of factories for products such as bricks, textiles, pottery, beers, glass, paper, cement and sugar as well as a workshop for dyeing textiles. In some of these firms, he did practical work himself. His overseas study in Germany developed his interest in chemistry-related industries and how modern Western factories actually worked. This in turn was reflected in his teaching at the Department of Applied Chemistry that favoured on-site factory training.

Indeed, Takamatsu and Nakazawa combined this engineering or factory element of applied chemistry with the two elements of Atkinson's teaching programme to produce their own version of applied chemistry teaching for Tokyo University and technical colleges in the late 1880s and 1890s. Their Department of Applied Chemistry used the whole laboratory teaching allocation for the first two years to train students thoroughly in analysis, comprising qualitative analysis, quantitative analysis and technical analysis, as much as in Atkinson's time. In addition, a large part of the curriculum for the first two years was devoted to engineering subjects, such as applied physics, steam engine, pumps, cranes, building construction, and mechanical drawing practice for applied chemistry students.

Its third-year, graduation work, was based on students' fieldwork in either a workshop in traditional Japanese manufacture or in a factory in the Western-style chemical industry and was structured in two sections: one involving a thesis and the other involving factory planning, design and drawing. Strongly reflecting Nakazawa's teaching philosophy, the object of the course of Factory Planning was to assign to students drawing the plans of a whole factory or part of a factory and to require them to add their own design in order to develop their business sense.

Particularly noteworthy were the research topics Takamatsu and Nakazawa gave to students for their graduation theses, which clearly showed the strata of several elements of applied chemistry teaching in Japan. They can be classified as: 1) the improvement of Japanese indigenous manufactures such as the investigation of enamel and cloisonné (*shippô-yaki*), 2) the exploitation of unused natural resources in Japan such as the method of extracting iodine from seaweed produced in Japan, and 3) solving technical problems in modern chemical industries introduced by transfer from Western countries such as glass making. All theses were grounded in analytical chemistry plus fieldwork that explored contemporary and traditional industries from which the samples had been acquired.



The teaching system of Takamatsu and Nakazawa was stabilised in the late 1890s, it became a prototype of applied chemistry teaching in Japan for its subsequent development in the 20<sup>th</sup> century, and contributed to the formation in Japan of a distinct and multiple identity for chemical technologists (*kagaku kôgyôka*), the differentiation of which from ‘ordinary’ chemists is epitomised by the separation of the Society of Chemical Industry of Japan (*Kôgyô Kagakukai*) from the Tokyo Chemical Society (*Tokyo Kagakukai*) in 1898.

## Conclusion

Three points become clear when looking into the process of the establishment of applied chemistry teaching in Japan. The first point is the more-or-less common issue of teachers’ range of abilities and outlook on applied chemistry, i.e. what they could teach and what they thought should be included in the course of applied chemistry. If this process is considered in the framework of model transfer, this point corresponds to the ‘transferred’ models. The second point is the importance of interaction, collaboration or even frustration, between a Western chemistry teacher and his Japanese students in the construction of the teaching practice of applied chemistry. In this sense, the mixture of analysis, fieldwork and engineering occurred by the mechanism of what sociologists call ‘transculturation’ within ‘contact zones’,<sup>9</sup> that is the modification, selection and hybridisation of ‘host’ and ‘guest’ cultural practices in a place where people with distinct cultural backgrounds mingle and interact with each other.

The above discussion also stressed the third point, the double role of applied chemistry for transplanting Western-style chemical industries, on the one hand and for innovating in the indigenous Japanese manufactures, on the one other, in Meiji period of Japanese industrialisation. This was where Atkinson showed a weakness as well as a great insight into a potential role of applied chemistry in Japanese industrialisation, by stating that the purpose of the graduation work was to ‘prepare students to improve the industries prospering in Japan’. Indeed, contemporary technology-minded Japanese officials were preoccupied with the transplantation of Western-style technology. Most of later historians of Japanese technology followed this assumption and have long considered it to be the sole vehicle of Japanese industrialisation. It is only recently that innovation in indigenous manufacturing sectors using Western scientific and technological know-how has been a focus of historical studies as an equally important feature of Japanese industrialisation.<sup>10</sup> This insight of Atkinson was a major reason why, with all its

drawbacks, his teaching of applied chemistry had long-term consequences for his students' teaching activities.

## Notes

<sup>1</sup> A full version of this paper will be submitted to *Historia Scientiarum*, the Western-language journal of the History of Science Society of Japan.

<sup>2</sup> See David Knight, *Ideas of Chemistry* (London: Athlone Press, 1992) on European's images of chemistry, and Togo Tsukahara, *Affinity and Shinwa Ryoku: Introduction of Western Chemical Concepts in Early Nineteenth-Century Japan* (Amsterdam: J.C. Gieben, 1993) for the introduction of chemistry into Japan and the image of chemistry for Japanese in the early nineteenth century.

<sup>3</sup> Robert F. Bud and Gerrylynn K. Roberts, *Science versus Practice: Chemistry in Victorian Britain* (Manchester: Manchester University Press, 1984).

<sup>4</sup> Atkinson's impact on Japanese water analysis in the early Meiji period is analysed in Hisao Shiokawa, "R.W. Atkinson: Shôgai to kare ni yoru jôsui suishitsu bunseki ni tsuite [R.W. Atkinson: On his life and drinking water analysis]," *Kagakushi* no. 6 (1977): 20-24.

<sup>5</sup> See Colin A. Russell, *Edward Frankland: Chemistry, Controversy and Conspiracy in Victorian England* (Cambridge: Cambridge University Press, 1996), p. 362.

<sup>6</sup> See Junji Tomozawa, "Atkinson ni yoru Nihon-shu jôzô no kenkyû [R.W. Atkinson's Study of Sake Brewing]," *Kagakushi Kenkyû* 75 (1965): 114-123 for a good summary of Atkinson's research on Sake brewing and the response of Japanese brewers to Atkinson's research. See also Tessa Morris-Suzuki, *The Technological Transformation of Japan: From the Seventeenth to the Twenty-first Century* (Cambridge: Cambridge University Press, 1994), p. 109.

<sup>7</sup> *Jitsugaku* literally means 'real learning' 'practical learning' or 'learning with substances' and was used polemically by a variety of scholars and thinkers in the Tokugawa period to criticise the 'futility' of the abstract theories of their rivals which were not based on actual experiences. Here I follow the definition of *jitsugaku* by the Japanese historian Sugimoto Isao as indigenous empirical learning for utilising natural resources for the benefit of people (*riyô kôsei no gaku*). See Isao Sugimoto, *Kinsei Jitsugakushi no Kenkyû: Edo Jidai Chûki ni okeru Kagaku Gijutsugaku no Seisei* [A Study of the History of *Jitsugaku* in early modern Japan: the emergence of science and technology studies in the mid Tokugawa period] (Tokyo: Yoshikawa Kôbunkan, 1962).

<sup>8</sup> See also James F. Donnelly, "Getting Technical: the Vicissitudes of Academic Industrial Chemistry in Nineteenth Century Britain," *History of Education* 26 (1997): 125-43, on pp. 131-136.

<sup>9</sup> See, e.g., Mary Louise Pratt, *Imperial Eyes: Travel Writing and Transculturation* (London and New York: Routledge, 1992).

<sup>10</sup> See, for example, Tetsurô Nakaoka, *Nihon Kindai Gijutsu no Keisei: "Dentô" to "Kindai" no Dainamikusu* (Formation of Modern Japanese Technology: Dynamics between "traditional" and "modern") (Tokyo: Asahi Shinbunsha, 2006).

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## Chemistry, Engineering, and Rationalisation in Germany 1919-33

*Jeffrey Allan Johnson\**

### **Introduction: The First World War and the changing face of chemistry in Germany**

The First World War initiated a fundamental transformation of the German chemical industry, which was affected in several significant ways:

- 1) the loss of prewar markets (and with them Germany's global dominance in dyes);
- 2) a shift from dyes and pharmaceuticals to the production of high explosives and chemical warfare agents, which had begun as an explicitly temporary wartime measure but changed its character in September 1916 with the military's so-called Hindenburg Program, requiring major investments in new "preparedness plants" designed to maintain a permanent productive capacity in war-related chemicals [although most of these plants were dismantled after the war as a result of the terms of the Versailles Treaty, they were an important precedent for future developments of consciously "dual use" technologies];
- 3) major expansions in productive capacity for strategically valuable inorganic raw materials and reagents (especially nitrates and sulfates), intended to make Germany largely independent of foreign imports after the war; and
- 4) a corresponding change in patterns of production and innovation, involving the production of fewer products at higher volume, which led to greater interest in solving engineering problems and designing apparatus than in the systematic laboratory synthesis that had previously dominated industrial research in organic chemicals.<sup>1</sup>

In addition to these industrial and technological changes, there was a change in the professional landscape of industrial chemists. During the war, a small but significant number of women chemists replaced men who had gone to war; more

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important, for the first time the leading professional association, the *Verein Deutscher Chemiker* (VDC, Association of German Chemists), actually recognised these women as potentially equal professional colleagues, and began admitting them to membership.<sup>2</sup> In the immediate aftermath of the revolution that ended the war, moreover, the new republic legalised collective bargaining for all German workers, including employed chemists, so that such professional employees could for the first time organise unions that industrial employers had to recognise. All of these changes confronted German chemists at the end of the war with major dilemmas.

The following paper will highlight two key responses by the German chemical profession to these transformations during the years from 1919 up to the advent of National Socialist rule in 1933: first, efforts to expand the chemical profession by opening new opportunities for chemists to find employment in branches of the economy where they had previously been little represented; second, efforts to develop closer ties between chemists and engineers, in order to meet the new technological challenges, to promote the ongoing “rationalisation” movement in industrial production, and eventually to establish something like a profession of chemical engineering. These efforts had rather mixed results.

### **Too many chemists, too few jobs? The problem of maintaining professional growth**

One of the leaders of the German chemical industry who thought deeply about the implications of the war for the chemical profession was Karl Goldschmidt (1857-1926), director of the Th. Goldschmidt AG in Essen. In April 1918, with Russia out of the war in the East and Germany still hoping for success in the West as a major spring offensive began, Goldschmidt wrote a perceptive analysis of the economic situation of chemists after the war. In this article, published in the VDC’s journal in August 1918, Goldschmidt focused on two critical developments. First, he expected an initially high postwar demand for chemists, paired with an initial shortage of male chemists, to bring more women into the profession and attract more students, including a backlog of five years of chemistry students returning after military service. Second, a “sudden” crisis would come in a few years with the saturation of overseas markets and the industry’s inability to hire more young chemists, just at the moment when the new generation of graduating chemists began looking for jobs.<sup>3</sup>

Hence Goldschmidt argued that it would be crucial to expand the profession, and he proposed the following suggestions:

- 1) the VDC should actively work to expand opportunities for chemists in related economic branches which used chemistry but employed relatively few chemists, especially in agriculture, but also in branches such as dyeing and tanning, ceramics and glass, metallurgy, and cement;
- 2) additional research institutes should be established, such as the Kaiser Wilhelm Institutes for Chemistry and Physical Chemistry created shortly before the war; and
- 3) the VDC should coordinate these efforts with an improved job placement service (*Stellenvermittlung*), which would assist young chemists by proposing the most suitable candidates to work in the new branches being opened to the profession.

Goldschmidt's suggestions initially had little impact, as the German military collapse and the November Revolution of 1918 raised many more pressing political and economic questions. But events soon proved Goldschmidt right. The data in Tables 1a/b reflect the postwar influx of chemists that he had predicted, together with the subsequent collapse in the job market. Thus while the number of chemistry students peaked in 1922/23, and the number of graduates peaked in 1923/24 (approaching triple the prewar level from 1913), the number of chemists hired by industry had already peaked in 1922 and by 1924 was little higher than the prewar level. At the same time, the number of new women graduates being hired fell from a relatively small but still promising figure of 16 to the discouragingly small number of 3.

**Table 1a**  
**Chemistry students and graduates, 1913/14 vs. 1923/24<sup>4</sup>**

<i>Winter-Semester:</i>	<i>1913/14</i>	<i>1921/22</i>	<i>1922/23</i>	<i>1923/24</i>
Chemistry students (German)	2,729	7,005	7,325	6,851
Index (1913/14=100)	100	256	268	251
Chemistry graduates (with Dr. or Diplom)	362	544	776	941
Index (1913/14=100)	100	150	214	260

**Table 1b****Hiring of chemists by the chemical industry, 1913 vs. 1922-24<sup>5</sup>**

<i>Chemists hired in year</i>	<i>1913</i>	<i>1922</i>	<i>1923</i>	<i>1924</i>
by the chemical industry	336	695	534	383
Index (1913=100)	100	207	159	114
[of these, new graduates]	170	411	345	215
Index (1913=100)	100	242	203	126
[number who were women]	n/a	16	10	3
(% who were women)		3.9%	2.9%	1.4%

Up to this point professional organisations like the VDC had been content simply to distribute notices to the schools warning students against studying chemistry, unless they were unusually well qualified. But these warnings seemed not to have had much impact.<sup>6</sup> For every starting position in the industry there were currently as many as 200 applicants. Hence new opportunities for employment must be found, or an entire generation of promising young chemists might be lost to the profession. In 1925 Goldschmidt thus repeated his earlier warning (using long passages taken unchanged from his 1918 article), and now he especially stressed the need to promote chemistry in related economic branches that hitherto had employed few chemists.<sup>7</sup> This time the VDC recognised the need to respond decisively.

### **Chemists and the industrial rationalization movement in the 1920s**

The VDC may have responded more strongly to Goldschmidt's renewed suggestions of 1925, because his arguments now seemed well-suited to the idea of "rationalisation," which had become a widespread trend in German industry in the postwar era. This was a general movement toward greater efficiency in production by more applying more "scientific" approaches, and one way to do this would be to replace vocationally trained masters and foremen by academically trained scientists.<sup>8</sup> The coal-tar dye industry, which began this process decades before the war, could be a model for the success resulting from carrying through this process, which could now be promoted in other industrial branches as well.

### The Karl Goldschmidt Office, 1925-34

During 1925-26, the VDC established an agency to promote opportunities for chemists outside the traditional chemical industry. This they named the “Karl Goldschmidt Office for Chemistry and Economics” (*Karl-Goldschmidt-Stelle für Chemie und Wirtschaft*) in Goldschmidt’s honor, after his sudden death in 1926. In the following year it was renamed “Karl Goldschmidt Office for Chemistry in Scientific Management” (*chemisch-wissenschaftliche Betriebsführung*). Participating with the VDC in this office were the Chemical Industry Employers’ Association (*Arbeitgeberverband der chemischen Industrie*), and the League of Employed Academics in Technical and Scientific Professions (*Bund angestellter Akademiker technisch-naturwissenschaftlicher Berufe* (previously [1919-1925] known as Budaci, for *Bund angestellter Chemiker und Ingenieure*, or League of Employed Chemists and Engineers, the professional union of the chemical industry). The Goldschmidt Office was not intended to be a job placement service as such, but acted instead as an information service (one might say propaganda office) to inform other branches of industry and governmental agencies as to the value of using professionally trained chemists to achieve their goals. In this function it would serve to promote “the most complete rationalisation” of German production, by making it “thoroughly scientific.”<sup>9</sup> Space in this paper does not permit a detailed analysis of the work of the Goldschmidt Office, which in any case existed in its original form only until 1929, when the crisis of the depression led the VDC to merge it with a job placement service. Then in 1933 the National Socialist regime forced the dissolution of the *Bund*, and in 1934 merged the Goldschmidt Office into a national jobs register controlled by the NS German Labor Front (*Deutsche Arbeitsfront* or DAF).<sup>10</sup>

### Did the profession expand?

Unfortunately, as 1933 was the worst point of the economic depression, it is difficult to use data from that period to measure the effectiveness of the efforts of the Goldschmidt Office to expand professional opportunities for chemists. Even so, although the onset of the Great Depression after 1928 increased the unemployment rate of chemists, the success of efforts to promote the expansion of chemistry into new areas was reflected in the fact that chemists lost relatively fewer jobs in the branches of industry outside the traditional chemical industry. By 1933 the VDC estimated that there were nearly as many German industrial chemists working outside the chemical industry (4,000, down only 500 since 1928) as within it (4,400, down 1,100 since 1928).<sup>11</sup> In the longer term, with economic recovery,



this trend of successful expansion into other industrial branches continued. One can compare employment statistics from the Reich Occupational Censuses for 1925 and 1939, which are shown in Table 2. These figures clearly show that the profession as a whole expanded by nearly 50% during this period, but more important, that two-thirds of the new opportunities were to be found outside the chemical industry as traditionally defined.

**Table 2**

**Reich Occupational Census data for numbers of employed German chemists<sup>12</sup>**

<i>Occupational branch/year</i>	<i>1925</i>	<i>(%)</i>	<i>1939</i>	<i>(%)</i>
Chemical industry	6 019	57	7 581	50
Education (all levels)	684	6	1 071	7
Government (except army)	204	2	883	6
iron, other metals industry	522	5	760	5
sugar industry	244	2	251	2
Electro-technical industry	236	2	424	3
Machine construction	178	2	300	2
Other branches	2 487	24	3 989	26
Total of employed chemists	10 574	100	15 259	100

### **The problem of establishing chemical engineering as a German profession before 1945**

Closely related to the postwar problems of rationalisation and expanding the chemical profession was the idea of developing closer ties between chemistry and engineering, which would be essential to ensuring the most efficient methods of large-scale production. As is well known, however, the situation in Germany was very different from that in the Anglo-American context, where during the first three decades of the twentieth century a profession of “chemical engineering” developed in close connection to the profession of chemistry and complete with professional organisations, teaching institutions and journals, as well as a fundamental methodology based on the concept of “unit operations.”<sup>13</sup> Although the larger German chemical plants all had many engineers, many leading German chemists and businessmen strongly opposed the idea of a “chemical engineer” or “chemist engineer” as such. For the typical attitude of leaders of the VDC, consider the views of Alfred Stock (1876-1946, chair of the VDC in the 1920s and director of the Kaiser Wilhelm Institute for Chemistry): “*German* higher education

knows no ‘chemist engineers.’ ...The education of men who are simultaneously chemists and engineers, given the high skills demanded from both types, would produce a half-breed, from which the chemical industry at least could hardly benefit.” Here Stock was almost directly quoting Carl Duisberg, technical director of the Bayer Dye Works and from 1907 to 1912 chair of the VDC, who in 1896 had similarly denounced the “chemist engineer” as an unworkable combination, because no one could simultaneously master both chemistry and engineering.<sup>14</sup> The German opposition to developing an interdisciplinary field of this type may seem a bit odd, as the German colleges of technology (*technische Hochschulen* or THs) all had departments of chemistry (often combined with metallurgy), and since 1899 chemists trained there could receive a degree of “Dr.Ing.,” i.e. doctorate of engineering. Despite this nominal expertise in engineering, however, before the war the actual content of a chemical education in the THs was not significantly different from that at the university, except for a greater emphasis upon physical and inorganic chemistry as well as the addition of a variety of mainly descriptive, survey courses on various aspects of “technical” or “applied” chemistry (or “chemical technology”). If chemists studied engineering, they generally did so in supplementary courses rather than as a major field. Before the Second World War there were only a few weak efforts in Germany to develop chemical engineering (or chemical process technology) as an academic discipline.

### **Chemical engineering without “chemical engineers”?**

As Krug and Meinicke have pointed out, the German chemical industry nevertheless found various ways to substitute for the lack of “chemical engineers,” principally by promoting the cooperation of chemists and mechanical engineers in the design of plant and apparatus.<sup>15</sup> For example, perhaps the leading chemical firm making extensive use of engineering was the Badische Anilin & Sodafabrik in Ludwigshafen and Oppau, with a subsidiary in Leuna-Merseburg (BASF, from 1925 the Upper Rhine division of IG Farbenindustrie AG), which had developed the Haber-Bosch ammonia synthesis before the war, followed by other hydrogenation processes). In July 1928 this division had 345 engineers in its “machine-technical” departments, most holding the Dipl.-Ing. or Dr. Ing. title, but it is unlikely that any of these was designated a “chemical engineer.” At the same time they had more than 500 chemists, nearly all with doctorates, as well as physicists, botanists, and agricultural scientists.<sup>16</sup> Smaller chemical firms, however, found themselves at a disadvantage, because they could not so easily afford to employ such large teams of specialists, nor could they easily obtain the information they

needed to enhance their production processes, as the bigger firms naturally sought to keep proprietary control over the innovative plant and apparatus designed by their teams.

### **Apparatus design as a substitute for “chemical engineering”**

As a result, even before the end of the First World War there were efforts to approach the problem of marrying chemistry and engineering within the VDC by focusing on apparatus design. One of the leaders in this effort was the chemist Max Buchner (1866-1934), who in 1918 helped to organize the *Fachgruppe für Chemisches Apparatewesen* (Fachema, Specialty Group for Chemical Apparatus) within the VDC, in order to promote collaboration among chemists, engineers, machinists, and industry in the development of new chemical process equipment. The group, initially divided into a section for “scientific and laboratory apparatus” as well as one for “large-scale technical apparatus” (the latter chaired by Buchner), shared the common goal of simplifying and standardising materials and forms, which was a central focus of the emerging rationalisation movement. Similarly central to the rationalisation movement was Buchner’s emphasis on replacing human labour by machines, which would make chemical production increasingly dependent upon engineering. But machine engineers could not successfully design chemical apparatus without the cooperation of chemists.<sup>17</sup> Thus he hoped to use the Fachema to bring chemists and engineers together for exchanges of ideas on large-scale plant apparatus — thus bypassing, one may say, the German reluctance to promote an interdisciplinary profession such as chemical engineering. Moreover, he soon found an ideal means to foster such interchanges, as well as to promote the development of an industry for chemical manufacturing apparatus (as well as laboratory apparatus). In 1920 Buchner organized the first “Exhibition of Machines, Apparatus, Equipment and Materials of all Types for Chemical Industry and Laboratories,” which was held on a rather modest scale at the VDC’s annual meeting in his home town of Hanover.<sup>18</sup>

The clumsy title of the first exhibition was subsequently simplified to *Ausstellung für chemisches Apparatewesen* (Exhibition of Chemical Apparatus), universally known by its acronym, Achema. Initially each Achema was held in conjunction with the VDC’s annual meetings (Achema II [Stuttgart 1921], III [Hamburg 1922], IV [Nuremberg 1925]), but by the latter year it was beginning to outgrow the framework of the VDC, having grown from an exhibition space of 560 m<sup>2</sup> (with 75 exhibitors and 876 participants) in 1920 to 2650 m<sup>2</sup> (with 112 exhibitors and 8,173 participants) in 1925.<sup>19</sup> Buchner also scored what he must have seen as a

major achievement by persuading none other than Carl Duisberg, a longtime opponent of chemical engineering in the VDC, to write the introduction to the first Achema yearbook (1925). In Duisberg's words, "the more the boundaries between physics and chemistry overlap in science and in practice, the more significant becomes the question of apparatus."<sup>20</sup>

### **The founding of the German Society for Chemical Apparatus (Dechema), 1926**

In 1926, following Buchner's proposal in connection with the 1925 Achema, the Fachema was reorganized as the *Dechema - Deutsche Gesellschaft für Chemisches Apparateswesen* (German Society for Chemical Apparatus), an independent group that remained affiliated with the VDC, but was now open to a wider membership beyond professional chemists. The purposes of the new organisation included the recruitment of both chemists and non-chemists as members, including engineers and technicians as well as firms engaged in the manufacture of apparatus for chemical plants, while promoting cooperative work between the VDC and other organizations including the *Verein Deutscher Ingenieure* (VDI, Association of German Engineers). Accordingly Waldemar Hellmich, an engineer who was particularly interested in the design of apparatus for chemical processes, joined the Dechema board as VDI representative. Of course the Dechema would continue to sponsor expanded Achema exhibitions, and its other goals included the supervision of standardisation work and "rationalisation" for chemical laboratories and industry (in cooperation with the *Deutscher Normenausschuss* (German Standards Committee, or DNA, founded in 1917; this had already issued 1500 standardisation leaflets by 1926), as well as the promotion of research and publications on chemical apparatus.<sup>21</sup>

### **The Association of German Chemists, Dechema, and Die chemische Fabrik**

The VDC first began to systematically examine questions of engineering (as opposed to its previous focus on "applied chemistry") with the serial publication *Die chemische Fabrik* (*The Chemical Factory*), which began in October 1927 as a joint VDC-Dechema enterprise under Max Buchner's editorship as a supplement to the VDC's *Zeitschrift für angewandte Chemie*. The preface emphasised the VDC's work in the "border areas between chemistry and other disciplines," partic-

ularly that “which joins the work of the chemist to that of the engineer and the builder of apparatus.” Just as in recent years it had become increasingly necessary to acquaint engineers and specialists in other disciplines with the creative processes of chemistry, so the students of chemistry needed to be introduced to the technical aspects of factory production. The new publication would thus cover the “nature of the chemical factory as a whole.”<sup>22</sup> By the end of the year the “supplement” had grown to nearly 400 pages, making it obvious that an independent publication would be needed; hence in 1928 *Die chemische Fabrik* began to appear as a separate journal (which continued under this title until 1941). Its substantial volumes contained articles and news on chemical manufacturing technology and apparatus, Achema news, specifications for new standards from the DNA (in cooperation with Dechema), and initially (in cooperation with the VDC’s Karl Goldschmidt Office) suggestions for new employment opportunities for chemists in other industries.

## Conclusions

The problems of the German chemical profession and the chemical industry in the aftermath of war, as discussed above, led to innovative responses on two related fronts during the years up to 1933. On the front of professional employment, the VDC established an organisation (the Goldschmidt Office) to promote new opportunities for chemists in areas outside the traditional chemical industry, in part with the justification that this would help to further rationalise the German economy by making production more scientific. On the front of German chemical manufacturing technology, the VDC also initiated efforts to promote closer cooperation between chemists and engineers, without promoting a nominal discipline or profession of “chemical engineering” (a notion then unacceptable to many leading academic and industrial chemists). On this front, German chemists and engineers also promoted the rationalisation of chemical laboratory work and industrial production, and the institutionalisation of a new focus on chemical apparatus, through the creation of professional groups (Fachema and Dechema), the holding of regular exhibitions (Achema), and the creation of a professional journal (*Die chemische Fabrik*). These various efforts together provided some, if not all, of the functions of the professional organisations for chemical engineering found in other nations. The most significant missing elements were in education and licensing; these were to some extent compensated for by the development of the academic discipline “process technology (*Verfahrenstechnik*),” which however did not become fully established until well after the period under discussion.

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## Notes

<sup>1</sup> These generalisations are based on current research on the German chemistry during the First World War, as part of a comparative analysis of munitions production, in collaboration with Roy MacLeod of the University of Sydney, and supported by a research grant from the U.S. National Science Foundation. For initial results see Roy MacLeod and Jeffrey Allan Johnson (eds.), *Frontline and Factory: Comparative Perspectives on the Chemical Industry at War, 1914-1924* (Dordrecht, NL: Springer, 2006).

<sup>2</sup> Jeffrey A. Johnson, "Frauen in der deutschen Chemieindustrie, von den Anfängen bis 1945," in *"Aller Männerkultur zum Trotz": Frauen in Mathematik und Naturwissenschaften*, ed. Renate Tobies, 2<sup>nd</sup> ed. (Frankfurt/M., New York: Campus-Verlag, 2008), 283-305, on 287-291; Christine Roloff, *Von der Schmiegsamkeit zur Einmischung. Professionalisierung der Chemikerinnen und Informatikerinnen* (Pfaffenweiler: Centaurus, 1989), 99-100.

<sup>3</sup> Karl Goldschmidt, "Die wirtschaftliche Lage der Chemiker nach dem Kriege," *Zeitschrift für angewandte Chemie*, 31 (1918), 157-160, on 159.

<sup>4</sup> Sources for Table 1a/b: "Statistik der Chemiker und Chemiestudierenden," *Zeitschrift für angewandte Chemie*, 26 (1913), *Aufsatzteil*, 756-757; "Statistik der Chemiker", "Statistik der Chemiestudierenden," *Zeitschrift für angewandte Chemie*, 38 (1925), *Aufsatzteil*, 1211-1214. The data are based on VDC annual surveys of approximately 480 firms and all but six university-level teaching laboratories.

<sup>5</sup> Sources for Table 1b: see previous note (Sources for Table 1a/b).

<sup>6</sup> "Warnung vor dem Chemiestudium," *Zeitschrift für angewandte Chemie*, 38 (1925), 219-220.

<sup>7</sup> Karl Goldschmidt, "Die Not der jungen Chemiker," *Zeitschrift für angewandte Chemie*, 38 (1925), 357-359.

<sup>8</sup> The classic analysis is Robert A. Brady, *The Rationalization Movement in German Industry: A Study in the Evolution of Economic Planning* (Berkeley, Calif.: University of California Press, 1933; repr. New York: H. Fertig, 1974); Hans Wupper-Tewes, *Rationalisierung als Normalisierung: Betriebswissenschaft und betriebliche Leistungs politik in der Weimarer Republik* (Münster: Westfälisches Dampfboot, 1995).

<sup>9</sup> Alfred Stock, "Die Karl Goldschmidt-Stelle für Chemie und Wirtschaft," *Zeitschrift für angewandte Chemie*, 39 (1926), 441-443, on 441; on Budaci see Heiner Ramstetter, "Der deutsche Chemiker im Krieg und Frieden (1914-1945)," in Eberhard Schmauderer (ed.), *Der Chemiker im Wandel der Zeiten: Skizzen zur geschichtlichen Entwicklung des Berufsbildes* (Weinheim: Verlag Chemie, 1973), 311-323, on 321-322.

<sup>10</sup> See documents in "Karl Goldschmidt Stelle 1926" (file), Gesellschaft Deutscher Chemiker, Frankfurt am Main; and annual reports in *Zeitschrift für angewandte Chemie*. On the Labor Front there is no general history, but see Karl Heinz Roth, *Intelligenz und Sozialpolitik im*

“Dritten Reich”: eine methodische-historische Studie am Beispiel des Arbeitswissenschaftlichen Instituts der Deutschen Arbeitsfront (München: K.G. Saur, 1993).

<sup>11</sup> “Statistik der Chemiker,” *Zeitschrift für angewandte Chemie*, 49 (1936), 522.

<sup>12</sup> Sources for Table 2: Germany, Statistisches Reichsamt, *Volks-, Berufs- und Betriebszählung vom 16. Juni 1925 (Statistik des Deutschen Reichs, Band 402, I and II: Berufszählung: Die berufliche und soziale Gliederung der Bevölkerung des Deutschen Reichs)*, passim; and *Volks-, Berufs- und Betriebszählung vom 16. Juni 1933 (Statistik des Deutschen Reichs, Band 556: Die Berufstätigkeit der Bevölkerung des Deutschen Reichs, Heft 1)*, passim. The table excludes directors and self-employed individuals.

<sup>13</sup> On the early development of chemical engineering (in the peculiarly German form of *Verfahrenstechnik*, “process technology”), with some comparisons to other countries, see the works of Klaus Krug, particularly “Zur Herausbildung der Verfahrenstechnik,” in *Der Ursprung der modernen Wissenschaften. Studien zur Entstehung wissenschaftlicher Disziplinen*, ed. Martin Guntau and Hubert Laitko (Berlin: Akademie-Verlag, 1987), 257-271, and “Zur Entwicklungsgeschichte der Verfahrenstechnik von den Quellen bis zu ihrer Emanzipation,” Diss. B (Dresden: Technische Universität, 1983), summarised in “Zur Entwicklungsgeschichte der Verfahrenstechnik von den Quellen bis zu ihrer Emanzipation (Thesen),” *NTM-Schriftenreihe zur Geschichte der Naturwissenschaft, Technik und Medizin*, 22 (1985), 51-59. See also Wolfgang Fratzscher and Klaus Krug, “70 Jahre Verfahrenstechnik: ein Rückblick,” *Chemie-Ingenieur-Technik*, 70 (1998), 634-646. A brief general survey with emphasis on the United States is Wayne M. Pafko, *The History of Chemical Engineering* (updated 9/25/2000), at [http://www.pafko.com/history/h\\_chem20.pdf](http://www.pafko.com/history/h_chem20.pdf) (accessed 6/9/2008).

<sup>14</sup> *Zeitschrift für angewandte Chemie*, 32/1 (1919), 93; *Z. angew. Chem.*, 9 (1896), 97.

<sup>15</sup> Klaus Klug and Klaus-Peter Meinicke, “Verfahrenstechnik: Auseinandersetzungen um ihre Emanzipation,” *Wissenschaftliche Zeitschrift TH Leuna-Merseburg*, 30 (1988), 331-338, on 332.

<sup>16</sup> Ingenieur-Liste (1. Juli 1928), in BASF Unternehmensarchiv, Ludwigshafen/Rh., C 623 Ingenieureinheiten Betriebsgemeinschaft Oberrhein 1928-1940; Chemiker-Liste (April 1926), BASF/UA, C 623. On BASF and its role in IG Farbenindustrie AG see Parts II and III (by Jeffrey Allan Johnson and Raymond G. Stokes, respectively) in Werner Abelschauser et al., *German Industry and Global Enterprise. BASF: The History of a Company* (New York: Cambridge University Press, 2004).

<sup>17</sup> Max Buchner, *Denkschrift über die Ziele und Aufgaben der Fachgruppe für chemisches Apparatewesen, Abteilung für chemische Großapparate* (Hanover, Ger.: Wilh. Riemschneider, [no date, 1919?]).

<sup>18</sup> “Chemisch-wirtschaftliche Nachrichten,” *Zeitschrift für angewandte Chemie*, 33 (1920), Wirtschaftlicher Teil, 288, 350-351.

<sup>19</sup> Utz-Hellmuth Felcht, Wolfgang Habig, and Gerhard Kreysa (Hrsg.), *Signale und Spuren: Festschrift anlässlich des 25. ACHEMA-Jubiläums 1997* (Frankfurt/M.: DECHEMA e.V., 1997), 34; see also Utz-Hellmuth Felcht, “Die DECHEMA – Geschichte im Spiegel der Gegenwart,” in Felcht and Kreysa (Hrsg.), *75 Jahre DECHEMA 1926-2001: Festschrift anlässlich des 75. DECHEMA-Jubiläums* (Frankfurt/M.: DECHEMA e.V., 2001), 6-15.

<sup>20</sup> *Achema-Jahrbuch, Jahrgang 1925*, Hrsg. Max Buchner (Leipzig: Verlag Chemie, 1925).

<sup>21</sup> “Fachgruppe für chemisches Apparatewesen,” *Zeitschrift für angewandte Chemie*, 38 (1925), 840; “Deutsche Gesellschaft für chemisches Apparatewesen,” *Zeitschrift für angewandte Chemie*, 39 (1926), 709-714, on 709-710. See also Bruno Holm, *Fünfzig Jahre Deutscher Normenausschuss* (Berlin: Beuth-Vertrieb, 1967).

<sup>22</sup> “Die chemische Fabrik,” *Zeitschrift für angewandte Chemie*, 40 (1927), 1203.



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## Introducing A. L. Stinville (1868-1949)

*José Miguel Leal da Silva,\* Jean Yollant\*\**

At the 5<sup>th</sup> International Conference on History of Chemistry, held in Estoril/Lisbon (2005), a poster “*Do you know Mr. Stinville?*”,<sup>1</sup> reporting the state of the affairs in the search of biographic data about Auguste Lucien Stinville (1868-1949), a French chemist and engineer who was called to Portugal a century ago to establish in Barreiro, near Lisbon, and for the Portuguese corporation Companhia União Fabril, S.A.R.L., head of the CUF Group, a remarkable chemical complex for the production of fertilizers, acids and other inorganic chemicals. The features of this complex, were the integration of chemical processes and the well engineered lay-out of individual plants, workshops, utilities and facilities, transport connections (road, railway and harbour) to reach and hold an internationally competitive scale, up to the deindustrialisation that marked the last decades of the 20<sup>th</sup>



Photo of A. L. Stinville abt. 1906

century. Participating in the history of this complex the name of A. L. Stinville (as he commonly signed) is still remembered in Barreiro (as by the nameplate of “Stinville street” in the old worker’s quarter), but the dimness of other biographical data and the proximity of the centennial of the establishment of CUF chemical plants in Barreiro suggested a follow-up of this quest. However, only the valuable contribution of Stinville’s relatives in France, meanwhile met, (including one of the present co-authors),<sup>2</sup> allowed for a remarkable progress in this search and provided answers to many questions that were unanswered in 2005.

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Moreover the establishment of a biographic profile, the “Stinville experience” not only confirmed how an active transnational know-how exchange could provide industrial opportunities to peripheral European countries, like Portugal, with suitable technological adaptation to local raw materials and conditions, but also typifies the evolution of the relationship between independent consultants, know-how providers and industrial entrepreneurs, depicting an intense technological career that rose in the last decades of the 19<sup>th</sup> century and lasted to the first years after the World War II.

The biographic elements collected about Auguste Lucien Stinville show a vivid personality, eager of further knowledge, aware and proud of his capabilities, that, in his professional life, has met strong characters like Jules Lefebvre, the PDG of Guano-Phosphate, Alfredo da Silva (1871-1942), the Portuguese industrial tycoon that established the CUF Group, and the French minister Louis Louchet (1872-1931) known as “an engineer, a statesman and a moderniser of France”.

If in the last meeting of the ICHC, the question “*Do you know Mr. Stinville?*” was put and more relevant data about his biography was asked for, we may now say that “*we (begin to) know Mr. Stinville*”. Intentionally, the portrait hereunder shows Stinville at the epoch when he met Alfredo da Silva and he knew Barreiro for the first time... about one hundred years ago.

### Stinville’s youth

Auguste Lucien Lamouche, the eldest of the six children of Auguste Adolphe Lamouche and his wife Lucie Radal-Charay, was born the 19<sup>th</sup> July 1868 in the 15<sup>th</sup> “arrondissement” of Paris. His name and the year of his birth, 1868 and not 1864, are confirmed by his military enrolment registry twenty years later.<sup>3</sup> However, following the option of his father, which for family reasons used to sign as “Auguste Adolphe Stinville” or as “Auguste Adolphe Lamouche dit Stinville”, he will adopt the patronymic “Stinville” in his professional and civil life, commonly signing as “A. L. Stinville”.

Auguste Stinville made his first studies, up to the “baccalaureat” (i.e. the access to upper studies) in the Collège Colbert, in Paris, being recognized as an excellent scholar. Albeit demonstrating a certain propensity to Chemistry, his aim, at the time, was to be admitted to the Naval School. This admission, at that time, would require a financial contribution (that his father made in 1885), and was preceded by a strict selection, based in the prior academic career and in a specific exam, and a demonstration of physical capabilities. Surpassing the two first prerequisi-

tes, up to the point of being one of the best qualified candidates, he was finally rejected due to his myopia. Finding himself jobless, Auguste Stinville got an employment at the “Laboratoires de Paris” (possibly the “Laboratoires de la Ville de Paris”) where he found a former teacher working as an “engineer”, (whose name is not known), but this experience opened to him the world of Chemistry and its industrial relationships. So successful was that experience that it inspired the future career of Auguste, that the familial memory registers his own expression to describe that period of his life: “So I got my happy lot!”.

### **The English (or Welsh) period**

In 1888, at 20 years of age, Auguste Lucien Lamouche, i.e. Auguste Stinville, was exempted from the military service (most probably by the same reason that blocked his ingress in the Navy) and, about that time, quitting the “Laboratoires” and being fluent in English, he leaves for England and/or Wales, to undertake an intensive industrial training practice. No documental evidence was yet found to reveal where and how this training was carried out, but Swansea is pointed out as a probable place. Judging his experience from what Auguste Stinville would later prescribe for a well-succeeded intensive preparation of one of his relatives, he may have stayed mainly at the shop floor and successively exerted all the basic functions then required of plant operators, from the hard labour working with roasting furnaces to the mastery of the chemical processes, equipments and materials. On his return to France, two years later, he described himself as “architecte d’usines”, a description that (as for the title of “ingénieur constructeur”, that he assumed later) represented more that the simple architecture and construction of plants and the arrangement of industrial sites, to also include a full knowledge of the chemical processes involved, joining today’s “process design” with the “basic and detailed engineering”.

### **Jules Lefèvre and the Compagnie du Phospho-Guano**

About that time (1888) the Compagnie du Phospho Guano, with headquarters in Paris, with Jules Lefèvre as chairman of the board, considered the construction of two plants on the Atlantic coast of France, to produce “superphosphates” by solubilisation of natural calcium phosphates (phosphate rock) mined in Tunisia. Each plant was to operate according to the best technology then available and to which Auguste may have had access during his stay in England or Wales, that

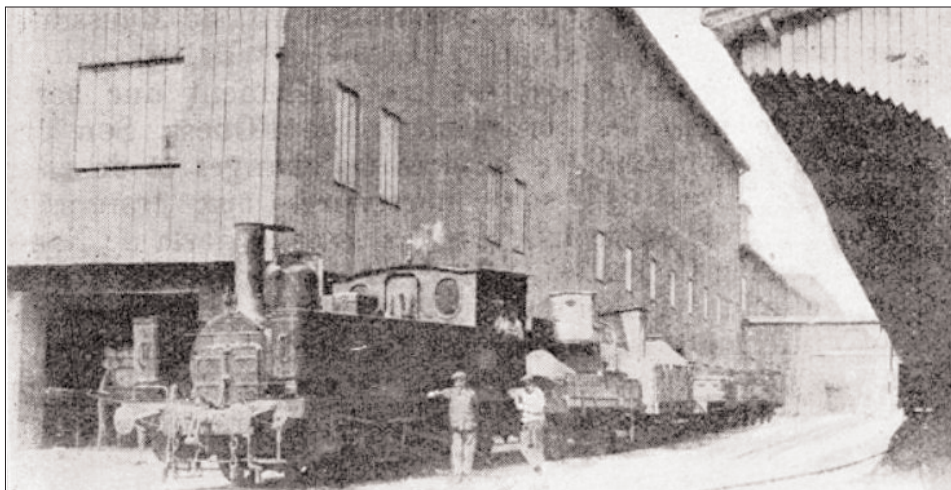


Honfleur plant abt. 1903

included two physically separated works: (a) the sulphuric acid unit (by the lead chamber process, starting from sulphur dioxide obtained by sulphur burning or, more currently in Europe, by the roasting of iron pyrites) and (b) the phosphate solubilisation unit (in French, the “malaxage”), where the phosphate rock was mixed with sulphuric acid, successively followed by a rest period in large caves lined with acidproof bricks (the “dens”), the extraction of the reacted mass, its grinding and packaging (commonly made, at that time, by shovel-filling of bags made of natural, resistant, hard fibres such as jute) These production facilities were to be built in Honfleur (Normandy) and La Pallice - La Rochelle (Poitou-Charentes).

The chemical fertiliser market was then very competitive, either in offer (including the access to raw materials and technologies) as in demand, reason why Lefèbvre would favour the services of a “free-lancer” who could provide a suitable engineering to his new plants. Reciprocally, Auguste Stinville was young, ambitious and self confident to the point of adding 4 years to his actual age to remove any investor’s reluctance based in his youth. After the inauguration of Honfleur, Auguste Stinville made Jules Lefèbvre aware of his actual age, but, since then, his year of birth became definitively and constantly 1864 , as stated in his death certificate and in the inscription in his grave!

As successful contractor he built the Honfleur plant in the scheduled time, between 1890 and 1892. A photo of that industrial unit, dated 1903, shows a warehouse of 3 ridge sheathings, built out of wood in the same design pattern kept for the



The “Sulphuric Acid Row” in La Pallice plant

La Pallice and Barreiro plants, with the “malaxage” as a more raised transverse section, to be seen in the background, and at left, the separate box-shaped body of the sulphuric acid unit. A part of these premises is still used as warehouse by the modern industrial unit currently in the place.

In 1896, also under his project management, was launched the second of these superphosphate projects i.e. the project of La Pallice / La Rochelle (a photo gives a view of its sulphuric acid section). A description of this plant, available in the Internet,<sup>4</sup> mentions a construction period from 1897 to 1901, which seems rather large and perhaps includes later developments. For this unit, Auguste Stinville had the support of his brother in law, Jules Yollant (married to his sister Lucie), who came from a quite different activity but, as already mentioned, was submitted to an intensive “on-the-job training” in other plants near Paris (Ivry and Aubervilliers) not only to supervise the plant construction but also, after the start-up, to second him in the technical management. In 1902, Auguste Stinville, finding that this technical routine jobs became rather limiting to development of his own activities, decided to resign and made his first voyage to the United States of America, passing on to Jules Yollant the technical management of the plant at La Pallice. Then, at the beginning of 1907, a swap of the two plant managers took place: the manager of Honfleur, Castéras, an engineer, graduate of the “Arts et Métiers”, to be meet again later, assumed the technical management of La Pallice and Jules Yollant was assigned technical manager of the Honfleur plant, position he would keep up to a non-friendly retirement in 1918, after the armistice that



ended the World War I. The extent of the successful technical career of Jules Yollant demonstrates not only his own solid qualities but also the validity of the intensive training provided by Auguste Stinville to train him from a non-chemical professional background to become a reliable technical manager in two important industrial plants.

## The gas plants around Paris

If we don't know exactly the aim of Auguste Stinville's first visit to the United States of America, in 1902, it may be inferred from his frequent contacts abroad in the years to come, and as an indefatigable voyager he was, that he was seeking for potential business and opportunities that might increase his potential in the project and engineering markets (in both directions, whenever possible). After returning to France, he married in 1906, and established in Paris his office as "ingénieur constructeur" (construction engineer). In 1907, this office was at the Square Pétrelle, near his domicile at the time, but in 1908 he moved to 56, rue de Londres, and before 1916 he moved again to larger premises in the 14, rue Chauveau Lagarde, near La Madeleine, where he centred his commercial activities and stayed at least up to the 1930's, most probably, even later.

From 1905 to 1910, Auguste Stinville undertook for the Société d'Éclairage, Chauffage et Force Motrice (a predecessor of Gaz de France) a large-scale project for the town gas supply network to Paris and surroundings, including the gas plants of Gennevilliers, Alfortville and Boulogne sur Seine. The establishment of Gennevilliers plant was particularly demanding, taking into account some speci-

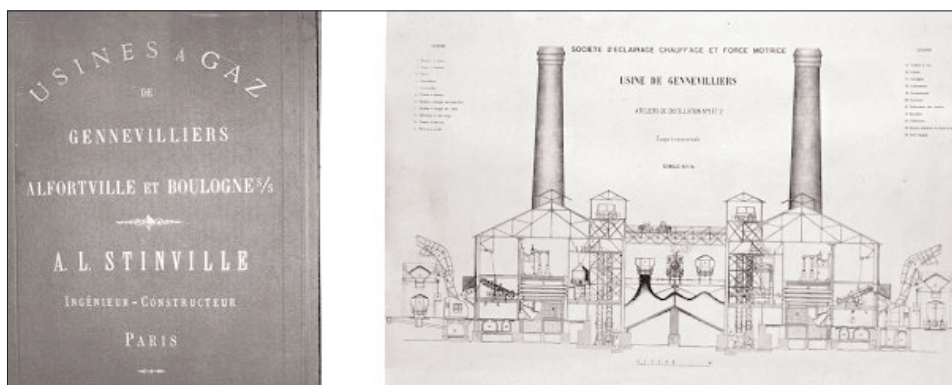


Photo album and plan of Gennevilliers plant

fic requirements, namely a good port by the Seine and effective coal handling mechanised facilities, as well as excellent land connections, by road and railway with an important access layout agreed between Stinville and the Director, Sartiaux, of the Chemins de Fer du Nord.

According to a practice he would also observe for Barreiro, Auguste Stinville collected in well-bound hard cover albums photographs of the erection of these big projects. The album for the gas plants around Paris contains 71 photos and 3 plans<sup>5</sup> and the album for Barreiro, with several phases of the construction (1908-1909), contains 38 photographs. Both these albums are in private ownership.

### **The CUF's plant, in Barreiro, Portugal**

In 1906 Alfredo da Silva, PDG and executive manager of CUF, finding that its industrial plants in Lisbon were constrained by an increasing urban pressure and with limited prospectives for further development, decided to acquire a large area land in the township of Barreiro, on the southern bank of the river Tagus, facing Lisbon. The site had a small pier as an access to the large estuary, allowing for the subsequent arrangement of larger harbour facilities, and easy rail and road connections. The presence of CUF in the market of fertilisers was partially assured by their own limited production in one of its Lisbon plants and by dependence on a potential competitor for the supply of the required sulphuric acid. Because their own production of fertilisers was not enough to meet the market demand, it had to be complemented, year after year, by substantial imports. This implied recurrent hard negotiations with producers abroad and a continuous strong competition with other rival representatives inland. Having a possible deep water harbour, for the import of the phosphate rock, as well as railway (and also maritime) connections to receive Portuguese pyrites, that also might be used for the distribution (including exports) of the plant products, why not break the dependency threads and produce phosphate fertilisers in Barreiro, in an European-sized competitive plant? And with an available surplus of sulphuric acid, why not enter in the production of other inorganic chemicals in a full integrated complex? Convincing the CUF Board to approve such developments, Alfredo da Silva had to face the same problem that Jules Lefebvre had met before: where to find the adequate lay-out for the integrated complex, detailed know-how for the chemical processes and professional skills to run it in a larger competitive domain?

Alfredo da Silva was personally aware of Jules Lefebvre's experience: the minutes of CUF Board meetings reveal that he wrote to Lefebvre and waited for an





The plant at Barreiro under construction (1906-1908).

answer to his letter before inviting Auguste Stinville to Lisbon. As a consequence, Auguste, came to Portugal and in 1907 was contracted to direct the Barreiro project, to follow its erection by a delegated engineer (Pellet, replaced the first designated Lemaire, soon returned to France by health reasons), to recruit in France specialised workers, to assist the formation of Portuguese industrial teams and to keep the office of Barreiro technical manager for two decades, acting via a resident engineer (a role that, after several unsuccessful experiences, was assigned to Casterás mentioned earlier, as technical manager of the La Pallice plant). The photos of the Barreiro construction album (1908-1909) show the impressive use of wood (pitch-pine) as the material of choice in the acid conditions found in the industrial buildings (as in Honfleur and La Pallice). The Barreiro chemical complex, with its facilities for metallic constructions, foundry, and mechanical workshops, power generation and a shipyard, soon was added with hard-fibre textile plants (to provide bags for the packaging of the produced fertilisers).

As technical manager, Auguste Stinville took part in the development projects and substantial capacity increases of the Barreiro plant up to his formal replace-

ment in 1927. These included the sulphuric acid by the chamber process (from Portuguese pyrites), acid concentration, superphosphates, acid leaching of the pyrite cinders for copper recovery, copper sulphate, sodium sulphate and hydrochloric acid, iron (ferrous) sulphate, purification of the purple ore by sintering (for iron making).<sup>6</sup> In 1925 together with Alfredo da Silva (then in France), Manoel de Mello (Silva's son-in-law and, later, his successor in CUF management) and a Portuguese corporation related to CUF, he was a partners in the SIC-CUF (*Société des Industries Chimiques CUF*), then incorporated in Paris (with headquarters in Stinville's office) for the production of aluminium in Southern France (Balaruc-les-Bains), project that was shelved in 1933. In 1937, Auguste Stinville changed letters with Alfredo da Silva giving him some details (and a very critical advice) on the "Basset process", then proposed for the reduction of iron ores in rotary kilns of Portuguese cement plants. During the World War II, CUF obtained technical expertise from British industrial chemists, described in another poster. Very recently, evidence has been found that, after the War, an exchange of technical information between Auguste Stinville and CUF staff was still active up to 1948, i.e. one year before Stinville's death.

The fertiliser plants erected in Barreiro, with the same typical wood structure of the French fertiliser units, as already mentioned, and that still survive in a substantial extent at Honfleur, were gradually replaced since the '80's by more modern units and were demolished in 2000. If a basic knowledge about the fertiliser technology might be achieved during a limited practice abroad, i.e. in Stinville's limited stay in England or Wales, the successful design of full-scale integrated complexes starting from grass-roots and fulfilling the four critical requirements for any project (time, cost, technical achievement and capable personnel) called for a great degree of engineering skill.

### **A trip around the world**

With the relative affluence provided by these successful projects, Auguste Stinville built his own house in Versailles, moving in 1907. The comfortable "villa" was surrounded by a park and the two floored building also lodged a studio and a chemical laboratory.

In 1916, his wife died. They had no children and he did not marry again. Then a quite diversified situation intervened and suspended his project activities. The World was at war and several foreign industrial plants in allied, friendly or dependent countries (United States, Japan, China, Indochina, Australia, South Africa

and England) were working to deliver essential supplies to France, to uphold her war effort. An inspection trip for technical auditing was required and Auguste Stinville was invited (reportedly by the statesman Louis Loucheur, himself a wise and determined engineer) to accomplish that mission. This engaged him from 1916 up to 1918 and represented an official recognition of his capabilities.

### **The electrostatic gas cleaning and the multiple-earth roasting furnaces**

His frequent contacts with technological developments in USA made him aware of the works and patents of Prof. Cottrell related to the then emerging field of electrostatic dedusting and cleaning of gases. Impressed by the capabilities of that technology and foreseeing its increasing use in process plants, Auguste Stinville published in 1918, as “scientific editor”, a monograph with 42 pages describing its fundamentals and use, with several striking examples taken from American industry.<sup>7</sup> He also obtained the exclusivity for France of the Cottrell (and later Cottrell-Möhler) patents and this may represent a very important part of his project activities between the two World Wars. The role of Auguste Stinville in the establishment of commercial societies for the further exploration of this technical field is still under investigation.

In 1929 he claimed and obtained a patent (French Patent 669,595) covering his own developments in the design of multiple-earth roasting furnaces, of the Herreschoff type. This is somewhat surprising and may be regarded as the issue of a “protective patent”, since furnaces of this type, albeit with a limited individual roasting capacity of 7.5 tonnes of pyrite per day, were already designed (in batteries of 4, plus 1 as stand-by) for each sulphuric acid plant he designed for La Pallice, probably Honfleur and later, in 1907-1908, Barreiro.

### **Last days**

One of the last photographs of Auguste Stinville, possibly in the 1930's, was found in Barreiro and was included in the commemorative album published in 1958 to celebrate the 50<sup>th</sup> year of the CUF plants in Barreiro. After a so intense professional activity, Stinville, finding himself aged and alone, left Versailles and the professional activities based in his office and lodged in the flat of his single sister Adèle, 12, rue du Faubourg Poissonnière, Paris. He died there, reportedly from pneumonia, on Sunday, 7<sup>th</sup> August 1949, “aged 85” (but really 81). The death certificate issued by the Town Hall (“Mairie”) of the 10<sup>th</sup> “arrondissement” of Paris,

identified him as Auguste Lucien Lamouche Stinville, chemical engineer, born the 19<sup>th</sup> July 1864 in the 8<sup>th</sup> “arrondissement” of Paris (and not the 19<sup>th</sup> July 1868 in the 15<sup>th</sup> “arrondissement” as per his birth certificate and military inscription). He was buried the next day in the grave of the Stinville Family, in the Old Cemetery of Colombes, near Paris, under the inscription “Auguste Stinville, 1864-1949”. No notice or obituaries were found in the main newspapers published in Paris in the next days.

## References and comments

<sup>1</sup> Silva, José M. Leal da, “*Do you know Mr. Stinville?*”, in Malaquias, Isabel; Homburg, Ernst and Callapez, M. Elvira (Editors), “*5<sup>th</sup> International Conference on History of Chemistry-Chemistry, Technology and Society-Proceedings*”, Sociedade Portuguesa de Química, 2006, Aveiro, pp. 569 to 578

<sup>2</sup> In November 2005, Miss Corinne Yollant, attentively navigating in the Internet, found the set of unanswered questions there placed about “A. L. Stinville” and caused the two co-authors to meet. Besides their careful examination of family archives (namely the valuable documents and notes left by Lucien Yollant, son of Jules Yollant and father of the co-author Jean Yollant), we are much indebted to Mrs. Anne-Marie Warnier born Foussard, M. Yves Yollant and obviously to Miss Corinne Yollant for all the important contributions provided. Unfortunately it seems that other documents eventually in possession of Auguste Stinville when he died were not kept by his sister Adèle and may be regarded as irremediably lost.

<sup>3</sup> Birth and military certificates (these last under Class 1888, Paris/Seine Subdiv., 1<sup>st</sup> Office, 10<sup>th</sup> volume, number 891) are available at the “Archives de Paris”.

<sup>4</sup> <http://inventaire.poitou-charentes.fr/patind/pi/notice.php?id=IA17000235#hautdepage>, consulted the 20<sup>th</sup> August 2007. The plant is also described in the file IA17000235 of the “Base Merimée”.

<sup>5</sup> A description of Gennevilliers plant is available at the “Base Merimée” in [http://www.ecm.culture.gouv.fr/public/mistral/merimee\\_fr?ACTION=CHERCHER&FIELD\\_98=DENO&VALUE\\_98=usine%20e0%20gaz&DOM=Tous&REL\\_SPECIFIC=1](http://www.ecm.culture.gouv.fr/public/mistral/merimee_fr?ACTION=CHERCHER&FIELD_98=DENO&VALUE_98=usine%20e0%20gaz&DOM=Tous&REL_SPECIFIC=1), consulted the 20<sup>th</sup> August 2007, However the major interest of this file seems to be about the overall chronology of the complex and the architectural value of its component buildings.

<sup>6</sup> The circumstances of Barreiro project and Stinville contract to CUF, including the mention to Lefébvre’s letter, are detailed in the Minutes of CUF Board Meetings 1906 to 1908.

<sup>7</sup> Bibliographical reference to this brochure, as well as other references collected up to the 5<sup>th</sup> ICHC are detailed in the reference [1] above and by reasons of economy of space are not reproduced here. Also due to space limitations, three photos, originally included in the exhibited poster and pre-published Conference text were removed (such as the name plate of “Stinville Street” in Barreiro; b) Stinville and his sister Adèle in the chemical laboratory installed in his “villa” at Versailles; and photo-portrait of A. L. Stinville in the ‘30’s, found and kept in Barreiro). The five photographs here included were selected from family sources (in France) and plant archives (also in Portugal).

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## Technological Transfer Issues: Percy Parrish Advising at “CUF, Companhia União Fabril” (40’s Twentieth-Century)

*Isabel Cruz\**

### Profile of Percy Parrish

Percy Parrish was born in 1884 in Dewsbury, in the County of Yorkshire, England. He began his technical studies, at Dewsbury Technical School, later transferring to Huddersfield Technical College and finally entering Leeds University. In 1900 he was employed as a chemist at J. Brown & Co., Ltd (Dewsbury). In 1914 he became general director and chemist at Eaglescliffe Chemical Company’s Work and in 1915 assumed the directorship of the ammonia and acids factories of the South Metropolitan Gas Company in East Greenwich, a position he held until the end of his life. He was elected an Associated Member of the Institute of Chemistry in 1918 and a Fellow in 1931. Parrish was the author of a number of scientific-technical works on the manufacture of chemical fertilisers, sulphuric acid and ammonia, which conferred on him international renown.<sup>1</sup> He died in 1947 at the age of 63. It was the company of his son-in-law, “The Chemical Plant and Sulphur Extraction Co. Ltd”, in Blackheath, London that maintained some of his contracts with the Portuguese corporation CUF, “Companhia União Fabril”.



Figure 1. Portrait of Mr. Percy Parrish. “Obituary”, *Chemical Age* (1947)

### The collaboration of Percy Parrish with CUF, Companhia União Fabril

According to available chronological data, Percy Parrish’s consultant services to CUF – “Companhia União Fabril, SARL” began in the middle of the 1940’s just

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when the corporation was preparing an expansion plan focusing on phosphate fertilisers. Modifying the production technology of the “super phosphates”, the name used in technical circles for the afore mentioned fertilisers was the aim of this expansion plan, and which envisaged the “enrichment” of fertilisers to a content of up to 40% of  $P_2O_5$ , involved the simultaneous use of phosphoric and sulphuric acids. Quite naturally, this meant taking into consideration two aspects of prime importance with regards to the industrial complex of the CUF factories in Barreiro: one was the need for a factory for the manufacture of phosphoric acid (as it happened, the installation was to be the first of its kind at national level), and two, deal with a greater demand for sulphuric acid.

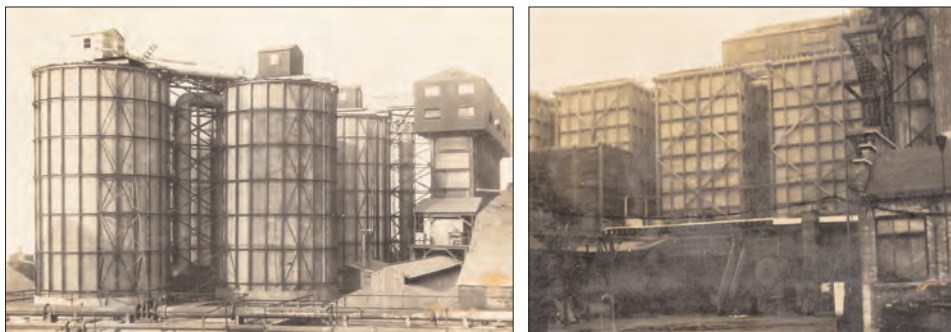
It was precisely the increase in the production of sulphuric acid the reason why CUF requested the technical assistance of the internationally renowned expert, Percy Parrish. Correspondence was exchanged during the middle of 1944 between Percy Parrish and Eduardo Madaíl, an engineer at CUF (general director, assistant technical and commercial director of the Company since December 1942), indicates that the objective was to increase the production of sulphuric acid without expanding the number of already existing production units (3 groups, each one composed by two twin plants arranged side-by-side, with a maximum overall production of  $6 \times 54$  tonnes per day of sulphuric acid at  $53^\circ\text{Bé} \approx 67\% \text{H}_2\text{SO}_4$  equivalent to about 217 tonnes of monohydrate i.e. of sulphuric acid 100%, to be henceforth designated as tonnes MHS) and without altering the process (nitration process, using lead box chambers).

### **Sulphuric chamber’s technology: the adoption of turbo dispersers and “Parrish liquid-phase system”**

The first solution presented by Parrish was the use of turbo-dispersers, a practical and well-known method introduced by Gaillard<sup>2</sup> and improved by Parrish<sup>3</sup> for increasing acid production in classic lead chamber systems. These devices were mounted on the roof of the roughly parallelepiped chambers in the acid plants in Barreiro, and made possible, within the parameters established, a more immediate solution to the urgent need for expansion. For the same amount of raw pyrites fed to the roasting furnaces the acid production was increased by 30%, which meant the considerable increase of 16.2 daily tonnes of sulphuric acid at  $53^\circ\text{Bé}$  (i.e. 10.8 tonnes MHS) per day and per plant.

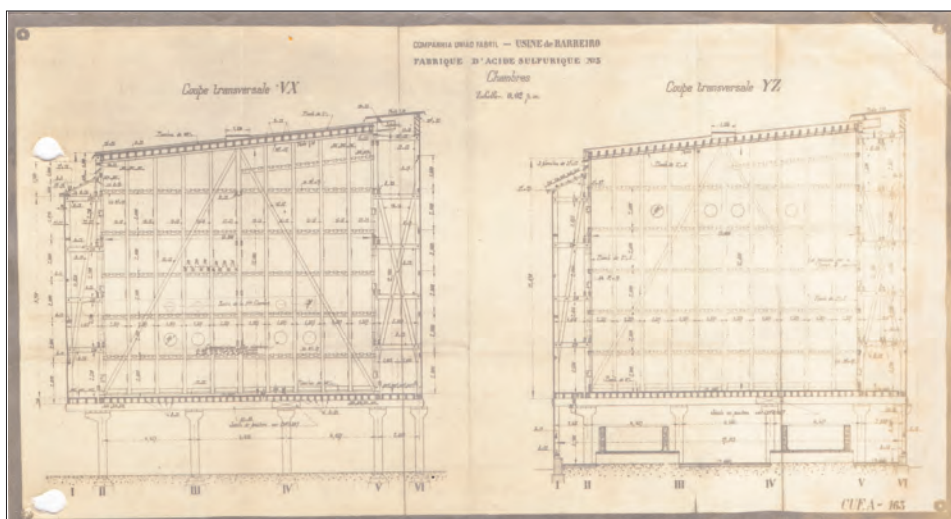
As a result, and on an experimental basis, ten turbo-dispersers were mounted in two of the already existing sulphuric acid plants (Nº 5 and Nº 6) in the following





**Figure 2.** Photographs of “tower chambers” types sent by Percy Parrish to CUF technical staff as example of “Parrish technology”, 1945.

manner: 5 in each, 2 + 2 + 1, distributed respectively between the 1<sup>st</sup>, 2<sup>nd</sup> and 3<sup>rd</sup> chambers. By the middle of 1946 they were working well and producing good results.<sup>4</sup> The turbo-dispersers send out a fine spray of cold acid at 48/49° Bé against the chamber walls. This not only favours the process kinetics and offers a much greater gas/liquid interface to the complex reactions that lead to acid production by the nitration processes, but also provides cooling in an exo-energetic process, avoiding (or reducing) the need of alternative through-the-wall cooling systems such as the external water spraying used by Parrish (and Mills-Packard) plants.<sup>5</sup>



**Figure 3.** Plan of a classic box chamber introduced in the acid plants in Barreiro by the French consultant A. L. Stinville in the beginnings of the 20th century.



Besides the use of turbo-dispersers, other measures were taken to approach the “Parrish liquid - phase system”. Taking advantage of the “influence of the walls” noticing that a sudden decrease in the time taken for acid formation was observed from the moment a liquid film began to cover the chamber walls and which continued to decrease as the film continued to spread, reaching its *minimum* (i.e. *maximum* process intensification) when the walls were completely moist,<sup>6</sup> Parrish decided to divide some of the originally existing chambers, in order to increase the area of contact between the reaction system components.

Consequently, each array of three large consecutive chambers that originally composed each plant was transformed into an assemblage of five consecutive chambers, two of them resulting from dividing the two larger ones, in an arrangement that better approached the “tower chamber” preconceived by Parrish. This innovation was recognised and adopted and by the middle of 1948 all the 3 groups of plants then in operation were using this system. Problems were encountered, however mainly due to a defective turbo-disperser mechanism.<sup>7</sup> Included in the set of measures necessary to implement “Parrish’s liquid - phase system” and increase plant capacity, Parrish made some other recommendations that were implemented in the sulphuric acid plants in Barreiro, such as:

- re-evaluation of the Glovers’ Tower performance, with a view to a possible re-dimensioning;
- operation of an extra mechanical furnace for the roasting of pyrites (the 5th) in each acid producing factory;
- increase the roasting capacity from 7.5 to 9 tonnes of pyrites burned per day in each existing furnace, modifying some details in its conception;
- modification of the furnace feed system and the ore trajectory in the multiple-earth furnace, in order to avoid short-circuiting.<sup>8</sup>

### **Other Percy Parrish initiatives in the CUF factories:**

#### ***a) Concentration and purification of sulphuric acid***

Enhanced by the important contributions proposed by Percy Parrish up to his death (in 1947), in particular in that which related to the process intensification, the chamber technology continued to be the solution of choice for all the next expansion steps of the sulphuric acid production in CUF. This option was maintained up to the time that customer specifications required a concentrated, iron-free, acid. Then, in 1950 the small first contact plant, in great part still conceived by Parrish, entered production for the supply of acid to these new client indus-

tries. However, only in 1952, with a second contact plant, built up when CUF initiated nitrogen fertiliser production, a definitive move to increase capacity with “contact acid” took place. Between the two first contact plants in CUF, an additional capacity increase was made still reproducing, as a module, the existing chamber plant concept.

Parrish also studied other questions arising from internal requests for a purer and more concentrated sulphuric acid supply. Apart from acid at 53° Bé, the Barreiro factories also offered as marketable brands of chamber acid at 60° Bé and 65/66° Bé ordinary, 65/66° Bé with 92% monohydrate and 65/66° Bé purified (this quality was only possible after 1945) – which makes clear that the technologies for purification and concentration of the sulphuric acid produced by the chamber process was available from very early on.<sup>9</sup>

In 1945 the Company adopted the “Parrish Concentration” (a designation used in inside circles), which was introduced in the context of the new phosphoric acid factory, required for the production of concentrated phosphate fertilisers (“triple-superphosphate”, also in a domestic dialect). As at least 70 tonnes/day of sulphuric acid at 60° Bé (78%), i.e. about 55 tonnes/day MHS were required, and as the five existing Kessler units produced daily 3.5 tonnes of acid at 65/66° Bé (i.e. 3.2 tonnes MHS) per concentrator that is, a total of 17.5 tonnes acid 65/66° Bé (i.e. 16.1 tonnes MHS), corresponding to 20.5 tonnes of acid at 60° Bé, alone could not supply enough to cover the needs for phosphoric acid and, not adapted to meet the requirements for large quantities of low concentration acid, did not even guarantee the economical viability of the process.<sup>10</sup>

The installation in Barreiro of the “Parrish Concentration” suffered greatly with the loss of its designer. A number of alterations in the time schedule due to delays in the delivery of refractory and anti-acid material coming from abroad were also contributory factors. Installation was only completed in July 1952.

During the same year a TREPEX de-arsenification, “Purification Parrish”, from “Chemical Plant & Sulphur Extraction Co.” was installed. Also in 1944, a project was already underway at CUF for de-arsenification by precipitation of arsenic with barium sulphide, confirmed by an internal document due to Eng.º Cabral. Percy Parrish, in the meantime, proposed a less expensive process (using a de-arsenificator “TREPEX”), obtaining arsenic sulphide by means of precipitation with H<sub>2</sub>S (generated locally for the process) and this technology was adopted by CUF in April 1945.

***b) Residues and transformation of sub-products***

As soon as the CUF complex in Barreiro started up at the beginning of the 20th century, the question of residues and what to do with them arose. According to the common practices at the time (successively upgraded during the complex's life), a certain part of these residues was "dispersed", but the major part of them, such as the lixiviated pyrite cinders ("purple-ore"), the sulphation muds from the lead chambers, the dust from the roasted pyrites removed by the electrostatic precipitators (electro-filters), the gypsum from phosphoric acid production, grew incessantly in piles and in circumscribed areas, a solution that could not be considered as acceptable long term.

A "quasi barter arrangement" had been established by CUF in which pyrite cinders, after their leaching in Barreiro for the recovery of its copper, were exported against a supply of steel<sup>11</sup>, or in which sulphation muds from the chamber plants (mainly lead sulphate) were exported, in return for a supply of lead ingots. The first mentioned arrangement may have had its end before World War II (period and extent still to be determined), the second arrangement was discontinued even earlier (~1924) because of the bismuth content of the lead, that passed to the sulphation muds and made the recycled lead unsuitable for use in accumulators.

Without exports, cinders and muds continued to mount up in huge piles on the land around the factories with no solution in sight (domestic consumptions only had solved the situation in the 50's). In 1945, at least 200,000 tonnes of lixiviated pyrite ashes had already been produced. Here was a tremendous technological problem. And naturally, as the factories expanded and processes became more advanced, so the amount of ashes continued to increase.

It is not surprising therefore that this problem was brought to the attention of Parrish during his stay at CUF and that he introduced a number of measures, many of them centred on finding other alternatives for lead, in order to find new solution. In relation to lixiviated ashes Parrish tried to find new customers and offered to intervene in presenting an updated version of an agreement to be negotiated in England with potential consumers.<sup>12</sup>

From 1945 onwards, Parrish, also was consulted to find solutions for the electro-filters dusts, namely by making use of the some of the elements contained therein, specially lead. In 1946 he considered the problem and informed CUF that he was developing a process to recuperate lead from the electro-filters. However the process was not completed as expected, most probably due to the death of Percy Parrish. His interest in the lead recovery from pyrite cinders also gave no interesting result, which he assumed to be connected to their chemical/mineralogical

state. Both cases were dealt later on by CUF, in Barreiro, in the fifties/sixties with the integrated recoveries and upgrading provided by the “cinders treatment plant” and the “non-ferrous metallurgy”.

### **Percy Parrish’s position in the CUF factories**

Presented here have been some of the technological interventions that best represent the work of Percy Parrish as chemical consultant of the Barreiro factories from half way through 1944 (the time at which he is thought to have been invited to join CUF) until May 1947 (the date when his collaboration ceased). Others not mentioned, although no less important are, for example, the modernisation of the sodium sulphate and hydrochloric acid factory, the set up of a phosphoric acid plant and the provision to the sulphuric acid plants with new apparatus to analyse and register and thus control the end product in terms of environment and quality. The role of Parrish is also very evident in the technical and business relations between CUF and those companies possessing “know-how” and equipment. He acted as a link, intervening in the ordering processes, advising on practically all technical aspects. And if Stinville was the consultant – founder of the technological process, by developing the industrial matrix base of the initial cluster (and in part accompanied the subsequent development for the industrial-chemical complex) by maintaining close contact with CUF, in particular with Alfredo da Silva, its head “boss”, Percy Parrish was the “expansionist consultant” who led the existing factories to higher performance levels within the same technological paradigm, through an exchange of correspondence with Eduardo Madaíl, engineer and pivot between decision and implementation, and a renowned technical-scientific interlocutor.

### **Notes**

<sup>1</sup> Robin Mackie; Gerrylynn Roberts, *Career Patterns in the British Chemical Profession during the Twentieth Century* (XIII the Economic History Congress, Buenos Aires, 2002) and “Obituary”, *Chemical Age* (1947): June, on 749.

<sup>2</sup> According to F. Stohmann; Bruno Kerl *et al*, “Gran Enciclopedia de Química Industrial”, volume XII, chapter XXXVI – Sulphuric (acid) p. 854, E. A. Gaillard had reconsidered and studied the former method proposed by Blau of spraying the internal walls of the chambers with cold acid; the Gaillard process was considered one of the most important among others like those of Falding, Moritz, Th. Meyer, Mills-Packard, Schmiedel-Klencke and Petersen, all concerned with the construction of specific types of chambers.

<sup>3</sup> According to *L'Industrie Chimique* (1956): September, on p. 278, Parrish had improved the "Gaillard system" and that had resulted in terms of a better rate of work of the turbo-dispersers with less moving force involved. The "Gaillard system" was described as: including one Glover and two Gay-Lussac, one assemblage of 4 to 8 empty towers, very slightly trunk-conic. In the roof (refrigerated by a basin of water) middle of each tower a turbo-disperser projects an acid rain towards the internal walls.

<sup>4</sup> "Two of our plants already have the turbo-dispersers working with splendid results". Cf. Archives of CUF-QUIMIGAL. Documentation of the ex-Division of Inorganic Chemistry and Metals – letter from Eduardo Madaíl to Percy Parrish, 17<sup>th</sup> June 1946.

<sup>5</sup> See "Systèmes intensifs a tours: pulvérisation d'acide dans les systèmes de chambres classiques", *L'Industrie Chimique* (1956): November, on p. 342.

<sup>6</sup> *L'Industrie Chimique* (1956): September, on p.278 and *L'Industrie Chimique* (1956): November, on p.343.

<sup>7</sup> *L'Industrie Chimique* (1956) : September, on p.273

<sup>8</sup> "I thank you for enclosing a print showing the arrangement of the feed of pyrites in relation to the connecting flue. The arrangement which exists, so far as your units N<sup>o</sup>.s 5 and 6 are concerned, is ideal, contrasted with that which was originally arranged in units N<sup>o</sup>.s 1, 2, 3, and 4 and I am gratified to think that at the first available opportunity units N<sup>o</sup>.s 1, 2, 3 and 4 will be converted to the arrangement now existing in units N<sup>o</sup>.s 5 and 6 (...) Much of the dust being entrained to your electrostatic precipitators has been due to the very unscientific arrangement of feeding the pyrites at the point at which the gases emerge, as represented by the arrangements of your N<sup>o</sup>.s 1, 2, 3 and 4 units.". Cf. Archives of CUF-QUIMIGAL. Documentation of the ex-Division of Inorganic Chemistry and Metals – letter from Percy Parrish to the "Foreign Section" of CUF, 14<sup>th</sup> March, 1945.

<sup>9</sup> As we can see in the announcements in "The Agriculture; publicity pamphlet of agricultural learning", CUF, N.º 30, 1912, and further statistical data present in several CUF *memorandum* to the Portuguese official regulator commission of chemical and pharmaceutical products named "Comissão Reguladora dos Produtos Químicos e Farmacêuticos". Archives of CUF-QUIMIGAL. Documentation of the ex-Division of Inorganic Chemistry and Metals. Process N.º 56 : "Comissão Reguladora".

<sup>10</sup> Archives of CUF-QUIMIGAL. Requirement processes for industrial installation. Descriptive and justifying memory. Sulphuric acid: "Parrish" concentration (70 ton/day), 13 of January, 1945.

<sup>11</sup> Archives of CUF-QUIMIGAL. Documentation of the ex-Division of Inorganic Chemistry and Metals. Process 16 (D): Jan de Poorter – letter from Percy Parrish to the "Foreign Section" of CUF, 21<sup>st</sup> October, 1946.

<sup>12</sup> Archives of CUF-QUIMIGAL. Documentation of the ex-Division of Inorganic Chemistry and Metals. Process 16 (D): Jan de Poorter – letter from Percy Parrish to the "Foreign Section" of CUF, 21.

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# Chemists for Industry on the Periphery of Europe: Training and the Rise of Professionalism in Spain during the First Half of the Twentieth Century\*

*Ángel Toca\*\**

## Introduction

The entrepreneurs of the mainland European chemical industries, at the end of the XIX<sup>th</sup> Century thought it was necessary to incorporate new specialists to manage new factories, whose scale and volume of production had increased significantly compared to that in earlier times. Chemical Engineering was one of the new disciplines which emerged slowly in United States and Europe, especially in Great Britain.<sup>1</sup> Different subjects and areas grew in other mainland European countries. For example, in France the ‘Instituts Annexes de Science Appliquées’, programmes of Industrial Chemistry were aimed at the resolution of practical problems of local industries.<sup>2</sup> The influence of organic chemical industry in Germany determined the numbers and the relationships between chemists and mechanical engineers. At the same time, Assistant Professors of Industrial Chemistry in the German Technischen Hochschulen claimed that technicians assigned to inorganic industry had to receive a specialised training. Meanwhile, German University Science Faculties incorporated Technical Chemistry as a way to introduce chemists into the study of chemical processes and their products.<sup>3</sup>

## Key players and places in the first third of XX century in Spain

During these years, Spain went through an intense process of industrialisation; this raised the necessity of producing qualified personnel in order to manage the new factories being built. All the teaching institutions of Science and Technology

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offered several models of training which however had the following characteristics in common:<sup>4</sup> (a) They all had young professors who had carried out postdoctoral studies in Europe; (b) All of them believed that in technical education the scientific aspects should dominate over the technological ones.

This approach was called the “*culture and engineering of laboratory*”. Two types of training institutions for specialists who wanted to work in chemical industry can be distinguished during the first years of the twentieth century, those in the public sphere and those in private institutions. In the public sphere there were three kinds of educational institutions, namely, Schools of Engineering, Industrial Schools and Faculties of Sciences of the Universities. The first offered little specialisation from the middle of the nineteenth century, in the few courses of Chemistry available the theoretical and descriptive aspects were given more importance than the practical ones. In the mid-nineteenth century some Schools of Industrial Engineering appeared in the most important cities in Spain. A few years later most of them closed because of lack of funds, with the exception of that in Barcelona which received financial support from the Catalan bourgeoisie.<sup>5</sup> Students could qualify as a chemical engineer, but studied just a few areas of chemistry (Organic and Inorganic Applied Chemistry and one course of Chemical Analysis); however there was a great shortage of equipment and laboratories.<sup>6</sup> In the early twentieth century two new Schools of Industrial Engineering opened, in Bilbao and Madrid, in response to the need of engineers in the growing Spanish industry. Despite the “renewal movement” in technical education that appeared in Spanish society, the teaching of chemistry in these Schools barely changed in the first 30 years of twentieth century.

The second were the Industrial Schools, a new type of educational institution established at the beginning of the twentieth century, with instruction at a low technical level and aimed to the training of foremen. In 1901 a Government decree established these Schools in the main cities of the country. They were earmarked to train personnel that would be able to replace the more than a thousand foreign technicians, at the time, in Spanish industry. Over three courses foremen for chemical industry received intensive training in the laboratory, and began to explore practical content of new disciplines, such as Electrochemistry or Metallurgy, which were not studied then in other establishments in Spain

The third type training of specialists was that given in University Science Faculties. During the last third of the nineteenth and the early years of the twentieth Century they underwent an intensive period of diversification, creating different subject area sections. In 1902 the chemistry section was created, in which over four courses chemists studied a few subjects of chemical nature, in a highly



theoretical manner and with almost no laboratory work. In 1922 came a new curriculum that extended the number of chemical disciplines studied and created new areas such as in Electrochemistry and Technical Chemistry. This last discipline aimed to study the procedures employed in industry and the operations of control laboratories. Due to the lack of specialists, Technical Chemistry was taught by Professors of other specialties (Chemical Analysis, Inorganic or Organic Chemistry). Because of this, the courses focused on descriptive aspects of the different chemical industries (organic or inorganic) and the main type of analysis that industrial chemists would have to carry out. The advent of the Second Republic allowed this situation to change. The Educational authorities increased to five the number of courses and also the number of optional subjects of an applied character, and promoted practical work for students in the industry. They established two chairs of Technical Chemistry in Spanish universities at Madrid and Oviedo. The Professors tried to introduce the study of unit operations in the same way as was studied in American schools of chemical engineering, but the beginning of the Civil War prevented this initiative.

In the private sphere, two educational institutions were of importance, both were supported by the industrial Catalan bourgeoisie, these were the Institute of Applied Chemistry (IQA) and the Chemical Institute of Sarrià (IQS). Most of the Spanish chemical industry was at the time installed in Catalonia; hence the Catalan employers were concerned about the training of technicians and specialists. They were convinced that they were unable to manage properly their factories, and sought to obtain the formal training not otherwise available for their staff.

The first institution, promoted by the regional government, trained technical staff over four courses of intense work in the laboratory. For this purpose the Government of Catalonia built a modern and well equipped laboratory, which gave a joint training facility for students of the School of Industrial Engineering, the Industrial School and IQA. In the late 1910's, IQA created a course for the studies of *Directores de Industrias Químicas* (Directors of Chemical Industry). Over four courses, these students got on hands on laboratory experience in the synthesis and analysis of organic and inorganic substances. At the end of the course they had to carry out a project on an industrial plant and which they then had to defend in public. In addition, they were expected to attend courses and conferences organized by IQA, which were taught by the Professors of IQA, directors of the main Catalan chemical industries or by reputed chemists such as Wilhelm Ostwald or Paul Sabatier.<sup>7</sup>

The second private initiative was that by the Jesuits, who believed that their novices had to learn the foundations of modern science at the same time as they studied theology. The head of IQS was Eduardo Vitoria, a Jesuit priest who since 1905 led the Laboratorio Químico del Ebro (Chemical Laboratory of Ebro), after he had ended his doctoral studies in Organic Chemistry in the University of Leuven, directed by Louis Henry. The growing interest that Laboratorio del Ebro aroused among the Catalan industrial class forced the Jesuits to transfer their chemical studies in 1916 to Barcelona. Vitoria maintained in the IQS the same practical approach to the study of chemistry that he had acquired during his stay in Leuven. Long hours of work within the laboratory, a good library with a comprehensive collection of journals, were the keys to the success that the IQS achieved during its early years. Its students were the sons of the Catalan bourgeoisie that previously had studied as chemists, pharmacists or engineers in the official establishments of education. During almost the twenty years that IQS was open, 312 students graduated from its three courses, all of them with a strong industrial approach. One of the IQS signs of identity was the immediate incorporation of their pupils to the industrial world, a result of the ties that the IQS alumni association established with Catalan chemical entrepreneurs. It seems not unreasonable to assert that the IQS was the first European institute to offer the specialty, Chemical Engineering,<sup>8</sup> even if none of what are now regarded as the distinctive elements of it were present in the training given during this time.

### **After the Civil War (1936-1939)**

After the Civil War, those chemists that remained loyal to the Spanish Republic were expelled from their University Chairs.<sup>9</sup> The new pro-Franco authorities looked for ways to fill the vacant chairs with followers and supporters of the new regime. In the same way, it was necessary to create new scientific and technological research centers that replaced the republican ones, as CSIC (Spanish Science Council). One of the professional groups that took good advantage of this situation after the Civil War were chemists, they stayed close to political and economical power. As happened in the Italian and the German fascist governments, Franco's regime decided that the new economic system was to be based on economic self-sufficiency, autarky. The chemists that maintained close to the Franco's power supported these economic ideas, this allowed them to acquire influence, occupy some of the most important positions in the government, the Universities and the new institutes for scientific and technical research.

Some of the educational institutions where specialists for chemical industry were trained before the Civil War remained open. Attention will be focussed on two of them, namely the Schools of Industrial Engineering and Faculties of Sciences. In 1954 the Professor of Chemical Engineering in Yale, Barnett F. Dodge, thought that Chemistry had been given too much emphasis in Spanish Schools of Engineering, "...few of Chemical Engineering and too much of Industrial Chemistry".<sup>10</sup> These types of School inherited the course model in use previous to the Civil War and were not able to propose new studies that gave answer to the economic self-sufficiency ideals of the Franco regime. Since the forties, industrial engineers perceived the massive arrival of chemists in industry as a threat to their profession.

Significant changes of opinion were made over time by some Professors that were involved in the training of Spanish engineers. José Martínez Roca, Professor of Organic Industrial Chemistry thought in the nineteen twenties that his discipline had to be centred in the descriptive aspects of processes and apparatuses, and dedicated special attention to the "scientific foundation on which their operations are based".<sup>11</sup> Twenty years later he thought that it was necessary to teach all those operations that are made most use of in the chemical industries, "all chemical industry can be viewed like a unit of several operations...".<sup>12</sup>

Other Professors such as Carlos Abollado tried to change this situation, promoting the introduction of a course of Physical Chemistry and another of Chemical Technology into the curriculum for industrial engineers. Trained as an industrial engineer, at the end of his studies he went to Germany to work in the electrochemical department of the Siemens Halske Company. When he returned to Spain he worked as an engineer in some industries, completing diverse production procedures. After the Civil War, he reached excellent positions within the pro-Franco administration and in the Institute of Applied Investigation (Instituto Juan de la Cierva). In respect to the training of new industrial engineers, his model was close to the German one. He thought "to use the development of descriptive chemistry to study each physicochemical theory...".<sup>13</sup> By this means, a student could simultaneously make contact with theory and its practical application, without it first being considered as a simple abstraction. In order to realise this aim it was necessary to modify the chemistry curriculum for industrial engineers, eliminating the discipline of Industrial Chemistry, diminishing the previous importance of Chemical Analysis and reinforcing the studies of Physical Chemistry with two courses, one of general chemistry and another of Chemical Technology, where "...[students could study] the technique of the operations and the methods to make the apparatuses".<sup>14</sup>

In the faculties of Science, chemists of different specialities, close to Franco's regime or close to the new academic authorities, occupied the first chairs of Technical Chemistry that became vacant. This implied different focuses for the discipline, two approaches mainly prevailed: a) to continue with descriptive aspects characteristic of the Industrial Chemistry; b) to incorporate concepts of the Chemical engineering such as unit operations.

In the second option, two different modes are to be found. Firstly, many Professors tried to maintain relationships with the political and academics authorities by incorporating unit operations inside the discipline of Technical Chemistry. The first to use this approach was Fernando Calvet Prats, the Professor of Technical Chemistry of the University of Barcelona. He had been Professor of Organic Chemistry in Barcelona before the Civil War; he was removed from of his Chair but reinstated in 1945 as Professor of Organic Chemistry in the University of Salamanca. Meanwhile, he worked as technical director of some chemical and biological companies, and created others, Instituto Bioquímico Miguel Servet and Laboratorio Zeltia. This experience later helped him to improve the discipline of Technical Chemistry. In the fifties, he obtained the Chair of Technical Chemistry in Barcelona; in 1952 he introduced the American model of Chemical Engineering, based on the study of unit operations. With the idea to give an impulse to new direction, Calvet invited the Professor of Chemical Engineering of the University of Yale, Barnett F. Dodge, in 1954. Throughout fifteen lectures, Dodge explained the main concepts related to the unit operations.<sup>15</sup> In his opinion, the course of Professor Calvet had designed was similar to that which he himself taught in Yale, but with an enormous deficiency of materials needed for practical learning. For that reason Dodge recommended that the University build a pilot plant for the training of Catalan chemists.

The second approach is represented by Antonio Rius Miró, Professor of Technical Chemistry in Central University of Madrid since 1940. He was trained as chemist, and later he obtained his PhD whilst at the same time that he taught Chemical Analysis and Electrochemistry in the Industrial School of Santander. He carried out postdoctoral studies in Switzerland and Germany, under the influence of well-known electrochemists such as Erich Müller (Dresden) and Carl Fichter (Basle). Upon his return from these studies, he moved to Zaragoza, where he began teaching in its Industrial School. At that time collaborated with the Laboratory of Biochemical Research led by Antonio de Gregorio Rocasolano, a renowned chemist who investigated in Colloid Chemistry. His relations with Rocasolano allowed him to teach Physical Chemistry and Electrochemistry in the Faculty of Science. Among his disciples was Jose Maria Albareda (Rius Miró directed his PhD disser-

tation in 1920's), who reached in Franco's time the position of Secretary of the CSIC (Spanish Research Council created in 1939). Rius Miró worked together local entrepreneurs in resolving some industrial problems, which allowed him to know Juan Abelló, an important entrepreneur close to the Franco authorities in the post-war period.<sup>16</sup> This chemist and pharmaceutical manufacturer asked Rius Miró to manage its chemical factories in León and Madrid in the middle of the 1930's.

When Rius Miró was appointed to the Chair of Technical Chemistry he had a good relationship with the economic and academic authorities of the Franco's new regime. Initially, he organised the mixing of aspects of discipline of German Chemical Technology with that of the American Chemical Engineering. In his view, more attention should be directed to the general foundations of Chemistry, leaving the descriptive aspects of chemical industries to illustrate the chemical laws.<sup>17</sup> Some years later, he wrote a handbook of Chemical Engineering, in which he described some of the unit operations used in industry (filtration, distillation, etc.) with precise descriptions of the laws of Physical Chemistry.<sup>18</sup> According to his staff, he was of the view that when trying to resolve the problems of the chemical industry a chemist well-trained in Physical Chemistry was preferable to an industrial engineer.<sup>19</sup>

With the idea of creating new studies that would produce chemists highly suited to the industrial world, Rius Miró devised a Doctorate in Industrial Chemistry. Thanks to the support received from the academic authorities, the new doctorate became a reality in the last months of 1944, and for nineteen years the University of Madrid was the only authorised centre to provide these PhD. Despite being conceived as doctoral degree, initially it was not necessary to present dissertation. Over two courses students received knowledge of Industrial Chemistry, Industrial Physics, Metallurgy and Physical Chemistry of Industrial Processes. At the end student had to submit an industrial project, for which was necessary to carry out bibliographical as well as experimental work. The organisation of the four disciplines in the course mainly followed the same approach that Arnold Eucken had devised for the German Technischen Hochschulen in thirties. Some of the professors who taught these subjects held important positions in the Government of Franco.

## Conclusions

For more than thirty years, there was no single model for training of specialists for the chemical industry in Spain. Official centres, Schools of Engineering, Indus-

trial Schools and Faculties of Sciences, and some private ones in the area of Barcelona that were close to the needs of the Catalan chemical industry as the Institute of Applied Chemistry and the Chemical Institute of Sarria, played different roles. After a fratricidal war that forced the best scientific and technical personnel outside Spain, chemists close to Franco's regime took advantage of their power and influence to impose their training model, which used the need for internal self-sufficiency by the regime as a way to justify their labour. With some characteristics of the German model of training chemists for industry and others from the Anglo-Saxon chemical engineering model, a Doctorate in Industrial Chemistry established by Antonio Rius Miró and his colleagues in the Central University of Madrid, prevailed over other alternatives in forties and the first half of fifties. Meantime industrial engineers tried to minimise the influence of chemists by suggesting the renewal of their studies, introducing different aspects of Physical Chemistry such as Kinetics and Thermodynamics into the Schools of Engineering. The result led to nonsense, denounced few years later by Jose German Vega, a member of the department of Chemical Engineering at the University of Wisconsin, according to whom there were "...two professions for a single task,..." engineers with knowledge of the chemical processes and chemist versed in physical processes of chemical industry.<sup>20</sup>

## References

- <sup>1</sup> J.F. Donnelly, "Chemical Engineering in England, 1880-1920," *Annals of Science* 45 (1988): 555-590; C. Divall, "Education for Design and Production. Professional Organization, employers, and the study of Chemical Engineering in British Universities, 1922-1976," *Technology and Culture* 35 (1994): 258-288.
- <sup>2</sup> J-C. Guédon, "Conceptual and Institutional Obstacles to the Emergence of Unit Operation in Europe," in *History of Chemical Engineering*, ed. Furter, W.F. (Washington D.C.: American Chemical Society, 1980), 45-76; M. J. Nye, *Science in the provinces. Scientific communities and provincial leadership in France, 1860-1930* (Berkeley: University of California Press, 1986).
- <sup>3</sup> G. Meyer-Thurrow, "The industrialization of invention: a case study from the German Chemical Industry," *Isis* 73 (1982): 363-381.
- <sup>4</sup> Á. Toca, "Ingeniería química en España: los orígenes (1850-1936) (I y II)," *Anales de Química* 102 (2, 3) (2007): 47-53; 59-66.
- <sup>5</sup> G. Lusa-Monforte, "La creación de la Escuela Industrial Barcelonesa (1851)," *Quaderns d'Història de l'Enginyeria* 1 (1996): 1-39.
- <sup>6</sup> A. Ferrán-Degrie, *El Laboratorio de Estudios Superiores de Química de la Escuela Industrial de Barcelona* (Barcelona: Imprenta de Pedro Ortega, 1911), 4.
- <sup>7</sup> Anonymous, *Química e Industria* 1 (1924): 6-8.

- <sup>8</sup> N. Puig-Raposo and S. López-García, "Chemist, Engineers and Entrepreneurs. The Chemical Institute of Sarrià's impact on Spanish Industry (1916-1992)," *History and Technology* 11 (1994): 345-59.
- <sup>9</sup> A. Toca, "Dos profesiones para un solo cometido". La introducción de la ingeniería química en España durante el primer franquismo, *Dynamis* 26 (2006): 253-285.
- <sup>10</sup> B.F.A. Dodge, "Chemical Engineering in Spain," *Chemical Engineering Progress* 56 (1954): 36.
- <sup>11</sup> J. Martínez-Roca, *Apuntes de Química Industrial Orgánica* (Madrid: Litografía F. Villagrasa, 1921), 6.
- <sup>12</sup> J. Martínez-Roca, *La Química en la Ingeniería Industrial. Discurso leído en la entrega de los títulos de ingenieros industriales de la promoción de 1941-1942* (Madrid, Nuevas Gráficas, 1942), 51.
- <sup>13</sup> C. Abollado-Aribau, *Sistema pedagógico para la asignatura de Química General presentado al concurso oposición para una plaza en la Escuela de Ingenieros Industriales de Madrid (1944)*. Archivo General de la Administración, legajo 1827.
- <sup>14</sup> C. Abollado-Aribau, "La industria química y la Química Industrial," *Ión* 45 (1945): 248.
- <sup>15</sup> B. F. A. Dodge, *Conferencias pronunciadas en el curso especial de Ingeniería Química que desarrollo en la cátedra de Química Técnica, del 15 de enero al 15 de marzo de 1954* (Barcelona: s.e. 1954), 3.
- <sup>16</sup> "Abelló Pascual, J," in *Archivo Bibliográfico de España, Portugal e Iberoamérica* (s.l.: s.e., s.d.), vol. 2 (3), 263.
- <sup>17</sup> A. Rius-Miró, *La ingeniería química y la industria farmacéutica. Discurso leído en la Real Academia de Farmacia* (Madrid: Real Academia de Farmacia, 1960), 7-8.
- <sup>18</sup> A. Rius-Miró, *Introducción a la Ingeniería Química* (Madrid: Ediciones Alfa, 1944).
- <sup>19</sup> O. Foz-Gazulla, "La Química Física en el mundo y en España", *Anales de Física y Química* 40 (1944): 236-242. Foz Gazulla was a chemist who collaborated with Rius Miró in the Physical Chemistry Institute of CSIC in Madrid since 1940.
- <sup>20</sup> J. Alemán-Vega, "Momento actual de la ingeniería química española," *Química e Industria* 12 (1965): 151.



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## An Environmentally Friendly Portuguese Manufacturing Company - CIRES

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This paper focuses on and analyses the technologies and policies, aimed at the reduction of environmental impact, implemented by one of the most important high-tech companies in the Portuguese chemical sector. In the context when environmental concerns had only recently become relevant to both public opinion and policy makers in Portugal. The company CIRES, devoted to the manufacture of plastics, was established in the early 1960's in Estarreja. This paper is part of an ongoing research on the effects of technology on the environment and how the technological choices made at various times were responsible for pollution problems in that area.

The 1960s witnessed the growth of concerns, to which previously not much attention had been paid, about environmental issues in general, and specifically, about the chemical substances that were invading the environment in industrialized nations.<sup>1</sup> In 1962, Rachel Carson published her controversial best-selling book *Silent Spring*,<sup>2</sup> which brought into the open the problems arising from the use of DDT, making both the American government as well as the public aware of the need to protect ecosystems.

Also in 1962, the European Council appointed, for the first time, an expert committee for the conservation of nature and natural resources, and another devoted to water pollution (the water chart was published in 1968).<sup>3</sup> In 1970, in the context of the European Year for the Conservation of Nature, the European Council defined, for the first time, the basic principles underlying environmental protection.<sup>4</sup>

However, the various attempts to implement environmental policies were not very successful which lead to the organisation of the Stockholm Summit in 1972. This summit aimed to establish co-ordinated actions between the countries involved in order to make the world aware of the need to protect the environment.

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As a consequence, a new organisation emerged, the United Nations Environment Programme,<sup>5</sup> UNEP, and in this way environmental policies became part of the agenda of the European Community.

During that period, the Portuguese government began to take its first steps toward environmental stewardship. In 1971, under the umbrella of the Portuguese Scientific Research Council, Junta Nacional de Investigação Científica e Tecnológica, JUICT, the government established the National Committee for the Environment. The committee was chaired by a geographer<sup>6</sup> and composed of 28 members, among whom were representatives of various ministries and agencies such as the Nuclear Energy Council (Junta de Energia Nuclear) and the Association for the Protection of Nature (Liga para a Protecção da Natureza). From 1974 to 1987, the role of the Portuguese government in designing a consistent environmental policy went through several ups and downs due to the change of the political regime, which occurred in 1974, when Salazar's dictatorship was overthrown. It was not until 1987 that the Law for Environmental Protection implemented,<sup>7</sup> establishing the foundation that would govern industry, and in particular, the management of industrial waste and effluents. Portuguese industrialists were in this way made aware of the need to comply with environmental regulations, which required the use, when ever possible, of "clean technologies" and the application of preventive techniques oriented towards recycling and the re-use of products for raw materials. "Cleaner Technologies (CTs) aim at preventing environmental damage at sources by producing lower quantities of waste and less harmful residuals than in the first place, and to use less energy and resources during the industrial throughput processes.<sup>8</sup> Clean Technology addresses the design and product and production processes from the very beginning".<sup>9</sup>

### **CIRES and its Policy of Environmental Management**

CIRES, SA, (Industrial Company of Synthetic Resins) was founded in 1960 and pioneered the manufacturing of Polyvinyl chloride (PVC), in Portugal.<sup>10</sup> This company was a joint-venture set up by the Portuguese Bank and Portuguese and Japanese corporations, and was based in Estarreja (Aveiro, Central Portugal).<sup>11</sup> It began its activity in 1963 by producing Polyvinyl chloride through polymerisation by suspension (PVC-S).<sup>12</sup> At present, besides manufacturing PVC-S, it has also since 1982 produced PVC of the emulsion type (PVC-E).<sup>13</sup>

The factory was built in the industrial region of Estarreja, Aveiro, at a time when concern for the environment was negligible.<sup>14</sup> The region where CIRES is located,

Estarreja/Aveiro is particularly environmentally sensitive. The quality of the soil raises various questions given its high friability level, its high permeability, and because in the past it has been extremely affected by pollution. The Aveiro estuary into which most industries drain their liquid effluents has specific environmental features. It is a wide zone of shallow waters with its mouth artificially opened to the sea. Water circulates due to the tides and poses problems for the quality of the water, especially in summer, when the flow of fresh water decreases. The remediation of water is in this way made difficult.

The common practice of industry was to drain the effluents to open reservoirs with no prior treatment, which dramatically affected soils and water resources. Even today, the Aveiro Estuary area is often mentioned as an extreme case of environmental pollution, causing problems of contamination in the food chain. The Aveiro Estuary is particularly rich in fish and birds, eels being one of the most abundant species. The above-mentioned practice, along with the establishment of new factories draining into the rivers and reservoirs, and the complaints of local land owners and authorities, led to a “hardening” of the position of the Ministry of the Public Works with respect to liquid effluent disposal.<sup>15</sup>

This state of affairs led to the creation, in 1961, of the Pollution Study Committee (Gabinete de Estudo da Poluição), whose purpose was to design the norms to be followed by anyone interested in establishing an industrial plant.<sup>16</sup> Yet even though environmental regulations in Portugal were relatively scarce at this time, the coincidence of the government’s first steps in this direction, and the will of the entrepreneurs involved in the founding of CIREs, led the company to begin implementing its own environmental policies.

Even in the early 1960’s, before the important above-mentioned legislative landmarks, CIREs, concerned with the destiny of the residues and effluent it produced, took steps in order to treat and control the properties of its solid residues and the liquid and gaseous effluents. Having, as a founding principle, the desire to reduce the negative impacts resulting from its production, CIREs, through the years, has made some laudable decisions suggesting an entrepreneurial culture well ahead of its times. In line with this agenda, in the late 1980s, CIREs became a member of the Working Group on Safety of the Association of Plastics Manufacturers in Europe (APME) and of the European Council of Vinyl Manufacturers (ECVM). The ECVM members subscribed, in 1995, to a voluntary agreement, the Declaration of Principles, which defined the commitments to environmental protection, which its members should follow, regarding the emissions of the monomer vinyl chloride (VC) and of PVC.<sup>17</sup> In 1993 CIREs also subscribed to the international programme “Responsible Care”, which was launched in

Portugal by the Portuguese Association of Chemical Companies (APEQ-Associação Portuguesa das Empresas Químicas).

In recent years, CIRES has intensified the implementation of measures leading to the optimisation of technologies and minimisation of pollution. Thanks to the changes carried out in the manufacturing procedures at the Estarreja factory, in particular the use of a *closed reactor*,<sup>18</sup> and with the purpose of adopting *Best Available Technologies* (BAT) the company considerably reduced VC monomer emissions. The technological concept of BAT refers to a more efficient technology in terms of environmental impact, available nationally as well as internationally.<sup>19</sup>

Because of its environmental performance, hygiene and safety record, CIRES has received praise from such organisations as the European Environmental Resources Management (ERM).

### **CIRES's Techniques for Environmental Protection**

In order to avoid adding to the pollution resulting from neighbouring factories, CIRES's administration decided to implement the treatment of its liquid effluents beginning in the 1960s, while the factory was still being built.<sup>20</sup> The company requested from the General Directorate of Hydraulic Services (Direcção Geral dos Serviços Hidráulicos -DGSH), the agency that oversees issues related to environmental concerns, a license for releasing these treated effluents outside the factory.<sup>21</sup> Involved in this discussion were the Portuguese engineers Durval Serra and A. Ribeiro, and the Japanese engineers S. Tanaka and K. Kimura.<sup>22</sup>

As a starting point for assessing the problem, data about the nature of the effluents, such as the total flow volume of the water to be drained,<sup>23</sup> resulting from the manufacturing process and from the water treatment station, the dissolved residual materials,<sup>24</sup> and their respective concentrations. Three hypotheses for the solution of the draining problem and their probable impacts on the interests of the neighbouring industries were analysed.<sup>25</sup> The formulation of these hypotheses constituted the first technical approach to the issue and served as a basis for consolidating ideas, clarifying doubts, and discussing other alternatives. The first included the possibility of releasing the effluents, except for those from the Water Treatment Station deemed substantially acidic, into a natural reservoir,<sup>26</sup> Vala Hidráulica, near the factory, where they would subsequently join a current of natural water.<sup>27</sup> This option was considered "economically interesting".<sup>28</sup> The second hypothesis proposed running the effluents through open or partially enclosed

ditches to a plot of land, designated “Mud Park” (“Parque de Lamas”), owned by CIRES. This alternative was considered “technically good” but very costly, because the configuration and nature of the terrain were not favorable. The last hypothesis included directing the effluents to the Vala de São Filipe, the same reservoir where the neighboring factories, UNITECA<sup>29</sup> and AMONÍACO PORTUGUÊS, deposited their effluents. Of the three solutions, this latter was considered to be the “most viable and most advantageous”.<sup>30</sup>

In the event, CIRES chose the first solution, opting to treat its drainage, and subsequently releasing by way of the Vala Hidráulica into the Ria de Aveiro.<sup>31</sup> Thus it abandoned the “most viable and the most advantageous” option of releasing the effluents without any special treatment into the Vala de São Felipe,<sup>32</sup> where the drainage of AMONÍACO AND UNITECA was already released, because the CIRES administration did not want to use a reservoir, which already caused serious pollution problems for the region.

Faced with the dilemma, of having to choose between the “economically interesting” first hypothesis, the “technically good” second hypothesis, and the “most viable and most advantageous”, CIRES opted to spend substantial sums in order to have its effluents pre-treated, with obvious positive consequences for the environment. As a result, a neutral or nearly neutral liquid was released, whose saline content was harmless to agriculture and livestock.<sup>33</sup>

Liquid effluent resulting from PVC synthesis essentially contains PVC particles in a water suspension or emulsion and a small amount of VC monomer.<sup>34</sup> In order to remove the particles in suspension, the liquid effluent is decanted<sup>35</sup> and subsequently treated in a sewage station where it is chemically neutralised and solids coagulated.<sup>36</sup> The PVC mud is then compacted by water removal through compressing. This technique removes emulsified PVC and the organic content and is highly efficient.<sup>37</sup> The quality of the effluent released is below the limits of contamination imposed by law.<sup>38</sup> In fact the level of total suspended solids (TSS) in CIRES’s effluent is about 20 times lower than the legislated value. The treated liquid effluents are then re-used in the refrigeration circuits of the factory and to water farm fields. In this way CIRES reduces its water consumption and minimises the environmental impact of the effluent on the pollution sensitive Aveiro Estuary.

Related to the solids treatment, even before starting manufacturing, CIRES excavated a reservoir in the factory estate called “Mud Park”, located in Lagoa de Frade, where solid residues resulting from VC monomer manufacturing were deposited. These residues are essentially a suspension of calcium hydroxide

[Ca(OH)<sub>2</sub>] arising from the synthesis of acetylene.<sup>39</sup> This process, employed in the manufacturing of the monomer vinyl chloride, was in current use until 1986.

The “Mud Park” was accused of contributing to the pollution of the Aveiro Estuary. The suspicion was based on the fact that mercury(II) chloride (HgCl<sub>2</sub>) was used as a catalyst in VC synthesis. However, instead of burying it, then a common practice, CIRES decided to export these residues to foreign companies, which carried out the recovery of mercury.<sup>40</sup> At present, the “Mud Park” is part of a green space. It became redundant, because CIRES no longer manufactures vinyl chloride from acetylene. CIRES, in collaboration with the National Institute of Agrarian Investigation (Instituto Nacional de Investigação Agrária - INIA) and the Enterprise for Environmental Recovery of the Soils of Estarreja (Empresa de Recuperação Ambiental dos Solos de Estarreja - ERASE), have been carrying out studies of chemical-physical characterisation for the use of this deposit. Given that conclusions of the study show that the Mud Park of calcium hydroxide is not responsible for the contamination of the grounds, it will be subject to reforestation, currently being carried out or later used for sanitary landfill. With respect to PVC residues,<sup>41</sup> the technology does not yet allow their re-use in the manufacturing process, and that is why CIRES recycles these solid effluents elsewhere in order to maximise their use.

CIRES's environmental management policy extends also to the problem of emissions into the atmosphere, with impacts on the environment as well as on the workers' health. Thus, in the 1960's, CIRES acquired new technology for a new production process of PVC-suspension, called Nova Tecnologia of SHIN-ETSU (NTSE), so-called for having been developed by SHIN-ETSU. One of its principal virtues of this technology had to do with security in the workplace, namely, because it significantly reduced the amount of the poison monomer vinyl chloride inhaled. In other words, this polymerization technique, known as the “non-scaling technique”, avoided the formation of scales (incrustations) in polymerisation vessels. As a result, there was no need for workers to enter inside the vessels in order to clean them. This fact was very important and much safer, because it prevented the escape of VC into the workplace.<sup>42</sup> This technology, considered superior, thus reduced the risks of the toxicity of vinyl chloride. With this process, SHIN-ETSU guaranteed the industrial hygiene standards demanded.<sup>43</sup>

Although these procedures reduced environmental contamination, the company continued its search for a technology that might further reduce the impact on the ecosystem. Today it uses closed reactor technology, with significant results in the reduction of VC monomer emissions. With this technology, CIRES complies with

the objectives established by the Voluntary Agreement of European PVC manufacturers.<sup>44</sup>

Closed reactor technology basically consists of a three-stage process. First, the connections between reactors, suspension tanks and the VC monomer main gasometer are established, all operations of charge and discharge being made without opening the polymerisation reactors. The second step consists of the collection of all VC monomer contaminated liquid effluents in a system of closed tanks, which are subsequently drained to a stripping column, retrieving VC monomer. Finally, the gaseous effluent is treated in a low temperature condensation unit, using liquid nitrogen, before being discharged into the atmosphere.

### **Concluding remarks**

From its beginnings CIRES has shown a consistent concern with environmental protection. In the 1960s its attitude was extremely innovative in the Portuguese industrial context in as much as there were neither official regulations nor a governmental environmental policy. It set internal targets regarding the quality of its effluents and in many cases its standards were higher than those established by law. CIRES, in particular, had a prominent role in the already polluted Aveiro region, by treating its liquid and solid effluents, some of them toxic, resulting from the process of manufacturing the monomer vinyl chloride. This company tried to reconcile environmentally friendly practices with economic profit. Profit was not pursued as a short run target, but was viewed in the context of the long run effects on the environment. Large investments were made in new technologies in order to minimise environmental impact.

CIRES has subscribed to most important measures of co-operation and technological development launched by national and European agencies committed to environmental protection:

“From the environmental point of view the company’s policy is exemplary. CIRES subscribed to ECVI environmental measures and to agreements aimed at reducing emissions by imposing itself more demanding specifications than those imposed legislated by the EC. This entailed large investments in technology, in particular that of the closed reactor (...).”<sup>45</sup>

From the environmental point of view, CIRES is also innovative because it does not confine itself only to the national context. Its close relationships with the most



industrialised countries, in particular Japan and the USA, has probably contributed significantly to its distinctive attitude.

Today, CIRES has a social responsibility, which means that it combines three principal parameters, economical growth, environmental protection and social cohesion. It has been environmentally certified to the ISO 14 001 standards.

In the end the company's dilemma lie between choosing between the easiest or cheapest paths or the path that which required higher costs in exchange for the protection of the environment and of the workers. Even in a country that in that period was essentially agricultural and in the process of developing industrially, CIRES notably set itself apart from current practices, having become known to associate the productive processes and economic profits with the importance of environmental preservation, and keeping in mind a vision of progress and developments in the future.

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## Notes

<sup>1</sup> Jeffrey K. Stine; Joel A. Tarr "At the Intersection of Histories: Technology and the Environment", *Technology and Culture*, 39, (1998), 601-40, on 604.

<sup>2</sup> R. Carson, "Silent Spring", (Massachusetts, The Riberside Press Cambridge, 1962).

<sup>3</sup> European Council, Estrasburgo, 6 May 1968 and Pedro Baila Antunes, "Evolução do Direito e da Política do Ambiente Internacional, Comunitário e Nacional", *Millenium*, 7, (July 1997), 100.

<sup>4</sup> Antunes, "Evolução do Direito", 100

<sup>5</sup> Antunes, "Evolução do Direito", 101.

<sup>6</sup> According to "Portaria" n° 316/7, *Diário da República*, n° 143, Iª Série, 19/6/1971.

<sup>7</sup> According to the law n° 11/87, *Diário da República*, n° 81, Iª Série, 7/4/87.

<sup>8</sup> John P. Ulhoi, "Industry and the environment: a case study of cleaner technologies in selected European countries", *Journal of Engineering and Technology Management*, 14, (1997), 259-71, on 261.

<sup>9</sup> Ulhoi, "Industry and the environment", 261-2

- <sup>10</sup> *Diário do Governo*, N.º 291, III Série de 16 de Dezembro de 1960 and *Certidão da escritura da criação da CIRES*, 1.º Cartório Notarial do Porto, 23/11/1960.
- <sup>11</sup> On 23 November 1960, with an initial investment of 20 million PTE. It began manufacturing in 1963.
- <sup>12</sup> Polymerisation by suspension is a process consisting of suspending the monomer in water by using suspension agents, the monomer being in the form of small globules. See D.C. Miles; J.H. Briston, *Polymer Technology*, (New York: Chemical Publishing Co., Inc., 1979), 256; B.G. Reuben; M.L. Burstall, *The Chemical Economy – A guide to the technology and economics of the chemical industry*, (London, Longman, 1973), 458.
- <sup>13</sup> In the polymerisation by emulsion water is also used as a means of suspending the monomer. The mechanism underlying the formation of the polymer is, however different. First the monomer is emulsified with a fatty acid or a cationic detergent. Then it diffuses through the aqueous phase, it emulsifies in micelles each of which acts as a polymerisation nucleus. Miles; Briston, *Polymer Technology*, 256-257.
- <sup>14</sup> Companhia Industrial de Resinas Sintéticas, CIRES, S.A., *Relatório Ambiental (Environmental Report)*, Lisbon, 1995, IX.
- <sup>15</sup> *Relatório 100/62*, “Esgotos da Fábrica”, 11 Setembro 1962 (CIRES Archive).
- <sup>16</sup> *Despacho de 21-8-61* and *Relatório 100/62*, “Esgotos da Fábrica”(CIRES Archive).
- <sup>17</sup> CIRES subscribed to a voluntary agreement in March 1995. The limit to VC monomer emissions stipulated 100 g per ton of PVC-S. Companhia Industrial de Resinas Sintéticas, CIRES, S.A., *Relatório Ambiental*, (Lisbon, 1998), 9-10.
- <sup>18</sup> Group Cires, *VICIRANDA – Revista do grupo*, N.º 5, Janeiro 1999, 8 and Companhia Industrial de Resinas Sintéticas, CIRES, S.A., *Relatório Ambiental*, (Lisbon, 1998), 10.
- <sup>19</sup> Ulhoi, “Industry and the environment”, 261
- <sup>20</sup> *Relatório 91/62*, “Da Reunião de Técnicos da CIRES, na Fábrica em 24/8/962 – Problema dos Esgotos da Fábrica em geral e dos da E.T.A. em particular”, 29 August 1962 (CIRES Archive).
- <sup>21</sup> *Relatório 100/62*, “Esgotos da Fábrica”..
- <sup>22</sup> *Relatório 91/62*, “Da Reunião de Técnicos da CIRES”.
- <sup>23</sup> Approximately 150 m<sup>3</sup>/h for the production of 500 tons/month of PVC. *Relatório 91/62*, “Da Reunião de Técnicos da CIRES”, 1.
- <sup>24</sup> These materials were hydrochloric acid, sodium hydroxide, calcium chloride, vinyl chloride, and mercury chloride. *Relatório 91/62*, “Da Reunião de Técnicos da CIRES”, 1.
- <sup>25</sup> *Relatório 100/62*, “Esgotos da Fábrica”.
- <sup>26</sup> Located next to CIRES's the southwest border.
- <sup>27</sup> The current of natural water was used in a public washing fountain, and was also used for irrigation.
- <sup>28</sup> *Relatório 91/62*, “Da Reunião de Técnicos da CIRES”, 2.
- <sup>29</sup> *Diário de Notícias*, “Mercúrio Contamina a Ria”, 30 April 2000. However, the company declared that an investment project is being carried out in order to replace the manufacture of chlorine in mercury cells by the membrane process. Interview given by Luis Montelobo (a chemical engineer and administrator of CIRES) to Maria Elvira Callapez, 22 May 2000. See also *Relatório 100/62*, “Esgotos da Fábrica”.
- <sup>30</sup> *Diário de Notícias*, “Mercúrio Contamina a Ria and *Relatório 100/62*, “Esgotos da Fábrica.
- <sup>31</sup> The main effluents drained to the general sewage collector were those coming from the water treatment station (WTS), process and sanitary premises. From the WTS the effluents would be suspension mud (iron hydroxide and calcium, mainly) from the decanters and from the washing

of filters, and liquids from the regeneration of the Demineralisation Section of the WTS, that is HCl and NaOH solutions. From process would come acid solutions (HCl), alkaline (NaOH) and saline (CaCl<sub>2</sub>), as well as water from PVC decanting, containing a weak suspension and mineral content, that is neutral. *Relatório 102/62* “Problema dos esgotos da Fábrica”, 25 Setembro 1962, p. 4 (CIRES Archive).

<sup>32</sup> The Vala de São Felipe, already heavily polluted, traversed an inhabited area.

<sup>33</sup> By “neutral” the company meant neutral to Red Congo Paper, that is free from mineral acids, the effluent being pH below 7 due to the presence of carbonic. *Relatório 102/62* “Problema dos esgotos da Fábrica”.

<sup>34</sup> Companhia Industrial de Resinas Sintéticas, CIRES, S.A., *Relatório Ambiental*, (Lisbon, 1994), 49.

<sup>35</sup> The treatment has been carried out since the factory was established. Treated contaminated water corresponds to a third of the water consumed by the factory. See CIRES, *Relatórios Ambientais 1994 – 1995*.

<sup>36</sup> Flootation consists of adding air to the flakes so they float in the upper part of the treatment tank where they are collected as mud. Companhia Industrial de Resinas Sintéticas, CIRES, S.A., *Relatório Ambiental*, (Lisbon, 1995), XIII.

<sup>37</sup> The organic content refers to the oxygen chemical deficit (OCD) and oxygen biochemical deficit (OBD). OCD is the parameter to evaluate organic matter in residual water. It also measures the capacity of eutrophication of drained water. Companhia Industrial de Resinas Sintéticas, CIRES, S.A., *Relatório Ambiental*, (Lisbon, 1995), 18.

<sup>38</sup> In fact the level of total suspended solids (TSS) in CIRES’s effluents is about 20 times lower than the legislated value. TSS are non-dissolved organic and inorganic particles carried by residual waters to the collector tank. Some of these deposited as sediments destroying aqueous habitats. Colloidal particles, which do not deposit quickly, make superficial water turbid. Organic particles can also cause oxygen deficit. Companhia Industrial de Resinas Sintéticas, CIRES, S.A., *Relatório Ambiental*, Lisbon, 1995, XXIII e Portaria n.º 74/90, *Diário da República*, n.º 55, 1ª série de 7 de Março de 1990. See also Companhia Industrial de Resinas Sintéticas, CIRES, S.A., *Relatório Ambiental*, (Lisbon, 1993), 76 and Companhia Industrial de Resinas Sintéticas, CIRES, S.A., *Relatório Ambiental*, (Lisbon, 1994), 50.

<sup>39</sup> *Relatório 102/62* “Problema dos esgotos da Fábrica”.

<sup>40</sup> *Informação 1/F/67*, “Resíduos do catalisador de síntese do VC”, (11 January 1967), (CIRES Archive).

<sup>41</sup> Residues are defined as any substance or object, which should be, get rid of. They are listed in the Decisão da Comissão 94/3/CE, 20 December 1993, which approves the European Catalogue of Residues (ECR). See Companhia Industrial de Resinas Sintéticas, CIRES, S.A., *Relatório Ambiental*, (Lisboa, 1996), 16.

<sup>42</sup> *Informação N.º 12/DG/74*, “Segurança na Fábrica – Proposta” (CIRES Archive).

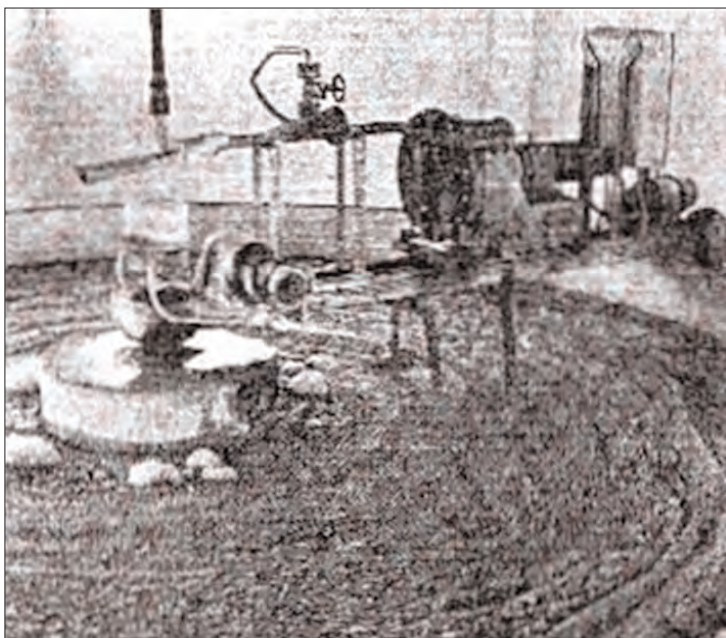
<sup>43</sup> As condições de segurança exigidas também se prendiam com a futura localização da fábrica de PVC. Seria construída próxima de um sanatório.

<sup>44</sup> The maximum allowed is 100 g of VC monomer per ton of manufactured PVC-S. See Grupo Cires, VICIRANDA - *Revista do grupo*, 5 (January 1999), 8.

<sup>45</sup> Interview given by Luís Montelobo, in Grupo Cires, VICIRANDA - *Revista do grupo*, 7 (July 1999), 3.



**Figure 1. PVC Decanting Equipment (CIRES Archive)**



**Figure 2. Sewage Station (CIRES Archive)**





Figure 3. PVC Mud Compacting Equipment (CIRES Archive)



Figure 4. Mud Park (CIRES Archive)



**Figure 5. Closed Reactor: Connections Between Suspension Tanks and Gasometers (CIRES Archive).**

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## Chemistry and Metallurgy in Portugal in the Eighteenth Century – The Cases of Gold and Silver

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Various Portuguese-speaking authors wrote about docimasy and metallurgy of several metals either in books or in manuscripts in the eighteenth century; these are valuable sources that need to be studied from the point of view of the history of technology and of the history of chemistry. The present paper aims at giving notice of the most important of such authors and about their ideas on the extraction of gold and silver from their ores. Their works are of the upmost interest although many of them are not easily accessible as in the case of the manuscripts.

Before the eighteenth century, a long tradition of mining and metallurgical activities related to gold and other metals already existed in Portugal. Tin and copper ores were extensively mined and processed in the sixteenth century in order to produce bronze for the manufacture of cannons and other guns. Also deserving mention are the gold and silver mining and processing activities carried out during the occupation of the Iberian Peninsula by the Arabs (roughly from the 8<sup>th</sup> to the 14<sup>th</sup> centuries) and those during the earlier occupation of the Peninsula by the Romans (from the 1<sup>st</sup> to the 5<sup>th</sup> centuries approximately) when the province of Lusitania, comprising parts of present day Portugal and Spain, was established. Georgius Agricola (1494-1555) refers to tin smelting and to gold mining by the Lusitanians in his *De Re Metallica*, published in 1556. Archaeological evidence of mining and processing works related to copper, tin and iron from pre-Roman times is abundant. In Brazil, a Portuguese colony since 1500, the search for gold and silver started immediately after the arrival of the Portuguese and the search for silver was much increased as soon as it was discovered in Spanish America.<sup>1</sup>

### The eighteenth century – authors and works

In 1711 the Italian Jesuit priest Giovanni Antonio Andreoni (1649-1716) published under the pseudonym of André João Antonil a book, *Cultura e Opulência*

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*do Brasil por Suas Drogas e Minas*, that described the types of occurrence of gold in Brazil (placer gold, gold in veins), some of the physical properties of the metal (colour, shape, purity) and the gold mining procedures (prospecting, extraction, dressing, panning) in use there. Silver mines in Brazil were also described by Antonil, but such information has been considered doubtful in terms of their existence and economic importance.<sup>2</sup> The author himself wrote on the front cover of his book that it contained information on how to discover silver mines in the colony thus giving the impression that they were not known at the time. He also referred to how to prospect for silver in veins, to various rock types containing silver and to two metallurgical methods of silver extraction using lead and mercury that is, applying the smelting method and the amalgamation method. The circulation of the book was quickly forbidden by the Portuguese authorities, mostly because it described in detail the routes to reach the mines, and this gives a measure of the secrecy with which these matters were dealt with by the Portuguese Crown.

Obtaining silver by subjecting ore to fire was already in use by the Native Americans when the Spaniards arrived in America and the latter continued to use the smelting method until it was replaced around 1556 by the amalgamation method (patio process). In the Potosí mines (then in Peru, now in Bolivia) the patio process was replaced by the faster pan process invented by Alvaro Alonso de Barba (1569-1662) in 1609. In the pan process the ore was mixed with salt and mercury and heated in shallow copper vessels. Most probably Antonil's description of the extraction of silver was based on Barba's *Arte de los Metales*, first published in 1624, and perhaps also on information obtained from the Jesuit missions, which existed not far from Potosí.<sup>3</sup> Since no silver occurrences of economic importance had been discovered in Brazil at the time, Antonil's book possibly expressed the hope that the metal might exist in the colony, similarly to what had happened in Spanish America.

This book, dedicated to the sugar and tobacco producers and to the owners of gold mines, is a valuable source of information about gold mining and about silver metallurgy in Brazil in the 17<sup>th</sup> and at the beginning of the 18<sup>th</sup> centuries.

In *Elementos de Chimica*,<sup>4</sup> published in Coimbra in 1788 (part I - theoretical chemistry) and in 1790 (part II - theoretical and practical chemistry), the Brazilian-born Vicente Coelho de Seabra S. Telles (*ca.* 1764-1804), presented a classification of the "corpos" (bodies) where gold and silver are considered to be ductile species of the gender metals of the order of the non self-combustible bodies –that is they may suffer "calcinação" (calcination) but only if subject to extreme heat (and so they were considered to be perfect metals)– in turn includ-

ed in the class of the combustible bodies. The elective affinities of both metals and their oxides were shown in tables: both elements have affinities with mercury and their oxides affinities with several acids. The author stressed the fact that gold and silver had very little affinity with oxygen and that their oxides were easily reduced. Elective affinity was defined as a kind of selection that a body *b* in a compound *ab* makes to combine with *c*, leaving *a* aside and originating a new compound *bc*. The physical and chemical properties of both metals were presented and used by Seabra to describe some metallurgical and docimastic procedures. According to the author, metallurgy, or the “arte” (skill) of extracting, smelting, purifying and mixing metals, was considered to be one of the most essential branches of chemistry and a specific science related to mining. Docimasy, or “arte” of assaying metals in ores, is the “arte” of knowing through laboratory procedures the nature and the average contents of metals in ores. Three docimastic methods of determining the contents of a given metal were presented by Seabra: one to be applied to hard and refractory ores using fluxes, like borax, followed by smelting, calcination or acid attack; a second one, the physical method of panning, to be used when particles of the metal are mixed with grains of sand for instance; and a third one to be applied to easily ground ores that, after grinding, were subject to dressing, then to fire, leading first to roasting and then to fusion, after what certain reagents should be added to the melt in order to separate any metal that might be mixed with the metal of interest. Seabra described clearly two methods for gold and silver analyses. A detailed description of a procedure for gold was given to assess its purity (gold assaying): through cupellation gold and silver were separated from the other metals (parting) and then the separation of gold from silver can be accomplished using *aqua fortis* and muriatic acid. As for the metallurgical methods Seabra mentioned large scale dressing and panning procedures for the processing of native gold in Brazil and to the crushing, grinding, dressing, roasting and purifying/refining steps in case the metal of interest was combined with other metals (“mineralizado”) or mixed with impurities like rock material. Even after all these steps the metal might not be totally pure and so gold and silver amalgamation should be used, a method that Seabra referred to only very briefly, amalgam being defined as the mixture or combination of mercury with any metal.

*Elementos de Chimica* was written as a textbook, based on Lavoisier’s concepts, to be used at the University of Coimbra. Seabra was successively appointed demonstrator of the Chair of Chemistry (in 1791), that included topics of metallurgy, lecturer to the same Chair (1793) and substitute-lecturer of the Chairs of Botany and Zoology (1795) and was promoted in 1801 to substitute-Professor of the Chair of Chemistry. Seabra wished that his book could be also used in Brazil,

but the impact of the book was negligible both in Portugal and in the colony, in terms of chemical metallurgy.<sup>5</sup>

In three memoirs about mineral resources in Portugal and colonies, published in 1789, and in two more, written most probably in 1792, one about the Portuguese Mint and the other one about gold mining in Brazil,<sup>6</sup> **Domenico Vandelli** (1735-1816), an Italian Professor who held the Chairs of Chemistry and of Natural History at Coimbra University and was director of several laboratories in Coimbra (University) and in Lisbon (Ajuda complex, Academy of Sciences, Portuguese Mint), presented a wealth of information about docimasy and the metallurgy of gold. The following is a summary of the key points in these memoirs:

- a) About the occurrence of gold in Portugal, he referred to the alluvial gold in the river Mondego, in the northern half of Portugal, and to the geographical areas where gold in rocks (forming veins and masses) might occur and be the source of alluvial gold. Mention was also made of alluvial gold in the river Tejo, in central Portugal, and to the gold and silver in the Iberian Peninsula, exploited in Roman times.
- b) Vandelli wrote extensively about the occurrences of gold in Brazil, either native or “mineralizado”, about the shapes of the metal (flakes, lumps, etc.), about the rocks and minerals to which it was associated and about the mining procedures for alluvial gold and for gold in veins or masses in the colony, giving technical advice about prospecting and the use of machinery.
- c) He noted that from lead occurring in Portugal he had extracted silver and also antimony, useful for laboratory purification of gold. Mercury, also found in Brazil, deserved his attention, too. Vandelli was the first to mention the association of platinum to gold in certain Brazilian gold ores, the so called “ouro preto” (black gold).
- d) Vandelli mentioned that the most common method of gold extraction in use in the Brazilian mines (washing/dressing and panning) was the one described by Agricola and by Antonio de Ulloa (1716–1795) and that the Castilians used the amalgamation method instead of panning. He stated that the amalgamation method was more expensive and less useful than the smelting method for the purpose of obtaining the gold not easily separable from gravel, sand or earth, or from rocks where it was seen in streaks. He described in detail the metallurgical method that involved smelting, cupellation and parting using either “agoa forte” or “agoa regia” (*aqua regia*), the gold becoming finally free from the imperfect metals, from lead (used in the smelting process) and from silver that was commonly combined with gold. Vandelli stated that sometimes not

even by amalgamation was possible to separate the metal from some of the very fine sand grains with which it was mixed and so it was lost.

- e) The particular cases of extractive techniques of gold from silver-bearing pyrites, from copper-bearing pyrites, either rich or poor in copper, and from gold-bearing pyrites were given special descriptions by Vandelli. These involve roasting, cupellation, cementation and parting. Pyrite was, according to the author, a very common component of the sands (“esmeril”) with which gold particles were mixed. Authors referred to by Vandelli were, among others, J. G. Lehmann (1719-1767) and J. G. Wallerius (1709-1785).
- f) Docimastic methods for gold occupied a large part of the memoirs about gold mining in Brazil and about the Portuguese Mint. A detailed description of the gold assaying procedures used in the Mint, involving smelting and the use of nitre, corrosive sublimate, *aqua fortis*, and copper, was given. The methods were strongly criticised by Vandelli who suggested that the method described in the *Mémoires de l’Académie des Sciences de Paris* of 1727, involving smelting and the use of bismuth, lead, corrosive sublimate and borax, should be used instead. He was very critical about the careless use of nitre and of *aqua fortis* and the ignorance of the assayers about the presence of platinum in gold which would affect the fineness of the latter.
- g) Vandelli also described the very old method of purifying gold with antimony and sulphur, which would result in a very pure metal. He recommended a simple laboratory procedure using “ácido de nitro fumante” (fuming nitric acid) to assess the presence of gold in common pyrites and in arsenic-bearing pyrites. Authors referred to by Vandelli in this context were, among others, H. T. Scheffer (1710-1759), B. G. Sage (1740-1824), P. J. Macquer (1718-1784), A. L. Lavoisier (1743-1794), A. Baumé (1728-1804), C. M. Cornette (1744-1794) and C. L. Berthollet (1748-1822).

The memoirs by Vandelli are particularly important because of the technical advice he gave on gold mining (which had practically no impact on the mining industry in Brazil mostly because the mine owners were adverse to the introduction of new procedures and preferred to keep mining activities based on slave labour that was less expensive) and on laboratory procedures related to gold and several of its ores (which had some impact on the procedures in the Portuguese Mint). At the beginning of his memoir about the Portuguese Mint he wrote that chemistry, after leaving aside the attempts to transmute metals, had reached a true state of perfection from which several “artes”, in particular metal refining,

did benefit, and made special mention of the good procedures in use in the Mints of England, France and Italy.

A former student of Vandelli, the Brazilian-born Manoel Ferreira da Camara (or Manoel Ferreira da Camara Bethencourt e Sa) (ca. 1762-1835) was sent, together with J. B. Andrada e Silva and J. Fragoso de Siqueira (see below), also former students of Vandelli, as naturalists affiliated to the Lisbon Academy of Sciences and paid by the Portuguese Crown, to travel for several years in various countries in Europe. The aim of their mission was to attend courses in Paris given by A. F. de Fourcroy (1755-1809) (chemistry), B. G. Sage (1740-1824) (mineralogy and docimasy) and J.-P.-F. Guillot-Duhamel (1767-1847) (mining) and to stay at the Academy of Freiberg to study geology and mining with W. A. G. Werner (1749-1817), metallurgy with W. A. Lampadius (1772-1842) and other subjects, and also to visit mines, smelters and other Academies related to mining and metallurgy. After returning from their travels they were to be involved in the reorganisation and improvement of mining and metallurgy procedures in Brazil and in Portugal.<sup>7</sup>

In an account presented to the Lisbon Academy of Sciences in 1789,<sup>8</sup> Camara wrote about the amalgamation method used in Brazil for native gold and about the smelting method used for “mineralizado” gold and non-native gold. The smelting method as practised in the “Casas de Fundição” (Government institutions where gold smelting, gold assaying and gold tax collection were carried out that existed in several mining districts in Brazil) was subject to strong criticism because of the way the purity of the gold was assessed (by visual comparison of the colour of the melt with the colour of some gold needles). Camara regretted also that, contrary to what happened in Sweden, Germany and Hungary, much gold of the “mineralizado” type was not properly extracted from its ores because of lack of skills, commenting that in those countries gold and the imperfect metals were extracted at the same time with great advantage. On the amalgamation method, Camara listed the four principles on which it was based and promised to describe in the very same memoir models of machinery and instruments for improving the combination of mercury and gold and also to describe the appropriate means of mercury mining, but such information was not given in the document.

In 1795 Camara published in Vienna, an 80 page book, *Rapport des Résultats des Expériences Chimiques et Métallurgiques Faites dans l’Intention d’Épargner le Plomb dans la Fonte des Minerais d’Argent*.<sup>9</sup> This was just an outline of a more detailed book that he intended to write one day, for the benefit of his country and fellow citizens.

In the *Rapport* he described a docimastic investigation on galenas, aiming at testing two different types of assaying and also a metallurgical experimental study on silver-bearing ores aiming at establishing a better method for their smelting by which lead would be saved and also improving the recovery of silver. For him, docimasy was a branch of metallurgy; he also stated that if the chemical mineralogist must work on each individual ore in order to know its components and the form in which these are, the metallurgist, on the contrary, has to work on the ores as they are found in nature that is mixed up.<sup>10</sup>

Camara engaged in the docimastic study in order to determine if the results of the assay by means of potash used in Harz were in agreement with the Freiberg results where the “flux noir” (a mixture of nitre and tartar) was used instead and also in order to know which assay was preferable in case of discrepancy of results.<sup>11</sup> After comparing the results of some tests that were conducted by him and by another assayer and after expounding some ideas on the physical and chemical aspects of the whole technical process, he concluded that he could not decide which the best assay was. He stated that a method that might be considered good for assaying all the types of galena was yet to be found and that the subject deserved to be better studied in order to discover an accurate, precise and easy method.<sup>12</sup>

The metallurgical investigation was a long and time consuming one that he had started because he knew, before his arrival at Freiberg, that the loss by combustion and volatilisation of lead, extensively used in the traditional way of processing the silver-bearing ores, was huge, and that the amount of silver lost in the scoriae (the leftovers of the smelting process that were thrown away) was also considerable. Having had confirmation of such information in Freiberg he decided to study the problem from a theoretical and a practical point of view, based on concepts and findings of Fourcroy, L. N. Vauquelin (1763-1829), B. Pelletier (1761-1797) and others. Camara’s ideas and the experimental work were focused on: the nature and composition of the mixtures of ores (silver ores, silver-bearing lead ores and silver-bearing copper ores) that were subject to smelting; the role of some elements present in them (sulphur, arsenic, phosphorus); the chemical processes involved (oxidation, reduction), and on the role of heat produced by the reverberating furnace that he considered the best one for the purpose. His conclusions and his suggestions to improve the method and the furnace are too long to be reproduced in full, but a paragraph summarizes the main results from Camara’s experiments: it was possible to simplify the smelting of the silver ores 1<sup>st</sup> by oxidation or combustion of the ores that contained the metal not too strongly bound; 2<sup>nd</sup> by smelting these very same ores without adding anything combustible except lead;

3<sup>rd</sup> by the fusion in furnaces applying just the heat or the caloric needed for the smelting and for the vitrifying of the metals to be obtained after separation from the silver.<sup>13</sup>

Lampadius considered that the *Rapport* deserved to be translated into German and promoted the publication in 1797 of the translation, with comments. The result was a 95 page book with Lampadius' comments (in footnotes and in a final summary), disagreeing in many aspects with Camara's ideas.<sup>14</sup>

In 1796 Camara wrote a memoir (which was never published) about the methods used in the foundries of Zalathna, in the Principality of Transylvania, of mineral dressing and of smelting which were famous all over Europe.<sup>15</sup> Ores containing both gold and silver were subject to smelting following lines that had been adopted after some testing and that led to a high degree of metal extraction even from low-grade ores. Native gold was subject to amalgamation. In the memoir Camara stated that the amalgamation method in use in Saxony at the time should be applied in Zalathna to silver-bearing minerals and even to gold-silver-bearing ores. He expressed this view possibly because he knew, before his arrival at Zalathna, that high amounts of gold in the scoriae and other losses of gold had been detected. This problem would not occur, in his view, if a "gold standard" ("padrão de ouro") was used in frequent testing of the smelting process.

Camara deserves to be commended for his continuous interest on metallurgy and docimasy, crystallised by his experiments in Freiberg and by his firm ideas.

Joaquim Pedro Fragoso de Siqueira (?–1833) was the author of a manuscript in Portuguese written in 1796 and kept at the Lisbon Academy of Sciences<sup>16</sup> about how silver was refined in the Halsbrück foundries, near Freiberg, in a laboratory called "das Brennhaus". The silver obtained either by cupellation or by amalgamation in the Halsbrück foundries and in the "Basse-Mulde" laboratory (see below) contained lead and copper as impurities. In the manuscript a very detailed description was given of how a very pure melt of silver was obtained by the careful use of a coal fired reverberating furnace and special crucibles that led to the oxidation of the imperfect metals present in the silver which were removed as a final operation. To facilitate the refining process additional lead and copper were added to the melt. The purity of the silver in the melt was assessed by visual inspection.

Siqueira was also the author of 107 page bi-lingual book (in French and in German) where he described the amalgamation and smelting procedures in use in



the foundries of Halsbrück<sup>17</sup> for silver. The book, according to what may be read on the front cover, was intended to serve as a guide for foreigners who wanted to visit the premises and for the youngsters who wanted to study the subject in Freiberg. The very descriptive text, accompanied by a couple of drawings, was in two parts, and an appendix. The first part, about amalgamation, included chapters on, the history of the method in Saxony; the choice, preparation and composition of the minerals to be amalgamated; the roasting of the minerals; the method of sieving, grinding and pulverizing them, and the amalgamation and mercury distillation procedures. The second part, on smelting, had ten chapters on, the roasting and smelting of various silver-bearing lead ores, of copper ores and of the products of such operations; the refining (“affinage”) of silver from the lead ores and from the amalgamation process; the re-smelting and the refining of the lead materials resulting from such operations, and (last chapter) the second refining of silver. This last chapter is practically the translation of the 1796 manuscript referred to above. In the appendix mention was made of a pilot laboratory called *Basse-Mulde* intended for amalgamation operations to be conducted before the smelting procedures; two other foundries in Freiberg; published works about amalgamation in the Saxony and about foundries in Freiberg, and to notable things that could be seen in the city. Siqueira’s book, although purely descriptive (he does not express any views about what he saw) was published in Germany in the two important technical languages of the times.

Manuel José Barjona (1758–1831) wrote in Latin (*Metallurgiae Elementa*) the first book on metallurgy ever published in Portugal.<sup>18</sup> This was in 1798, whilst in Coimbra, when he was substitute-Professor of Chemistry and Physics teaching mainly Experimental Physics. After the start in 1787 of an academic career he reached the top in 1801 when he became Professor of Metallurgy. The book, after a short introduction on some aspects of the history of metallurgy, was divided in two parts, the first on metallurgy and the second on docimasy (1<sup>st</sup> section) and montanistic, or the “arte” and science of exploiting ore deposits (2<sup>nd</sup> section). Docimasy was defined by the author as the science that deals with laboratory assaying using small amounts of ores in order to determine their contents in metals. Metallurgy was defined as the science that, based on the findings of docimasy, dealt with methods by which metals and semi-metals are extracted from ores in large amounts, having in mind their uses by Man.

The first chapter of part I of the book was about the metallic substances in general (their physical and chemical properties, in which he used concepts from Lehman, A. F. Cronsted (1722-1765), H. Cavendish (1731?-1810) and Lavoisier,

among others), and the classification of the forms in which the substances were found in nature. Gold and silver were genders of the order of the perfect metals (those that if subject to friction do not originate any taste or smell). Seven compounds (“espécies”) of gold and thirteen compounds of silver were listed, in both cases established on the basis of their combination with other metals or semi-metals. In the second chapter Barjona describes the physical and chemical operations (comminution, smelting, refining, parting, etc.) to which ores are subject, as well as the instruments used for that purpose. In chapter three the basic rules, mostly chemical, that must be used in metallurgical and docimastic procedures were presented.

Part II of the book starts with a chapter on objectives and types of docimasy (either dry or humid) and on general operations, some twenty names of authors being listed. Barjona concluded that humid docimasy was preferable to the dry type. The second chapter is about the chemical analysis of metals and the procedures for all the species of gold and silver are described in detail. The third and last chapter of this section was about cupellation and parting of the perfect metals. For Barjona silver was the easiest metal to subject to cupellation. Parting of silver from gold was described in detail. The second section (on montanistic) started with a chapter on the geology and mining of veins containing metals. The following, last chapter was about metallurgical works and in it he described the washing, grinding and amalgamation procedures relative to native gold and silver, as well as the operations to extract both metals from their various species by the smelting method.

A facsimile edition of *Metallugiae Elementa* edition containing a translation into Portuguese and also comments by Prof. A. Morais Cerveira (Universidade do Porto) and Prof. M. Portugal V. Ferreira (Universidade de Coimbra) was published in 2001.<sup>19</sup> The points made in the comments were that, a) the book was clearly the work of a man much more interested in physics than in mineralogy, mining or chemistry (he uses concepts from the phlogiston theory and from Lavoisier’s chemistry); b) Barjona was much more prepared to write about docimasy than about metallurgy, but in both cases he relied heavily on many authors, mainly when writing about metallurgy; c) the structure and the general tone of the book, as well as the fact that it was written in Latin in a time when other textbooks were in Portuguese, reveals his conservatism, although as far as the new Statutes (1772) of the reformed university was concerned, the textbooks should be written in Latin; d) the book anticipated the creation of the chair of Metallurgy. After the reform of the University, the Congregation of the Faculty of (Natural) Philosophy recommended that the Professors should publish textbooks on their

disciplines; Barjona's book is one of the rare examples of compliance; f) no reference to *Elementos de Chimica* is made by Barjona.

The Brazilian-born José Vieira Couto (1752–1827), also a former student of Vandelli, was the author of a memoir dated 1799 about the “capitania” (administrative territory) of Minas Gerais, Brazil, where he presented his ideas on how metallurgical activities should be conducted in Portugal.<sup>20</sup> Couto was sent there by the Portuguese government in order to make an inventory of the mineral resources of the area and see what gain Portugal could get from their exploitation and from the discovery of new mines. At the time, the Brazilian gold (and diamond) mines were already in a state of decadence that had started in the 1750/1760's, in spite of the efforts of the Portuguese Crown.<sup>21</sup> Couto thought that Portugal, having already the mines, the miners and vast areas to be prospected for minerals mostly in Brazil, should have a metallurgical industry that in his view, should start with the preparation of a treatise about three types of metallurgical works, namely: a) “metalurgia mecânica” (mechanical metallurgy), dealing with mineralogy, geology and mining methods to be applied mainly in the gold mines, the author making a brief mention of the amalgamation method and to smelting method using lead to extract the metal; b) “metalurgia pirotécnica” (smelting and the previous preparation of ores) related to other metals and dealing also with non-metallic substances used in metallurgy; c) “metalurgia docimástica” (docimastic metallurgy), or the “arte” of gold and silver assaying, aiming at giving: i) the assayers the necessary training to work in the “casas de fundição” and in the mints; ii) those working in the foundries and in the mines the necessary training to become able to assess properly in a recurrent way the results of their technical activities. The chapter of Couto's memoir dealing with metallurgy ended with a small list of requirements that the potential author of the treatise should fulfil: be good in physics and in mineralogy, have been in Brazil and have seen the mines in Saxony, Hungary, Transylvania and Austria. Possibly he was thinking of either Camara or Andrada e Silva. A second fundamental step to establish a metallurgical industry, according to Couto, would be the construction of foundries to produce iron that was absolutely necessary for the mining activities.

Couto in a sense was an idealist since apart the suggestions above, he gave others including the reduction of holidays to increase productivity of the mines.

In 1801 a Chair of Metallurgy was created at the University of Coimbra, thus separating it from Chemistry, and it was occupied for some twelve years, from 1801

to 1813, by the Brazilian-born José Bonifácio de Andrada e Silva (1763–1838) who had spent ten years in Europe and had gained an excellent reputation as a mineralogist. As professor in Coimbra he apparently was not the most assiduous member of the teaching staff as far as classes were concerned and also tended to miss faculty meetings. A sort of laboratory (the “Gabinete de Metalurgia”) was established in 1801 under his direction. He promoted the preparation of a dissertation on the amalgamation of silver ores by one of his post-graduate students in 1805. He never published any book or paper on metallurgy.<sup>22</sup>

In 1801, a chemical laboratory, the Royal Chemistry Laboratory, was established in the Portuguese Mint in Lisbon as a section of the University of Coimbra. Andrada e Silva was its first director. The next year a chair of Docimasy was created in the Mint and was taken by João António Monteiro (1769–1834), also a former student of Vandelli, and substitute professor of Metallurgy from 1801 to 1804 at Coimbra. However, being most of the time in Paris working with the great mineralogist R.-J. Haüy (1743–1822), Monteiro never paid too much attention to the Chair.<sup>23</sup>

### Concluding remarks

The number of written works on metallurgy and docimasy (six books and nine memoirs) that were prepared in the 18<sup>th</sup> century by the seven Portuguese-speaking authors is probably much larger than one could expect from a country in the periphery of science and technology. Except the textbook by Seabra (on chemistry) all the others are technical ones. The quality varies a lot from work to work; in the opinion of the present authors the names of Seabra and Camara followed by Vandelli and Barjona stand out. Three of the books deserved to be published in Vienna and in Dresden, the rest were published in Portugal, either in Coimbra or in Lisbon. As for the memoirs, only four of them, by Vandelli, were then published, by the Lisbon Academy of Sciences. This in a sense gives support to the idea that if academic or formal science was not a strong point in Portugal at the time, technical applications were of much interest in Portugal and Brazil.<sup>24</sup>

The last fifteen years of the century was in fact the period when such publishing activity took place in force: from 1788 onwards all the memories and books, except Antonil’s were printed. It was certainly expected that the advanced metallurgical techniques described in them for the extraction of gold and silver (either contained in lead ores or native) from ores in Brazil and Portugal would be put in practice. Most of the works contain detailed descriptions of the smelting and

amalgamation methods as they were practiced in Europe.<sup>25</sup> None of them describes in detail the amalgamation procedures as used in Brazil and so we have to rely for the purpose on W. Eschwege's *Pluto Brasiliensis*, the original edition of which was published in 1833. The authors of the works referred to in the previous sections were all well aware of what was going on abroad, they cited numerous authors, books and memoirs; many of them were of great reputation. Even so, their works had no visible impact either in Brazil or Portugal. For one reason, no industrial foundries for gold and silver existed in Portugal and Brazil and even the Portuguese Mint and the "Casas de Fundição" in Brazil did not change their procedures much right after the publishing of the books and memoirs. We believe that two factors contributed to this state of affairs: one of them was that the abundance of native gold was still sufficient for it to be collected by the traditional procedures of panning; the other factor was that its replacement by new techniques would not bring any advantage as compared with the slave labour used both in mining and agriculture.

It is interesting to note that various authors (Seabra, Camara, Couto) and the naturalist related to metallurgy, Andrada e Silva, were Brazilian-born and also that in general they dedicated their works to their fellow citizens and to Brazil. At the time many students from Brazil would go to Coimbra, the only university in Portugal, and showing special interests in Natural Sciences they enrolled in the Faculty of (Natural) Philosophy.

The metallurgical processes in use up to 18<sup>th</sup> century were mainly understood as physical ones in the sense that the amalgams were considered to be mixtures and methods in foundries depended on the use of fire, even if heat had a double role, physical and chemical. It was in the 18<sup>th</sup> century that a strong impulse arose to understand the chemistry of such processes (oxidation, reduction, etc.). Only later on, with the development of chemistry, were the chemical mechanisms involved in the processes of extracting the noble metals from a great variety of ores, many of them of complex nature, fully understood. Improvements on the extraction of gold and silver from their ores had resulted mostly from experience and experimentation such as the work by Camara, with the several types of silver-bearing ores, and of Vandelli, with the several types of gold-bearing ores, demonstrate.

The Portuguese Crown, influenced by the physiocratic ideas coming from the rest of Europe, was the ultimate promoter of the technical development that Portugal urgently needed to revitalise the mines in Portugal and Brazil and discover new ones, not only the gold mines, but also the iron, coal and mercury mines. Sending naturalists who had graduated in Coimbra either to the colonies to carry out the so called "viagens geognósticas" (geognostic travels)<sup>26</sup> or to the best European cen-

tres where mining and metallurgy were taught and practised were two important decisions taken by the Crown. At the same time the Crown was happy to receive technical reports about amalgamation and smelting from the Portuguese legations abroad, namely in Turin, Vienna and Copenhagen, as happened between 1793 and 1795.<sup>27,22</sup> The Crown had forbidden the circulation of information related to the gold mines in Brazil, as in the case of Antonil's book and probably as in the case of the memoir by Vandelli on the Brazilian gold that was not published, contrary to what was normal, in the *Memorias Economicas* of the Lisbon Academy, being published only 100 years later, in Brazil.

*The authors thank Prof. Helmut Malonek for his help in dealing with the works in German.*

## Notes

<sup>1</sup> Important references to the history of mining and metallurgy in Portugal are: G. Agricola, *De re metallica* (New York: Dover Publications, 1950); J. C. Allan, "A Mineração em Portugal na Antiguidade", *Boletim de Minas* 2, 3 (1965): 139-17; A. Castro, "Desenvolvimento das Actividades Produtivas", in *História de Portugal 1245-1640*, dir. José H. Saraiva, 2 (Lisboa: Publicações Alfa, 1983): 209-222; C. Fabião, "O Passado Proto-histórico e Romano", in *História de Portugal*, dir. José Mattoso, 1 (Lisboa: Círculo dos Leitores, 1992): 77-299; M. F. Rodrigues and L. M. Mendes, *História da Indústria Portuguesa – da Idade Média aos Nossos Dias* (Mem Martins: Publicações Europa, 1999); A. C. F. Silva, "A Idade dos Metais em Portugal", in *História de Portugal Origens – 1245*, dir. José H. Saraiva, 1 (Lisboa: Publicações Alfa, 1983): 101-148; C. Torres, "O Garb-Al-Andaluz", in *História de Portugal*, dir. José Mattoso, 1 (Lisboa: Círculo dos Leitores, 1992): 363-416. See at [www.pmt.usp.br/notas/notas.htm](http://www.pmt.usp.br/notas/notas.htm) a concise history of metallurgy in Brazil: F. J. Landgraf, A. P. Tshipschin and H. Goldenstein, "Notas sobre a História da Metalurgia no Brasil (1500-1850)".

<sup>2</sup> André João Antonil, *Cultura e Opulência do Brasil por Suas Drogas e Minas* (Lisboa: Publicações Alfa, 1999). Doubts about the existence and the economic importance of the silver mines as described by Antonil are expressed in W. L. Eschwege, *Pluto Brasiliensis* (Belo Horizonte: Livraria Itatiaia Editora, Lda. and S. Paulo: Editora da Universidade de São Paulo, 1979) and also in A. Mansuy, *Cultura e Opulência do Brasil por suas Drogas e Minas* (Institute d' Hautes Études de l'Amérique Latine, Paris: 1968).

<sup>3</sup> In the 17th century a link did exist between the Portuguese settlers from S.Vicente, in Brazil, and Potosí, using a land path through Guairá, where a Jesuit mission operated.

<sup>4</sup> Vicente Coelho de Seabra, *Elementos de Chimica* (Real Officina da Universidade): MDC-CLXXXVIII). A facsimile edition was published in 1985 by the University of Coimbra, the cover of which mentions that Part I was printed in 1788 and Part II in 1790.

<sup>5</sup> D. Thornburn Burns, Maria Filomena Camões, António M. Amorim da Costa, "Distinguished People and Places Important in the History of Portuguese Analytical Chemistry", *Microchim Acta*, 152, (2005):137-151, write, on 139, that the book "... was written with the intention of being

used in a course be taught by the Sociedade Literária do Rio de Janeiro but which never took place". It has been pointed out that M. J. Barjona did not make any reference to Seabra's book, published 10 years before. And M. P. Ferreira (see note xxii) comments that the book had been received with indifference by his colleagues.

<sup>6</sup> The references for the five memoirs are as follows: D. Vandelli, "Memoria sobre Algumas Produções Naturais deste Reino, das Quais se Poderia Tirar Utilidade", *Memorias Economicas da Academia Real das Sciencias de Lisboa* I (1789): 176-186; D. Vandelli, "Memoria sobre Algumas Produções Naturais das Conquistas, as Quais ou São Pouco Conhecidas, ou não se Aproveitam", *Memorias Economicas da Academia Real das Sciencias de Lisboa* I (1789): 187-206; D. Vandelli "Memoria sobre as Produções Naturais do Reino, e das Conquistas, Primeiras Matérias de Diferentes Fábricas, ou Manufacturas", *Memorias Economicas da Academia Real das Sciencias de Lisboa* I (1789): 223-237; D. Vandelli, "Memoria III Sobre as Minas de Ouro do Brazil", *Anais da Biblioteca Nacional do Brasil* 20 (1898): 266-278; D. Vandelli, "Memoria sobre a Casa da Moeda e Prejuízo que Sofre a Real Fazenda e o Publico pela Falta dos Conhecimentos Quimicos", in "*Domingos Vandelli, Aritmética Política, Economia e Finanças*" dir. J. Vicente Serrão (Lisboa: Banco de Portugal, 1994): 85-90.

The importance of "Memoria III", published in 1898 and more recently in Domingos Vandelli, *Memórias de História Natural*, Introd. and Edit. Coord. José Luís Cardoso (Porto, Porto Editora, 2003): 29-42 has been pointed out in M. Serrano Pinto, "D. Vandelli e a inventariação dos recursos minerais brasileiros", *Actas do 1º Congresso Luso-Brasileiro de História da Ciência e da Técnica*, (Évora: Universidade de Évora, 2001): 214-225.

<sup>7</sup> For details about these travels see Manuel S. Pinto, "A Experiência Europeia de Manoel Ferreira da Camara e seus Reflexos no Brasil - Algumas Notas", in "*Geological Sciences in Latin America - Scientific relations and exchanges*", org. S. Figueirôa e M. Lopes, (Campinas, 1994): 245-264.

<sup>8</sup> Manoel F. Camara, "Memória de Observações Físico-económicas acerca da Extracção do Ouro nas Minas do Brasil", *manuscript in the British Museum, Catalogue Addit. Manusc. 1841 - 1845*, MSS # 15191: 94-122, London. The memoir is reproduced in M. C. Mendonça, *O Intendente Câmara* (S. Paulo: Companhia Editora Nacional, 1958).

<sup>9</sup> Manoel F. Camara, *Rapport des résultats des expériences chimiques et métallurgiques faites dans l'intention d'épargner le plomb dans la fonte des minerais d'argent* (Vienne: Imprimerie de Patzowsky, 1795).

<sup>10</sup> Camara *Rapport*: "...si le chymiste minéralogue doit opérer avec chaque individu pour faire connoître ses parties constituantes, et l'état dans lequel elles se trouvent, le métallurgiste au contraire doit agir avec les minerais tels qu'ils se trouvent dans la nature, c'est à dire, melangés." 38.

<sup>11</sup> Camara *Rapport*: "...si l'essai par moyen de la potasse usité au Hartz concordoit avec celui qu'on fait par le moyen du flux noir usité à Freyberg ... et pour connoître lequel de deux, dans le cas de discordance, étoit préférable." 70.

<sup>12</sup> Camara *Rapport*: "...qu'on est encore loin d'avoir trouvé une méthode qui puisse être considérée comme bonne pour essayer toute sorte de galenes; que cette matière... merite d'être travaillée et approfondie, à fin de trouver une méthode sûre, invariable [sic] et facile..." 80.

<sup>13</sup> Camara *Rapport*: "D'après ces expériences j'ai vû qu'on pourroit, comme je l'avois pensé, simplifier beaucoup la fonte des minerais d'argent et cela 1º. par oxidation ou combustion des minerais qui le contiennent poussé pas trop loin. 2º. Par la fonte de ces mêmes minerais sans addition d'autre corps combustible que de plomb. 3º. Par la fusion faite dans des fourneaux ou on



employroit seulement la chaleur ou le calorique comme nécessaire à la fonte et à vitrifier les métaux à avoir après le depart de l'argent." 40.

<sup>14</sup> Manoel F. Câmara, *Resultate chemischer und metallurgischer Erfahrungen in Absicht der Bley-esparung bei dem Schmelzprozess* (Dresden: Waltherischen Bofbuchbanlung, 1797).

<sup>15</sup> Manoel F. Câmara, "Nota sobre a Extracção nas Minas do Transilvânia Escrita em Zalathna aos 5 de Março de 1796", *manuscript*, Arquivo Nacional da Torre do Tombo, Núcleo do Ministério dos Negócios Estrangeiro, caixa 526, Lisbon. See Manuel S. Pinto "A Memoir Written in 1796 by Manoel Ferreira da Câmara about Mining in Transylvania", in *INHIGEO Meeting Portugal 2001 'Geological Resources and History', Proceedings* (Aveiro, Universidade de Aveiro, 2003): 363-372.

<sup>16</sup> Joaquim Pedro Fragoso de Siqueira, "Memoria sobre o Modo de Refinar a Prata em Grande, como se Pratica em Freyberg, na Saxónia Eleitoral", *manuscrito Série Azul 374, Academia das Ciências de Lisboa*.

<sup>17</sup> J. P. Fragoso de Siqueira, *Description Abrégée de Tous les Travaux, tant d'Amalgamation, que des Fonderies qui Sont Actuellement en Usage dans les Ateliers d'Amalgamation et des Fondries de Halsbrück, près de Freyberg* (Dresden: 1800).

<sup>18</sup> Emmanuel Josephus Barjona, *Metallurgiae Elementa* (Conimbricæ: Typis Academicis, MDC-CXCVIII).

<sup>19</sup> The facsimile edition with the title "*Elementos de Metalurgia*", published in 2001 by the University of Coimbra, has a "Apresentação" by Prof. A. Morais Cerveira (p. V to IX) and is followed by the translation into Portuguese ("*Elementos de Metalurgia*") that has a "Posfácio" by Prof. Martim Portugal Vasconcelos Ferreira (p. 3 to 7).

<sup>20</sup> José Vieira Couto, *Memória sobre a capitania das Minas Gerais; seu território, clima e produções metálicas* (Belo Horizonte: Fundação João Pinheiro, 1994).

<sup>21</sup> About such efforts see for instance Manuel S. Pinto, "Aspectos da História da Mineração no Brasil Colonial", in *Brasil 500 anos - A Construção do Brasil e da América Latina pela Mineração*, ed. CETEM/MCT (Rio de Janeiro, 2000): 23-40.

<sup>22</sup> M. Portugal Ferreira, "José Bonifácio d'Andrada e Silva (mineralogista, académico, mineiro do início do século XIX)", *Memórias e Notícias, Publ. Mus. Lab. Mineral. Geol., Univ. Coimbra* 106 (1988): 19-32. See also Martim R. P. Vasconcelos Ferreira, *200 Anos de Mineralogia e Arte de Minas* (Coimbra: Universidade de Coimbra FCTUC, 1998).

<sup>23</sup> Burns, *et al.*, *Microchim Acta*, 152, (2005):137-151, on 138. See also M. Portugal Ferreira, O Museu de História Natural da Universidade de Coimbra (Secção de Mineralogia e Geologia) desde a Reforma Pombalina (1772) até à República", *Memórias e Notícias, Publ. Mus. Lab. Mineral. Geol., Univ. Coimbra* 110 (1990): 53-76.

<sup>24</sup> About this topic see Carlos Filgueiras, "Havia Alguma Ciência no Brasil Setecentista?", *Química Nova* 21, 3 (1998): 351-35.

<sup>25</sup> See Donata Brianta, "Transmissione del Sapere Técnico nell'Industria dei 'Non Ferrosi' e Circolazione dell' Ingegnere Minerario in Europa e in América Latina (Meta XVIII-Metà XIX Sec.) 1750-1850", *Richerche di Storia Sociale e Religiosa* 58 (2000): 127-246.

<sup>26</sup> W. Simon, *Scientific Expeditions in the Portuguese Overseas Territories (1783-1808)*, (Lisboa: Instituto de Investigação Científica Tropical CECA, 1983).

<sup>27</sup> Pinto, "A Experiência Europeia", 258.

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## Chemistry “Laboratory Hands” in Portugal in the Nineteenth Century: Aspects Of An Evolution

*Isabel Cruz;\* Sandra Lopes\**

The present document approaches the question of a specific professional area (so to speak) –the laboratory hand– and his role in the development of Chemistry and the teaching of the subject in Portugal. The objective is to outline the general points of evolution within the perspective of their work with the Chemistry lecturer, professor or investigator, and thus provide a better understanding of the historic process of Chemistry and its development in the educational institutions of the XIX century.

The text “*Preparadores de Química da Escola Politécnica (1837-1856)*”, already showed that these “laboratory collaborators” were receiving very little recognition.<sup>1</sup> Certain official sources (yearly publications, reports and directives) plus archived documentation however provides information in a number of cases as to who the hands were, the regulations and the tasks related to their work in the laboratory and makes it possible to evaluate aspects of their role in the field of Chemistry in Portugal.

### **Chemistry laboratory hands and Educational Institutes in Portugal in the XIX century (some examples)**

In both the polytechnic scientific institutes created in 1837 in Lisbon and in Oporto, respectively called “Escola Politécnica” and “Academia Politécnica” a space, which was called a laboratory, was allocated right the beginning for the practice of Chemistry; <sup>2,3</sup> here are to be found the lecturers of the subjects (or subject) of Chemistry, those men totally responsible for the subject, the lecturers who were to substitute them and the preparers (translated in this text as laboratory hands). Although relatively ignored in the various scenarios history recreates

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about the past of the XIX century institutes in Portugal, the laboratory hands were in fact extremely important in ensuring the normal functioning of the laboratories and the teaching of Chemistry.

In spite of fundamentally employed to assist the lecturer and relieve him of the so called minor preparatory tasks necessary for the practical classes and investigation, the laboratory hand would, when needed, alternate between a mere laboratory assistant helper to practically an assistant lecturer. It is clear however that a barrier existed that prevented lecturer and hand –each with a totally independent professional situation– from being confused one with the other.<sup>4</sup>

Research carried out, particularly in these two institutes, shows that as a rule the laboratory hand was recruited from within the pharmaceutical class<sup>5,6</sup> –less clear however and difficult to evaluate with accuracy was their level of qualifications– a “Grade I” Classification signified having completing the School of Pharmacy course which was attached to the School of Medicine and Surgery. A “Grade II” Classification signified having spent a number years of practice under the supervision of a pharmacist or an apothecary.<sup>7</sup>

The first answer to the next question, why pharmacists were recruited for laboratory hands, is based on investigations of a substantive nature, carried out by two learned individuals on the History of Pharmacy in Portugal (XVIII and XIX centuries). In 1991 the work of José Pedro Sousa Dias refers to the fact that apart from the Galenic-Hippocratic remedies for bleeding and purging, which were very dominant in the XVI a XVIII centuries, new therapeutic remedies were being introduced in Portugal, for example, treatment with chemical medicines such as metallic salts, mainly antimony and mercury, other medicinal substances obtained from distilling vegetable drugs and the use of drugs coming from the New World. As a result, a group of individuals appeared, although still very small, associated with practising Pharmacy and skilled in preparing chemical medication, a practice that required firm knowledge of a set of techniques used to obtain pure active substances as opposed to complex mixtures obtained using the Galenic method.<sup>8</sup> An identical perspective, but for a subsequent period including the first half of the nineteenth century and focused on the teaching of pharmacy at Coimbra university, is presented by Rui Pita, 1995: “It must be remembered however that the chemical knowledge of the apothecaries was practical and not theoretical, preparations were obtained solely by means of practice. On the other hand, the fact that the young apprentice apothecaries came into contact with the techniques and the chemical processes in a place exclusively dedicated to these procedures, quite obviously provided them with a level of knowledge and a certain sensibility regarding the chemical question which

would, without doubt, be extremely useful in their professional activities. Furthermore: even this very incipient chemical knowledge, compared with the knowledge that existed at the time, which was virtually non-existent, or which remained in the hands the master apothecaries who instructed their apprentices, was in all truth quite relevant”.<sup>9</sup>

The second answer is of a socio-professional nature. As the job of laboratory hand is viewed by the lecturing community as being subordinate, in truth an “unqualified” position in an educational perspective, (for this same reason, right from the start an insurmountable gap separates the two), the professional group of pharmacists –those of which could master the daily regime of the Chemistry laboratory– were the ones best suited to the position.

However, we notice that not all pharmacists had the right profile for the position, indeed of all the cases of laboratory hands in the 1st half of the XIX century that we researched, only one left us in doubt as to whether or not he had in fact studied Chemistry. Therefore, and in conclusion, pharmacists were accepted as laboratory hands if they had accumulated a certain amount of scientific knowledge in the area of Chemistry.

Names of some of these individuals to be found in the records are: Francisco Mendes Cardoso Leal Júnior the former laboratory hand in the Chemistry laboratory of the “Escola Politécnica ” (The Polytechnic School of Lisbon) who attended Luis da Silva Mouzinho de Albuquerque’s Physics and Chemistry Course at the Chemistry Laboratory of the “Casa da Moeda de Lisboa” (The Lisbon Mint); José António de Aguiar (1812-1850), Francisco Pereira Amorim de Vasconcelos (1812-1859) and Manuel Nepomuceno (1830-1911), all laboratory hands at the Chemistry laboratory of the “Academia Politécnica” (The Polytechnic Academy of Oporto): the first was a interim laboratory hand, and after completing his course at the Polytechnic Academy was given the post of substitute lecturer; the second was a temporary laboratory hand who attended Chemistry classes at the Polytechnic Academy of Oporto and the last (and the first to be nominated for the position) as a pharmaceutical student at the School of Medicine and Surgery in the same city, also studied Chemistry at the same institute.

During the 2<sup>nd</sup> half of the XIX century other educational institutes start to appear in these cities: the Industrial School in Oporto and the Industrial Institute in Lisbon, and also the Agricultural Institute, in Lisbon only. Although we cannot yet be certain, we are of the opinion that some of the basic differences between these new Schools, whose main objective was to train professionals at a less qualified level, and the other already existing Schools (Coimbra University,

Polytechnic Academy and Polytechnic School), could have facilitated a certain approximation between the lecturers of Chemistry and their laboratory hands, an approximation that also came to have an affect on some of the most elite orthodox schools.

We can quite safely state therefore that the role of the laboratory hand was undergoing transformation. On one hand, a tendency is arising in the industrial institutes where individuals are working in the laboratory and are called “assistants”. These individuals are certainly well qualified and can be closely compared to laboratory assistants in the rest of Europe, namely France, Britain or Germany. In this category we find the German Carl von Bonhorst (?-1918), previous assistant to Remegius Fresenius who occupied right from the start the position of assistant created in 1872 by António Augusto de Aguiar in the Chemistry laboratory at the Industrial (and now) Commercial Institute of Lisbon.<sup>10,11</sup>

The position “Head of Practical Work” also appeared in the laboratory field (in Polytechnic School, more precisely),<sup>12,13</sup> and fitted, but not for long, Charles Lepierre (1867-1945), one of the first students to graduate from the Municipal School of Industrial Physics and Chemistry of Paris. Lepierre was persuaded by José Júlio Bettencourt Rodrigues (professor in the Industrial and Commercial Institute of Lisbon and also in the Polytechnic School of Lisbon) in a meeting at Roberto Duarte Silva’s house to leave Paris and come to Portugal to take up the post of laboratory hand for the subject of Chemical Technology.<sup>14,15</sup> This was a variation of the subject introduced in 1884 in the Industrial and Commercial Institute of Lisbon and which Bettencourt intended to be a true innovation in the field of technical teaching in Portugal, directed mainly at those individuals who graduated in Commerce with a specialized knowledge in commercial chemistry.<sup>16</sup>

The introduction in the Chemistry Laboratory of a position, a working relationship, between the laboratory hand, as he was still known, and the lecturer or professor –a process taking place in Portugal during the decades of 1870/1880– is evident by the appearance not only of terms such as “assistant”, but also “head of practical works” or “head of chemistry services” in laboratory regulation terminology.

It is also evident that not only the pharmacists were applying for the job of laboratory hand as happened for example 20 to 30 years previously. Applications are being presented by those individuals who studied at the Chemistry laboratory of the Industrial and Commercial Institute of Lisbon<sup>17</sup> or the Chemistry laboratory of the Agronomy and Veterinary Institute (the former Agricultural Institute), very often possessing extra knowledge acquired during technical working visits

abroad, or with extra working experience acquired under the orientation of foreign lecturers; as an example, we quote the case of the agronomist César Justino de Lima Alves who worked during a certain period (2 years more or less) in two chemical-agricultural laboratories, one in Paris and the other in Berlin;<sup>18,19</sup> he was later given the position of laboratory hand of “Organic Chemistry” at the Polytechnic School, and also laboratory hand of the 6<sup>th</sup> subject, “Inorganic Chemistry”, at the same institution.

These individuals are now professionally recognised as competent analysts and as a result the profile of the laboratory hand starts to undergo change. Perhaps due to this or because the mentality of the Portuguese scientific elite adapted to new influences, is the reason why not only do their names appear together with that of the lecturer in scientific works published under the auspices of the Academy of Sciences, but their own articles of renowned value are duly recognised –the case of Miguel Ventura da Silva Pinto (during many years the Physics and Chemistry laboratory hand in the Industrial and Commercial Institute of Lisbon), or Emílio Dias (Organic Chemistry laboratory hand at the Polytechnic School)<sup>20</sup> –specializing in the areas of scientific and technical instrumentation.

With the approach of the end of the XIX century, some of these laboratory hands, practical chemistry supervisors, laboratory assistants, were given an opportunity to follow a teaching career, as for example Manuel Nepomuceno, who began as a laboratory hand at the Industrial Institute of Oporto (as well as at the Polytechnic Academy), and later became a Chemistry professor at the same Institute;<sup>21</sup> Carl von Bonhorst who had been practical chemistry assistant for over a decade at the Industrial and Commercial Institute of Lisbon, later moved to the Marquês de Pombal Industrial School, to become its Chemistry professor;<sup>22</sup> Charles Lepierre, who rapidly moved from the laboratories of the Polytechnic School and the Industrial and Commercial Institute of Lisbon to the Brotero Industrial School in Coimbra, where he too was responsible for the subject of Chemistry,<sup>23</sup> and also Luís António Rebelo da Silva, who as an agronomist could apply for the position of head of the chemistry services at the General Institute of Agriculture in 1884 and in 1887 became professor of Chemistry at the same Institute, by that time already named The Agronomy and Veterinary Institute.<sup>24,25</sup>

### **The laboratory hand in the tradition of Fremy**

Historically, in the first laboratories, the laboratory hand was a trainee chemist, learning how to manipulate and carry out the most elementary chemical prepara-

tions. This was a process that began as being very exclusive, as Edmond Fremy said in his *Les Laboratoires de Chimie: le Laboratoire de Chimie Inorganique au Museum* – “for a long time, professors would not allow anyone into their laboratories except their own laboratory hand”– supervised by a great chemist, whom he hoped to succeed in the future. Thus, the laboratory hand although assisting the Master was a disciple not an employee.

A certain intensification in laboratory training which marked the transition of these “modest” laboratories into “investigation laboratories” (both terms used by Fremy), an evolutionary process, led above all by Justus Liebig, the creator of the process, but where others, many of them his pupils, also played an important part in spreading the concept, for example, Wurtz, Hofmann, Bunsen or Fresenius, must have modified to a certain extent the routines of a chemist in training. A comment of Fremy throws light on the problem. With the new teaching methods used to form chemists not enough time was given to consolidating the elementary practices of manipulation and preparation, thus young investigators threw themselves into research without having mastered certain fundamental procedures, that is, and using Fremy’s words: “students terminated without concluding their chemical studies and were not sufficiently qualified to prepare a course”.

With the development of another laboratory concept, the teaching laboratory, and in particular the Laboratory at the *Museum*, established by Fremy in 1865, the importance of the basic qualifications of chemists assumes greater proportions in as much as the “préparateurs” are equal to “true chemists”, that is, those individuals who studied during four years the different areas of Chemistry – general, organic and mineral - in permanent confrontation between theory and practice. After qualifying with a bachelor degree in Science, the Fremy laboratory assistants, the “préparateurs”, could work in the scientific or industrial laboratories. Some of them managed to reach top positions in teaching and were known as “true scientists”. It is therefore important to point out that whatever the point of view adopted abroad (France and Germany in particular), the laboratory hand is a chemist. Within the context of teaching he is responsible, inclusively, for accompanying the students; but above all, he has exclusive and preferential access to the lecturer’s laboratory where, assisting the master in his investigations (sometimes even directly cooperating) captures the legacy of the Art this master transmits to him and once again we see - the laboratory hand is a chemist in the line of succession.



## In Portugal

More in Oporto than in Lisbon, and more in the "technical" institutes (industrial, agricultural, commercial) than the elite polytechnics, the 2<sup>nd</sup> half of the XIX century sees, in our opinion, an opening in the scientific and technical areas of teaching which allowed the laboratory hand to be viewed as much more than a mere performer of minor tasks in the practical classes.

In a certain way, it could have narrowed the differences between the national situation and the foreign concept; we remember that in 1892 Alfredo Bensaúde, refers very clearly to the role of the laboratory hand in the development of scientific teaching in his project for the reform of technological teaching –the embryo document of the previous reform which created the I.S.T. (The Technical Superior Institute), the first institution totally dedicated to preparing civil (not military) engineers in Portugal– when he relates what he very impressively observed in Germany:

"The laboratory preparer or assistant has a very important role (...) They are in great part responsible for explaining and orientating the practical exercises of the technological courses, tasks that require in most if not in all cases, an amount of knowledge as vast as that of the lecturer himself, because the explanation of an industrial process is often no more than the theory of the same process".<sup>26</sup>

Whether with greater or lesser incidence and efficiency, and certainly affected by the unreceptive mentalities of the time we even so consider that this "democratization" or opening, did in fact take place and did bear fruit. We are also of the opinion that the importance attributed to the laboratory hands and other posts that in the meantime started to appear within the *spectrum* of the laboratory, apart from the lecturers and professors did contribute very positively towards consolidating groups of chemists in Portugal, and perhaps even helped to develop "a school". The next step is to discover if and how this could have happened.

## References

<sup>1</sup> Isabel Cruz, "Preparadores de Química da Escola Politécnica (1837-1856)", *Atalaia/Intermundos*, 10 – 11 (2002): 177 – 96, on 178.

<sup>2</sup> Isabel Cruz, *Química, laboratórios de química e valorização do ensino prático: a Escola Politécnica, a cadeira de química e o seu laboratório (1837-1859)*, (Lisboa, CICTSUL: Comunicação privada, 2000).

<sup>3</sup> Isabel Cruz, "A Química na Academia Politécnica do Porto (1837 a 1868)", *Atalaia/Intermundos*, 8-9 (2001): 257 – 92, on 262-263.

<sup>4</sup> Cruz, "Preparadores de Química da Escola Politécnica (1837 – 1856)", 177.

- <sup>5</sup> Cruz, “Preparadores de Química da Escola Politécnica (1837 – 1856)”, 186-187.
- <sup>6</sup> Cruz, “A Química na Academia Politécnica do Porto (1837 a 1868)”, 262-264 and 291 (endnote 18).
- <sup>7</sup> Américo Pires de Lima, *Subsídios para a História do Ensino de Farmácia em Portugal*, (Porto: edição do autor, 1933), 11-12.
- <sup>8</sup> José Pedro Sousa Dias, *Inovação técnica e sociedade na farmácia da Lisboa setecentista*, (Lisboa: Faculdade de Farmácia, Universidade de Lisboa, 1991).
- <sup>9</sup> João Rui Couto da Rocha Pita, *A Farmácia na Universidade de Coimbra (1772 – 1836)*, (Coimbra: Faculdade de Farmácia, Universidade de Coimbra, 1995), 325-326.
- <sup>10</sup> António Augusto de Aguiar, *Estatutos do Laboratório de Química Prática*, (Lisboa: Tipografia de Castro Irmão, 1872), 6-7.
- <sup>11</sup> “Carl von Bonhorst. Necrologia”, *Revista de Química Pura e Aplicada*, 7-8 (1918): 243-44, on 243.
- <sup>12</sup> José Júlio Bettencourt Rodrigues, “Projecto sumário de regulamento dos trabalhos e serviços do Laboratório de Química Mineral da Escola Politécnica de Lisboa; posto em execução, como experiência e sob responsabilidade do respectivo director no ano lectivo de 1889 a 1890”, in *The Mineral Chemistry Laboratory of the Polytechnic School of Lisbon (1884 – 1894)*, eds. Ana Luísa Janeira, Maria Elisa Maia, Pilar Pereira (Lisboa: Livraria Escolar Editora, 1996), 151 – 70, on 165-166.
- <sup>13</sup> José Júlio Bettencourt Rodrigues, *Exposição ao Conselho da Escola Politécnica sobre o ensino e mais serviços da 6.ª cadeira*, (Lisboa: Tipografia Universal, 1885), 9-10.
- <sup>14</sup> António Herculano de Carvalho, “O Instituto Superior Técnico”, in *A Universidade Técnica de Lisboa e os seus mestres: notas biobibliográficas*, dir. Moses Amzalak (Lisboa: Universidade Técnica de Lisboa, 1956), 707 – 876, on 783.
- <sup>15</sup> Charles Lepierre, “Os grandes homens de ciência: Roberto Duarte Silva”, *Técnica*, 13 (1928).
- <sup>16</sup> José Júlio Bettencourt Rodrigues, *Exposição a propósito dos concursos ao lugar de preparador da cadeira de Tecnologia; lida ao conselho do Instituto Industrial e a ele endereçada*, (Lisboa: Tipografia Universal, 1884), 13 and 20.
- <sup>17</sup> Emílio Dias, “Índigo ou anil, a sua extracção em terrenos de África – António Augusto de Aguiar, Alexandre Bayer e Carlos von Bonhorst, no ensino da química prática”, *Revista de Química Pura e Aplicada*, 1 a 4 (1919): 43 – 61, on 49-52.
- <sup>18</sup> César Justino de Lima Alves, *Relatório apresentado à Direcção Geral de Agricultura, por César Justino da Lima Alves, sobre o seu tirocínio no laboratório de Química-Agrícola do Instituto Agronómico de Paris*, (Lisboa: Imprensa Nacional, 1892), 3.
- <sup>19</sup> Luís António Rebelo da Silva, “Duas palavras em resposta ao artigo “Os químicos em Portugal”, publicado pelo dr. H. Mastbaum na Revista Químico Farmacêutica do Porto”, *Revista Agronómica*, 12 (1904): 329 – 39, on 335.
- <sup>20</sup> Emílio Dias, *Aos Ex.<sup>mos</sup> Srs. Accionistas da Companhia Lisbonense de Iluminação a Gás*, (Lisboa: Tipografia Elzeviriana, 1885).
- <sup>21</sup> António Joaquim Ferreira da Silva, *Breve notícia sobre o ensino da química na Academia Politécnica do Porto e sobre os regulamentos do laboratório e dos trabalhos práticos dos alunos*, (Porto: Tipografia Ocidental, 1893), 15.
- <sup>22</sup> Emílio Dias, “Índigo ou anil, a sua extracção em terrenos de África”, 51.
- <sup>23</sup> Charles Lepierre, *Despedida (Ao público sensato de Coimbra)*, (Coimbra: Tip. França Amado, 1911), 5.
- <sup>24</sup> Luís António Rebelo da Silva, *Relatório da análise dos vinhos apresentados nas exposições de Berlim e de Paris em 1888 e 1889*, (Lisboa: Imprensa Nacional, 1890), 3.

<sup>25</sup> “Luís António Rebelo da Silva; Professor da cadeira de Química Agrícola” [Nota biobibliográfica], in *Anais do Instituto Superior de Agronomia*, ed. Instituto Superior de Agronomia (Lisboa: Instituto Superior de Agronomia, 1920), 28 – 30, on 28.

<sup>26</sup> Alfredo Bensaúde, *Projecto de reforma do ensino tecnológico para o Instituto Industrial e Comercial de Lisboa*, (Lisboa: Tipografia da Academia Real das Ciências, 1892), 14.

### **Additional bibliography**

- António Joaquim Ferreira da Silva, “Para a história da química; O Dr. R. Fresenius”, *Revista de Química Pura e Aplicada*, 12 (1911): 347-50.
- Camilo Castelo Branco, *O meu condiscípulo. Cavar em Ruínas*. (Lisboa: Livrarias de Campos Júnior – Editor, 1866).
- Francisco da Fonseca Benevides et al., *Projecto de reorganização do Instituto Industrial e Comercial de Lisboa, elaborado pelo Conselho Escolar do mesmo Instituto, em cumprimento da Portaria de 13 de Fevereiro de 1892*, (Lisboa: Tipografia Universal, 1892).
- Isabel Cruz, “O laboratório de Química do Instituto Industrial de Lisboa: conflitos entre ensino e produto da ciência aplicada (1852 – 1864)”, in *Divórcio entre cabeça e mãos? Laboratórios de Química em Portugal (1772 – 1955)*, eds. Ana Luísa Janeira; Maria Estela Guedes; Raquel Gonçalves, (Lisboa: Livraria Escolar Editora, 1998), 83-89.
- Isabel Cruz, “From the Industrial and Commercial Institute of Lisbon to the Technical Superior Institute – what transition? (1892 – 1922)”, in *5th International Conference on History of Chemistry – Chemistry, Technology and Society; Proceedings*, eds. Isabel Malaquias, Ernst Homburg, M. Elvira Callapez, (Lisboa, Sociedade Portuguesa de Química, 2006), 626 - 36.
- Joaquim de Santa Clara Sousa Pinto, *Oração fúnebre que na ocasião das Exéquias do Ill.<sup>mo</sup> Sr. José António de Aguiar, lente da Academia Politécnica da cidade do Porto, recitou na sala dos Actos, onde se colocou o seu retrato, o seu colega e amigo Joaquim de Santa Clara Sousa Pinto, lente da mesma Academia, no dia 13 de Março de 1850*, (Porto: Tipografia Comercial, 1850).
- José Pereira Salgado, “A Química na Academia Politécnica do Porto”, in *O Ensino na Academia Politécnica*. Primeiro Centenário da Fundação da Academia Politécnica e da Escola Médico-Cirúrgica, 1837 – 1937, (Porto: Universidade do Porto, 1937).
- Luís Albano de Andrade Morais Almeida, *À memória do Ill.<sup>mo</sup> Sr. José António de Aguiar, lente substituto das aulas da Academia Politécnica da cidade do Porto*, (Porto, 1850).
- Luís Porfírio da Mota Pegado, *Relatório sobre o Instituto Industrial e Comercial de Lisboa; Ano lectivo de 1888 – 1889*, (Lisboa: Imprensa Nacional, 1890).
- Pedro José da Cunha, *A Escola Politécnica de Lisboa; Breve notícia histórica*, (Lisboa: Faculdade de Ciências da Universidade de Lisboa. Primeiro Centenário da Fundação da Escola Politécnica, 1937).
- Virgílio Machado, *O ensino da química no Instituto Industrial e Comercial de Lisboa*, (Lisboa: Imprensa Nacional, 1892).

### **Legislation/Official documents**

- *Anuário da Academia Politécnica do Porto*; vários anos lectivos (Porto: Academia Politécnica do Porto, 1878 -1900).

- Concurso para o lugar de chefe de serviço químico na secção agrícola do Instituto Geral de Agricultura. “Diário do Governo”, N.º 104, de 8 de Maio de 1884.
- Nota de aviso sobre o único candidato admitido ao concurso para o provimento do lugar de chefe de serviço químico do Instituto Geral de Agricultura, Luís António Rebelo da Silva. “Diário do Governo”, N.º 160, de 18 de Julho de 1884.
- Nota de aviso de abertura, de 30 de Julho de 1889, de concurso público documental para selecção de dois alunos, habilitados com o curso de agronomia pelo Instituto de Agronomia e Veterinária de Lisboa para seguirem, por conta do estado e durante dois anos, as lições práticas e os exercícios de análise química nos laboratórios oficiais de França ou Alemanha. “Diário do Governo”, N.º 170, de 1 de Agosto de 1889.
- Fundo documental da Escola Politécnica – Museu de Ciência da Universidade de Lisboa. Livros de Actas das Sessões do Conselho Escolar, 1.º - 6.º.
- Arquivo da Casa da Moeda/Imprensa Nacional – Casa da Moeda: ACM/INC. Casa da Moeda. Laboratório de Química. Aula de Física e Química. Matrículas dos alunos – *Livro 1.º*, Ano de 1824 para 1825 – Parte Física.
- Arquivos da Torre do Tombo, ANTT. Ministério do Reino. 1.ª Direcção. 1.ª Repartição. Relatórios (1843 – 1894). Mç 3644.

### ***Diversa***

- “Almanaque comercial de Lisboa”, Anos: 1881 a 1890, 1892 e 1893.
- “Anuário-Almanaque comercial (ou Anuário oficial de Portugal)”, Anos: 1894 a 1899.
- “Jornal de Ciências Matemáticas, Físicas e Naturais”. Tom. I, Novembro de 1866 a Dezembro de 1867 ao Tom. VI, Fevereiro 1900 a Maio 1902.
- “Jornal de Farmácia e Ciências acessórias”, Tom. 1848 ao Tom. 1859.
- “Jornal da Sociedade Farmacêutica de Lisboa”. Tom. I, Julho 1835 a Julho de 1837.
- “Jornal da Sociedade Farmacêutica Lusitana”. Tom. 1838 ao Tom. 1848.
- José Lourenço Sousa, *Quinto almanaque comercial, fabril, administrativo, eclesiástico e militar do Porto e seu distrito, para 1860*, (Porto: Tipografia Popular de J. L. de Sousa, 1859).
- José Lourenço Sousa, *Décimo almanaque, comercial, fabril, judicial, administrativo, eclesiástico e militar do Porto e seu distrito para 1865 – 1866*, (Porto: Imprensa Popular de J. L. de Sousa, 1864).

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## Working in a Transitional Territory? Chemical Consultants in the United Kingdom, 1870-1914

*Anna Simmons\**

An example of the evolving identity of chemistry and the chemical profession can be found in the changing activities of chemical consultants in the United Kingdom, particularly in the period leading up to the First World War. To earn a living, consultants typically took on a number of roles including general analytical work, government appointments such as that of public analyst and industrial consultancy. This latter role involved performing process and product control, advising on chemical practice, and undertaking research, activities which have been highlighted by historians such as James Donnelly, David Edgerton, Sally Horrocks, Robin Mackie and Gerrylynn Roberts.<sup>1</sup> However, during the late nineteenth and early twentieth centuries, the territories occupied by chemical consultants changed as professional borders evolved. Initially the chemical consultants worked independently in their own laboratories providing expertise on various chemical questions for a range of customers. By the end of the period, the specific expertise required by government and industry was beginning to be provided by in-house employees in designated laboratories rather than by independent practitioners.

### Changes in the professional borders of chemistry

This review provides a quantitative examination of the changes in professional borders by using the Open University's "Biographical Database of the British Chemical Community, 1880-1970".<sup>2</sup> At the heart of this project is the "Chemists Database" which includes details of the lives of around 9,000 chemists, assembled from a wide-range of sources such as obituaries, membership records of the three major British chemical institutions, the Chemical Society, the [Royal] Institute of Chemistry and the Society for Chemical Industry, and standard biographical works. A part of this database, containing abbreviated records of around 4,900 individuals, can be viewed on the website: <<http://www.open.ac.uk/ou5/Arts/chemists/>>. The paper is divi-

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ded into two parts. The first half contains a statistical profile of consultants' careers and establishes the percentages of chemists working as independent consultants and presents data on their qualifications, geographical locations and type of employment, exploring how these factors changed over time. In the second half of the paper, having briefly examined the careers of some consultants, their activities will be placed in a wider context and the shifts in the professional boundaries within which they were working is explored.

Although a full understanding of the construction and composition of the "Chemists Database" is unnecessary for the purposes of this paper, a brief explanation of certain aspects is helpful for interpreting the data presented. To study the activities of chemical consultants in the United Kingdom, all chemists selected from the database are those who can be classified as working in the consulting sector. To assess changes occurring in their activities over time, these chemists are divided into two periods, 1877-1886 and 1887-1917. These date ranges are based on the date of entry of each individual to any of the three major British chemical institutions, the membership records of which form the foundation of the database. The start and end dates of the periods arise from changes in the membership criteria of one of the three societies surveyed, namely, the Institute of Chemistry.

Twenty-seven percent of chemists in the British chemical community<sup>3</sup> who joined one of the three societies in the period 1877-1886 were employed as consultants at some point in their careers, with twenty-two percent of the joiners from the second period working in this sector. However, even with this drop, over one fifth of all chemists who joined the three societies in the years leading up to the First World War worked at some point during their careers in the consulting sector. The individuals included in the 27% and 22% form the basis of the data explored in the remainder of this paper. By looking at which chemical institutions they belonged to, their qualifications, geographical locations and employment, it is possible to shed light on who those working as consultants were, and what they did during the course of their careers.

## Profiling Consultants

***Table 1. Membership of Chemical Societies***

<i>Institution</i>	<i>% of Society Membership working as consultants</i>	
	<b>1877-1886</b>	<b>1887-1917</b>
Chemical Society	43	31
Institute of Chemistry	57	39
Society of Chemical Industry	22	19

As the data in Table 1 shows, a significant percentage of the membership of all three chemical societies worked as consultants, although (in line with the overall decline in the numbers working in this sector) these percentages were larger for those joining in the first compared to the second period. That the percentages are highest for the Institute of Chemistry (IC) is not surprising. It was consulting and analytical chemists who were instrumental in the foundation of the Institute in 1877, at a time when professional boundaries within the subject were being delineated. The Institute and its remit thus reflected their professional concerns<sup>4</sup>. Membership of the Institute constituted a qualification and for those working in independent consulting practice this provided evidence of their professional skills to potential customers.

Data on the qualification levels attained by consultants is provided in Table 2. As a group, consultants were highly qualified, especially with regards to holding a higher degree – one third of those joining in the first period possessed a doctorate.

***Table 2: Qualifications of Consultants<sup>5</sup>***

	<i>All Bachelors</i>	<i>All Masters</i>	<i>All Doctorates</i>	<i>All IC</i>	<i>IC &amp; Degree</i>	<i>IC Only</i>
<b>CONSULTANTS (%)</b>						
1877-1886	20	7	33	53	20	33
1887-1917	36	11	28	52	29	23
<b>BRITISH CHEMICAL COMMUNITY (%)</b>						
1877-1866	27	5	25	39	16	23
1887-1917	44	12	19	39	24	14

Comparing consultants with the wider British Chemical Community, Table 2 shows that whilst members of the British Chemical Community were more likely to hold a Bachelors' degree, consultants were more likely to have obtained a doctorate. Again the importance of qualification through the Institute of Chemistry is clear: 53% & 52% of consultants joining in the first and second periods, compared to 39% (for both periods) of the wider community were members<sup>6</sup>. In fact, for joiners in the first period 33% of consultants compared to 23% of the wider community held only the Institute of Chemistry certification as a qualification. This is not unexpected. A key objective for the consulting and analytical chemists, who were instrumental in establishing the Institute of Chemistry, and subsequently creating its examinations system, was to demonstrate professional competency. Many practitioners who had developed their chemical and analytical skills



through a combination of apprenticeship, employment as an analytical assistant and unexamined studies at a range of institutions became members of the Institute for this reason.

**Table 3. Geographical Locations**

<i>Location</i>	<i>1877-1886 (%)</i>	<i>1887-1917 (%)</i>
London	27	22
South-East	8	7
South-West	6	3
W Midlands	5	4
E Midlands	2	2
East Anglia	1	1
Yorkshire	6	5
North-West	11	10
North	3	2
Wales	3	2
Scotland	6	5
N Ireland	1	-
Irish Republic	3	1
UK & Ireland General <sup>7</sup>	7	6
Overseas	9	26
Unknown	3	4

As table 3 illustrates,<sup>8</sup> consultants worked in a range of locations throughout the United Kingdom and also overseas during the course of their careers. London was however the dominant location for consultants within the UK. Of consultants joining one of the three societies in the period up to 1886, 27% spent part of their careers in London, with 8% based in the South East. Although it was founded as a national organisation, this partly reflects the Institute's metropolitan focus. However it also reflects the needs of the types of firms located in this area. These were often highly specialised businesses in a range of manufacturing industries such as dyestuffs, pharmaceuticals and metals. Crucially, the expertise provided by consultants was especially suited to these firms' requirements. Similar figures apply to those joining during the years 1887-1917, with 22% of consultants based in London and 7% in the South East. However consultants were not solely London based: for those joining in the years 1877-1886, 11% worked in the North West, again reflecting the concentration of industry there, while 9% spent part of their careers overseas. Many of these individuals worked in more than one region

during their working lives. The flexible nature of the independent practitioner's work facilitated holding multiple appointments and made it easier to take up employment in additional or new locations. In general, similar figures apply for those joining in the second period, with the exception of the high 26% working overseas. The flexibility of consulting work no doubt facilitated overseas employment in this sector, particularly in the Dominions and British colonies. However it is interesting to note that amongst the wider British chemical community overseas employment also peaked at 31% for those joining the three societies between 1887 and 1917.<sup>9</sup>

Consultants were not only geographically mobile; they were also occupationally versatile, working additionally in academia, industry, government and other sectors.<sup>10</sup>

***Table 4. Number of Employment Sectors<sup>11</sup>***

<i>Number of Employment Sectors</i>	<i>1877-1886 (%)</i>	<i>1887-1917 (%)</i>
1	16	21
2	46	49
3	31	25
4+	7	6

For the majority of consultants, their work in the consulting field was part of a multi-sector career, something which reflected the numerous applications and flexibility of chemical skills. 84% of those joining in 1877-1886 and 80% of those joining in 1887-1917 worked in at least two sectors, with 38% and 31% in three or more. Work in consulting was most commonly combined with employment in industry (22% and 31% respectively) or academia (16% and 8%), with 9% and 10% working in all three sectors during the two periods of joining. While the overall percentage of those working in the consulting sector decreased from 27% to 22% over the two periods, more consultants who joined in the years 1887-1917 worked only in this field (21% compared to 16%). This perhaps reflects the continued importance of the private consulting practice. For example, although he had worked in government and industry as part of his training, Horatio Ballantyne (1871-1956), after appearing as an expert witness in the Welsbach Incandescent Gas Light Co. patent infringement case, set up as a consulting chemist in London in 1896 and established a reputation as an authority on chemical processes patent law.<sup>12</sup>

However, it is the 22% rising to 31% of chemists that worked in both the consulting and industrial sectors whose careers are the most striking. Consultants'

increasing involvement with industry reflects the growing employment of chemists from the wider British chemical community in this sector. 64% of chemists joining the three societies in the period 1877 to 1886 worked in industry, a figure which is already greater than many might expect given the common perception that Britain's failure to apply science to industry was one cause of industrial decline. Furthermore the percentage working in industry rose to 72% for those joining in the period 1887-1917. Although not to the same extent as in industry, consultants were also finding more work within government laboratories. Those working in both the consulting and government sectors rose from 4.5% to 7% across the two periods of joining. In the wider British Chemical Community, government employment grew from 13% to 16% over the same time-scale.

### **Consultants' Careers**

From the data previously presented it is clear that the role of consultants within the chemical community in the years leading up to the First World War was changing as the specific expertise required by government and industry began to be provided by in-house employees rather than by independent practitioners. This will be examined further by looking briefly at the careers of some individual consultants, exploring their links with industry and placing their activities in the wider context of the developing provision of in-house chemical expertise within the firm.

Frequently, the model adopted in Germany and the USA in the late-nineteenth century, where large firms often built up teams of scientific and technical experts who carried out research and tested processes within the organisation, is presented as the norm for industrial activity.<sup>13</sup> However, as in other areas of firm development, British firms may have preferred to rely on external solutions and buy expertise in an open market. That at least one-fifth of chemists joining the three societies worked in the consulting sector in the period up to the First World War suggests that there was significant demand for such individuals. Consulting chemists provided a range of chemical services for firms which included performing process and product control, analysing samples, advising on chemical practice, testing new products and undertaking research.<sup>14</sup>

As David Edgerton and Sally Horrocks have highlighted "many firms employed consultants for both analytical testing and R & D of new products before establishing their own in-house facilities".<sup>15</sup> This applied to William Chattaway's work for the chocolate manufacturers Cadbury Brothers. He performed analyses and

visited Cadburys' works in a consultative capacity for four years up to 1902.<sup>16</sup> In the previous year, 1901, Nathaniel Parr Booth, who had previously worked as a junior analyst under Chattaway, was appointed as Cadbury's first chemist.<sup>17</sup> Chattaway's main employment was as Superintendent of the Drug Trade at Apothecaries' Hall, Blackfriars, London, from where he operated a consultancy practice. Providing expertise for Cadburys was just one aspect of this practice. In a designated laboratory containing equipment including a polarimeter, refractometer and microscope, in addition to basic glassware, balances and burners,<sup>18</sup> chemical analyses and research were performed for customers as diverse as the General Post Office, the Crown Agents for the Colonies and a toothpaste inventor called Mr A. G. England.<sup>19</sup> Chattaway also held government appointments which contributed to his consultancy practice – he was public analyst for both the London Borough of Hammersmith and the town of Colchester in Essex. Later on, the Hall analytical laboratory provided drug standardisation services for a range of pharmaceutical firms before they established in-house laboratories which were licensed for animal testing. From 1909, the drugs digitalis, squill, strophanthus, cannabis indica and ergot were physiologically standardised in the Hall analytical laboratory for firms which included Boots, British Drug Houses and Allen & Hanburys.<sup>20</sup>

The utilisation of outside expertise to provide process and product control, as well as advice on chemical practice, can be seen in a range of sectors within the chemical and related industries. As Sally Horrocks has highlighted in her PhD thesis, the biscuit manufacturers, Huntley and Palmers, provide another example of consulting activity being a precursor to employing in-house chemical expertise. Huntley & Palmers consulted various local chemists prior to creating a full-time post in 1907. For example, John Cuthbert Welch, while employed at the nearby brewery H & G Symonds in Reading, advised on the supply of manufacturing equipment to the biscuit manufacturers.<sup>21</sup> Other consulting chemists provided chemical services for firms through established laboratory practices. For example, Alexander Norman Tate owned an analytical and consulting practice in Liverpool, A Norman Tate & Co, which specialised in the analysis of oils, waxes and fats for W H Lever and other soap and food manufacturers.<sup>22</sup> As had occurred with Cadburys and Huntley & Palmers, the use of consultants here was again a precursor to the employment of in-house chemists. Although it requires more investigation than is possible in this paper, perhaps we can see this employment of chemical consultants as a transitional stage before in-house research laboratories with research chemists came to be widely adopted in Britain in the first half of the twentieth century.

## Shifting Boundaries

That this stage was transitional can be seen when we look at the prominence of consulting in the chemical career over a longer period. Although the overall proportion of chemists employed in consulting who joined the three societies in the periods 1877-1886 and 1887-1917 remained high, we have already seen that there was a decline in the percentage working in this sector between the first and second periods, from 27% to 22%. Taking entrants to the three institutions over the entire duration of the database, this decline accelerated sharply after the First World War.

**Table 5**  
***Percentage of Consultants in the British Chemical Community***

	1877-1886	1887-1917	1918-1943	1944-1956	1957-1970
% Consultants	27	22	14	11	10

As more chemists were employed within firms, the consultant's role moved away from supplying chemical skills from the outside to that of providing expertise from within. As the data in Table 5 shows, the percentage working in consulting declined sharply from 22% to 14% between the second and third periods. The function of the consulting role within the chemical career also changed. A new type of consultant emerged – the individual who turned to consultancy after retirement, perhaps early, from a long industrial or institutionally-based career.<sup>23</sup> This is illustrated by the career of John Frederick Briggs (1871-1963), Chief Chemist to the British Cellulose & Chemical Manufacturing Co from 1916, (British Celanese Ltd from 1925). He remained with this firm until his retirement, but continued with them as a consultant until shortly before his death in 1963.<sup>24</sup>

With around one-quarter of all chemists who joined the three chemical societies in the years leading up to the First World War working as consultants, the significance of consulting as part of the chemical career at this point must not be undervalued. Such work was particularly important with regard to the contributions it made to British industry. Although services such as performing process and product control, advising on chemical practice, and undertaking research took place outside of the firm and thus were often hidden from view, they should not be overlooked. Given the prominence of the consultant in the chemical career at this time, perhaps the period traditionally associated with British industrial

decline, can now be seen in a different light. Instead of concentrating on a perceived absence of the industrial research model adopted in Germany and the USA, we can see it as a stage where the consultant's role was prominent, with chemical expertise provided externally for UK firms by independent practitioners.

However the territory that consultants occupied at this point was only transitory. As the data in Table 5 illustrates, the percentage of chemists working in the consulting sector declined sharply amongst those joining the three societies after the First World War. Increasingly the specific expertise required in government and industry was beginning to be provided by in-house employees rather than by independent practitioners. The nature of consulting was also changing to a role pursued at the end of a career, rather than at its centre. These differences had roots both within the changing relationship of chemistry with other disciplines and the developments taking place within the chemical and allied industries, factors which provide plenty scope for further investigation. As chemistry's identity evolved the dynamic and versatile contributions made by consultants to the economy were transformed.

## Notes

<sup>1</sup> David Edgerton and Sally Horrocks, "British Industrial Research and Development before 1945," *Economic History Review* 47 (1994): 213-38; James Donnelly, "Structural Locations for Chemists in the British Alkali Industry, 1850-1910," in *The Chemical Industry in Europe, 1850-1914: Industrial Growth, Pollution and Professionalization*, eds. Ernst Homburg, Anthony Travis and Harm Schröter (Dordrecht: Kluwer, 1998), 203-19; Robin L. Mackie and Gerrylynn K. Roberts, "Career Patterns in the British Chemical Profession during the Twentieth Century," in *Origins of the Modern Career*, eds. David Mitch, John Brown and Marco van Leeuwen (Aldershot: Ashgate, 2004), 317-36.

<sup>2</sup> Robin L. Mackie and Gerrylynn K. Roberts, "Biographical Database of the British Chemical Community, 1880-1970," <http://www.open.ac.uk/ou5/Arts/chemists/>

<sup>3</sup> By virtue of the sources from which it is constructed, the complete database does contain individuals who spent their entire education and career overseas and had no link to the UK other than society membership. To eliminate such individuals, the chemists referred to in this paper are a group we have defined as "British" by virtue of factors such as employment or education in the UK. This group is referred to in this paper as the British Chemical Community. For an investigation of the entire database and its overseas constituents see G.K. Roberts and A.E. Simmons, "The Overseas Dimensions of British Chemical Societies, c.1890-c.1950," *Historia Scientiarum* 16 (3), March 2007, 224-43.

<sup>4</sup> Colin A. Russell, Noel G. Coley and Gerrylynn K. Roberts, *Chemists by Profession: The Origins and Rise of the Royal Institute of Chemistry* (Milton Keynes: Open University Press, 1977), 135-57.

<sup>5</sup> Data is given as a percentage of those for whom we have information on their education. "All" refers to all individuals in the categories of "Consultants" and "British Chemical Community" who held a bachelors, masters, doctorate etc.

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<sup>6</sup> The figures are different from table 1 because table 2 reports the percentage of the 27% and 22% of consultants who joined the Institute, whilst table 1 reports the percentage of the entire IC membership who worked as consultants.

<sup>7</sup> This figure represents the chemists who we know were working in the UK and Ireland but for whom we have no exact geographical location.

<sup>8</sup> The percentages in table 3 have been normalised to 100%.

<sup>9</sup> For those joining between 1877 and 1886 the figure is 13%.

<sup>10</sup> The “other” category includes a wide range of employment fields, such as medicine, military service, finance, the law and administration.

<sup>11</sup> Data is given as a percentage of those for whom we have information on their careers.

<sup>12</sup> *Journal of the Royal Institute of Chemistry* 80 (1956): 245-46; *Chemistry & Industry*, 6 Jan 1933, 5.

<sup>13</sup> A.D. Chandler, *Shaping the Industrial Century: The Remarkable Story of the Evolution of the Modern Chemical and Pharmaceutical Industries* (Cambridge, Mass.; London: Harvard University Press, 2005), 114-43.

<sup>14</sup> Mackie and Roberts, “Career Patterns.”

<sup>15</sup> Edgerton and Horrocks, “British Industrial Research,” 220.

<sup>16</sup> Apothecaries’ Hall Archive, Society of Apothecaries, London: Management Committee Minutes (hereafter MCM), 19 May 1902 and 25 November 1902.

<sup>17</sup> S.M. Horrocks, “Consuming Science: Science, Technology and Food in Britain, 1870-1939,” PhD Thesis, University of Manchester (1993): 107-8.

<sup>18</sup> Apothecaries’ Hall Archive, E/7 Loose Papers, Box 4, Inventory of Analytical Plant and Apparatus.

<sup>19</sup> Apothecaries’ Hall Archive, E/7 Loose Papers, Box 5, Papers re contract for manufacturing England’s patent toothpaste. For General Post Office: MCM 15, 22, 29 November 1904. There are numerous references to work performed for the Crown Agents, see A.E. Simmons, “The Chemical and Pharmaceutical Trading Activities of the Society of Apothecaries, 1822-1922,” PhD Thesis, The Open University (2004): 211-16.

<sup>20</sup> Simmons, “The Chemical and Pharmaceutical Trading Activities,” 242-46.

<sup>21</sup> Horrocks, “Consuming Science,” 87, 89; Entry for J.C. Welch in Mackie and Roberts, “Biographical Database.”

<sup>22</sup> *Journal of the Society of Chemical Industry* 9 (1892), 594; *Proceedings of the Institute of Chemistry* (1892), ii, 24-6; *DNB*.

<sup>23</sup> J.A. Radley, “Why Consultancy?” *Chemistry in Britain* 3 (1967): 250-52.

<sup>24</sup> *Journal of the Royal Institute of Chemistry*, November 1963, 400; *Chemistry & Industry*, 23 November 1963, 1869-70.



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## Chemical Careers in Postwar Britain: Centrifugal Discipline /Centripetal Profession?

*Robin Mackie; Gerrylynn K. Roberts\**

At the start of the twentieth century, chemistry was, arguably, widely perceived as the most fundamental science, but it was definitely perceived as the most useful science; chemists were the largest group of scientists in Britain, finding occupations right across the economy –in industry, government, academia and a range of other fields. They had been the first British professional group to be organised around a scientific discipline and chemists served as a model for other emerging British scientific and technical professions. After the Second World War, university provision for science in Britain increased rapidly and chemists continued to be the largest group of scientists, certainly on the criterion of being the largest number of graduates in any pure science subject, at least into the early 1970s. Chemists and chemical research were at the heart of a vigorous industry, which would also continue to expand into the 1970s. In the final decade of the twentieth century, however, there was a perception of ‘decline’. Using an anthropomorphic metaphor for the trajectory of the discipline of chemistry, David Knight described it as having become ‘middle-aged’ during the first half of the century.<sup>1</sup> With the reputation of chemistry both enhanced and tarnished by participation in two world wars, its intellectual status appeared to be changing as, on the one hand, physics came to be seen as the most fundamental science while, on the other, biological sciences became the exciting frontier.

Knight’s volume was one of a clutch of general histories of chemistry published in Britain at about this time; another was W.H. Brock’s now standard volume.<sup>2</sup> Not only did the more or less simultaneous appearance of these works signify a maturing of the sub-discipline of the history of chemistry, they also signified the historical importance of the discipline of chemistry itself at a time, certainly in the UK, of, as we now know, peak enrolments,<sup>3</sup> coupled with financial stringency in academia, external pressures on the academy towards mission-oriented research arising from concern about an alleged lack of engagement between industry, academia and government, and talk –which would become reality early in the

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twenty-first century— of departmental closures and amalgamations. On the continent, in their history of chemistry also published in the early 1990s, Bernadette Bensaude-Vincent and Isabelle Stengers argued that there had been a dilution of disciplinary identity with an intellectual shift from ‘chemistry’ to a range of ‘chemistries’ with adjectival prefixes, perhaps even a return to the handmaid status from which the discipline had emerged two centuries previously.<sup>4</sup> The image of a centrifugal discipline captures this perception –a perception which analyses the discipline primarily in terms of research activity. Our approach, however, is to look at the discipline not in terms of research alone, but as a profession, from the perspectives of education and employment. That is, we examine who the chemists were.<sup>5</sup> Looking at the education and careers of chemists who joined the profession between 1944 and 1971, we can see on the one hand precisely the centrifugal tendency of the emergence of ‘adjectival chemistries’, but on the other hand we can also see a simultaneous centripetal pull of a still potent core identity for chemistry –on the educational side in terms of knowledge and skills and on the employment side in terms of a professional orientation defined in relation to that knowledge and those skills.

Consequent on government policy, the post-Second-World-War period was one of rapid growth in undergraduate numbers in Britain. From under 400 per year during most of the 1930s, the annual number of chemistry degrees doubled from 1948 to 1956 and tripled by 1966. 1969, when 2679 BScs in chemistry were awarded, was the peak year for undergraduate degrees in the period 1944–1971 on which we shall focus here. Although, the number of degrees awarded in other sciences also increased, what is striking in comparative terms is the continuing prominence of chemistry. Over the entire period to 1971, chemistry was the largest pure science degree cohort in Britain. It accounted consistently for roughly a quarter of all honours degrees awarded in pure science. By the early 1960s, some 55% of all those earning bachelors degrees in chemistry in Britain also went on to gain a PhD.<sup>6</sup>

What was officially counted under the heading of ‘chemistry’ in 1971 by the University Grants Committee, the administrative body for higher education at the time, included degrees in nine sub-disciplines: applied chemistry, colour chemistry, industrial chemistry, inorganic chemistry, organic chemistry, physical chemistry, polymer chemistry, technical chemistry, and textile chemistry. Chemical technology, agricultural, biological, clinical and physiological chemistry were counted under other subject headings, while biochemistry and chemical engineering were separate subjects.<sup>7</sup> Thus, the UGC considered as ‘chemistry’, not only the three major branches, but also a range of applied ‘chemistries’. Under

the UGC's definition, what we might call the 'biological' chemistries were, by this time, seen as being distinct from chemistry. An even more fragmented image emerged from the 1971 Census of Population of Great Britain. It grouped under the subject heading 'chemistry' some 56 different 'disciplines of first qualification' listed by members of the population at large.<sup>8</sup> Whereas the UGC's list demonstrated an academic, administrative understanding of the shape of the field for a particular year, the census figures reflected individuals' perceptions of what the subject of their education had been. The UGC figures were a snapshot for a particular year, whereas the census reported the qualifications of individuals aged from 18 to 70; that is, over a long period. Despite individuals' declaration of such diversity, the Stone Report on the Future of University Chemistry, commissioned by the UGC in the 1980s, recorded that the vast majority of graduates still took their degrees in the three classic branches of inorganic, organic and physical chemistry.<sup>9</sup>

Calling chemistry a 'central discipline' both, in its interactions with its neighbours, physics and the biological sciences more generally, and in its economic role, the Stone Report was fundamentally concerned with what we might term a 'pedagogic discipline' of chemistry –how this resource-intensive subject should best be organized nationally for teaching. 'The community, the chemical industry, and nearly all other industries need graduates who know the basis of their subject, who can find their way quickly through the maze of known facts, who know what methods might be used to solve new problems, and, above all, who are prepared to go on learning throughout their working lives'.<sup>10</sup> Therefore, the report concluded, university chemical education should consist of a broad-based study of the principles of chemistry, with ancillary physics. With such foundations, any necessary specific training could be readily gained on the job –a much better route, it argued, than vocationally oriented higher education. Elsewhere, we have called this model a 'liberal education' in chemistry. This argument on the basis of 'pedagogical efficiency' was remarkably congruent with the principles laid down in Britain in the 1840s, when the discipline was articulated there in institutional terms.<sup>11</sup>

A great deal is thus known about the supply of qualified chemists available to the labour market, and the picture is by no means as negative as has at times been suggested. However, it is much harder to find data about chemists' behaviour in the labour market. The evidence used tends to be either university enquiries into the first destinations of their graduates, or one-off surveys in particular years –both of which focus on individuals at particular points in time. Our methodology is to study collectively the whole careers of successive cohorts of individuals who

defined themselves as chemists by joining one or more of the three principal British chemical organizations over the period 1880-1971: the [Royal] Institute of Chemistry<sup>12</sup> (f. 1877), the Chemical Society<sup>13</sup> (f. 1841) and the Society of Chemical Industry<sup>14</sup> (f. 1881). In this paper, we offer some preliminary reflections based on our analysis of those who joined in the post-war period from 1944-1971.<sup>15</sup> Over that period the total membership of the three societies, adjusted for overlap, increased from roughly 14,700 to 36,700. Allowing for deaths, resignations and lapsed members, this increase of 22,000 in annual membership means in fact that some 37,000 individuals joined one or more of the societies over the period. That amounts to an average of about 1050 chemists joining each year during 1944-1956, increasing to 1500 per year during 1957-1971.<sup>16</sup> These are the chemists whom we profile in this paper; we have information on the careers of about 90% of them.<sup>17</sup>

### Employment Sectors (%)

	1944-1956 N = 13354 n = 647		1957-1971 N = 20729 n = 711	
	<i>All</i>	<i>Sector</i>	<i>All</i>	<i>Sector</i>
<b>Academia: Total</b>	<b>38</b>	<b>100</b>	<b>38</b>	<b>100</b>
Only	11	30	13	35
& Industry	15	38	12	33
& Government	7	19	7	17
All 3	5	13	6	15
<b>Government: Total</b>	<b>33</b>	<b>100</b>	<b>25</b>	<b>100</b>
Only	8	24	4	18
& Industry	13	39	9	34
& Academia	7	22	7	26
All 3	5	16	6	22
<b>Industry: Total</b>	<b>71</b>	<b>100</b>	<b>64</b>	<b>100</b>
Only	38	54	37	58
& Government	13	18	9	14
& Academia	15	21	12	19
All 3	5	7	6	9

Note: N is the calculated number on whom we have employment information; n is the number in the sample on whom we have employment information.

A particularly striking feature of chemists' employment, which is revealed by a whole-career rather than snapshot approach, is the extent to which they worked across sectors of the economy. The table, 'Employment Sectors (%)', shows the principal employment sectors in which our two post-war cohorts worked, as a percentage of the cohorts as a whole (columns headed 'All') and also as a percentage of those employed in each sector (columns headed 'Sector').<sup>18</sup> The pattern is fairly consistent between the cohorts. Multi-sector working explains why the 'Totals' rows in the columns headed 'All' in the table add up to more than 100%. In both periods, just over 20% worked in 3 or more sectors during the course of their careers, while the percentage working in two different sectors of the economy fell slightly from 34% to 27%.

The columns headed 'Sector' show that some 65-70% of academics, a category in which we have here included school teachers, spent at least part of their careers working in another sector. This was also the case for 75 to 80% of those employed in government. Even among chemists in industry, the majority employment sector throughout our work, more than 40% worked elsewhere at some point in their career. Of course, the length of posts in particular sectors varied enormously. Those in the overlap segments will include many who spent just a few years working in one sector before building their career in another. However, the whole-career behaviour of chemists thus indicates that inter-sector boundaries were not as rigid as sometimes suggested.

Equally important, but not illustrated in our table, are movements within each sector, across intra-sector boundaries. For example, within academia, there was movement between colleges and universities. Within the industry category, chemists moved between working in the chemical industry or in large firms, to working in other industries and/or in smaller firms. For example, about two-thirds of the firms in which chemists from both cohorts worked were in the chemical industry and roughly 60% in other industries. Even within the chemical industry, a very high proportion of chemists moved among branches –our industrial chemists worked in some 22 classes of the Standard Industrial Classification [1981].<sup>19</sup> They also moved between firms within and among the classes. Chemists in the 1944-1956 cohort worked in some 700 separate firms in the chemical industry. We suggest therefore that it is essential to move away from seeing chemists' employment in discrete boxes to recognising the extent and significance of occupational mobility, transferring knowledge and expertise from one workplace to another.

Geographical mobility was also characteristic of chemists' careers. Across the two cohorts, roughly 40% worked abroad for some part of their careers.<sup>20</sup> This figure

includes chemists born elsewhere who studied or worked in Britain or countries of its Empire, as all the chemical societies had large overseas memberships. Only the Royal Institute of Chemistry (RIC) required British nationality of its members in our period. 17% of the 1944-1956 cohort and 22% of the 1957-1971 cohort of chemists in our RIC samples who were born in the UK worked abroad. Adding in the British nationals born in the Dominions or wider Empire gives a figure of about 30% working overseas for both RIC samples.<sup>21</sup> Almost 60% of chemists moved between regions in the UK, while roughly 30% moved more than once.<sup>22</sup> Close to a third of each of the cohorts worked in Greater London and approximately a further quarter worked in counties of the southeast. The only other regional figures larger than 10% are 21% falling to 16% in the northwest and 11% of the first cohort in the West Midlands and in Scotland, falling to 6% and 8% respectively.<sup>23</sup> To some extent, this is of course a reflection of the metropolitan focus of the societies from which we have drawn our samples, despite all three being national, indeed international, bodies. 18% of the first cohort and 26% of the second both moved geographically and changed sector.

This degree of occupational and geographical mobility is easier to describe than to explain. We are still working on the analysis, which is likely to be economic and as much to do with circumstances within the various sectors in which chemists worked as with the nature of their chemical activities. For example, the geographical pattern suggests that chemists were affected by shifts over time in the location of industry in Britain.<sup>24</sup> Furthermore, we noted for chemists from our previous cohort, 1918-1943, the reflection in their careers of a shift in the wider economy to more structured, hierarchical career ladders, which entailed mobility as individuals sought to move up them. For the 1957-1971 cohort, there has been a further shift to portfolio careers, which entail mobility for different reasons.<sup>25</sup> It should also be noted that, when we look at the careers of individuals, they were highly particular and varied, despite each being allocable to our categories. Individual agency remained important.

Given the degree of mobility which we have shown, what is of most interest to us for purposes of this paper is that chemists were in fact able to move in this manner. Despite the apparent centrifugal tendencies suggested by the multiplication of specialities, and the huge array of specialist fields in which they worked, there was a countervailing centripetal tendency as well. Chemists had and were seen to have –by virtue of their liberal education in chemistry– core transferable knowledge and skills which allowed them to move from sector to sector, as well as within a sector. These were the core values of the liberal independent practitioner.<sup>26</sup> Their professional identity was that of chemists, rather than ‘adjectival chemists’.

Their experience of work, especially for those who moved between sectors must have been such that they could adapt readily, giving them a certain professional elasticity that employers must have recognized and valued. We would argue that, certainly chemists produced in the period up to 1971, some of whose careers are still ongoing, thus maintained a distinct professional disciplinary identity. Interestingly, this identity has been reinforced recently from the perspective of pedagogy. Following a period of disciplinary gloom as undergraduate chemistry enrolments fell and prominent British chemistry departments were closed down in the early years of the twenty-first century, chemistry has been described recently as ‘experiencing a renaissance’ in Britain, with increased government funding and the undergraduate intake once more on the increase.<sup>27</sup> Prestigious new teaching laboratories have been opened on the basis of the pedagogical efficiency of teaching core chemical skills and knowledge, essential to a wide range of research and employment.<sup>28</sup> Arguably, the pedagogical and the professional are jointly centripetal in effect.

## Notes

<sup>1</sup> David M. Knight, *Ideas in Chemistry* (London: Athlone, 1992), 179.

<sup>2</sup> William H. Brock, *The Fontana History of Chemistry* (London: Fontana Press, 1992).

<sup>3</sup> See Peter Morris, “Chemistry in the 21st Century: Death or Transformation?”, elsewhere in this volume.

<sup>4</sup> Bernadette Bensaude Vincent and Isabelle Stengers, *Histoire de la Chimie* (Paris: Éditions la Découverte, 1993); trans. by D. van Dam, (Cambridge, MA: Harvard University Press, 1997).

<sup>5</sup> See Gabor Pallo, “Boundaries of Chemistry: Interest and Identity in Early Twentieth Century”, elsewhere in this volume.

<sup>6</sup> Great Britain, University Grants Committee, [various series titles] (London: HMSO, 1944-1962); latterly, *First Destination of University Graduates* [1962-63 to 1970-71] (London: HMSO, 1954 to 1972).

<sup>7</sup> Great Britain, University Grants Committee, *First Destination of University Graduates*, 1970-71.

<sup>8</sup> Office of Population, Census and Surveys, *Persons with Qualifications in Engineering, Technology and Science*, Studies in Technological Manpower, No. 5 (London: HMSO, 1976), 15. The equivalent survey for 1961 included 16 sub-disciplines in the chemistry count; General Register and General Register Office, Scotland, *Census 1961, Great Britain: Scientific and Technological Qualifications* (London: HMSO, 1962), xv.

<sup>9</sup> University Grants Committee, “The Future of University Chemistry: Report of the Chemistry Review” [The Stone Report], 1987, typescript of final draft copy, kindly provided by the then Head of the Department of Chemistry, The Open University. The review committee included 5 academics and a Deputy Chairman of ICI Pharmaceuticals.

<sup>10</sup> The Stone Report, 8.



<sup>11</sup> On the development of the 'liberal education' model of chemical pedagogy in Britain from the 1840s, see Robert F. Bud and Gerrylynn K. Roberts, *Science vs Practice: Chemistry in Victorian Britain* (Manchester: Manchester University Press, 1984).

<sup>12</sup> Colin A. Russell, Noel G. Coley and Gerrylynn K. Roberts, *Chemists by Profession. The Origins and Rise of the Royal Institute of Chemistry* (Milton Keynes: The Open University Press, 1977). It received the designation 'Royal' in 1944.

<sup>13</sup> R. L. Mackie, "Chemical Societies and the Demarcation of the British Chemical Community, 1870-1914," in *Creating Networks in Chemistry. The Founding and Early History of Chemical Societies in Europe*, ed. Anita Kildebaek Nielson and Sona Strbanova (Cambridge: Royal Society of Chemistry, 2008), 140-61. 1971 is the terminal date for this study because there was a major institutional change on 1 January 1972, when the Royal Institute of Chemistry and the Chemical Society committed to amalgamation, with a view to subsequent unification.

<sup>14</sup> J. F. Donnelly, "Defining the Industrial Chemist in the United Kingdom, 1850-1921," *Journal of Social History*, 29 (1996): 779-96; Robin Mackie and Gerrylynn K. Roberts, "Un secteur à part? Les chimistes industriels et la Society of Chemical Industry dans le contexte de la communauté chimique britannique," in *Chimie et Industrie en Europe. L'apport des sociétés savantes industrielles du XIXe siècle à nos jours*, ed. U. Fell (Paris: Editions des Archives Contemporaines, 2001), 127-47.

<sup>15</sup> This paper forms part of Robin Mackie's and Gerrylynn K. Roberts's project, 'Studies of the British Chemical Community, 1881-1972: The Three Principal Institutions', which was initiated with funding from the Leverhulme Trust. At the core of the project is the 'Chemists' Database' constructed at the Open University. This includes all Council members (c. 1900) plus systematic samples with a random start of members at large of the three societies drawn for five periods in their history determined by changes in Institute of Chemistry policies, totalling some 4,200 individuals. Biographical information was assembled by record linkage from a wide range of sources. Our data track individuals over the whole of their careers wherever possible. All data presented in this paper, which draws on two cohorts of members at large—those joining in the periods 1944-1956 and 1957-1971—are calculated from the 'Chemists' Database'. We have, however, eliminated those individuals whose connection with Britain was tenuous as they neither worked nor studied in the UK or its Empire. Our 'Biographical Database of the British Chemical Community', contains a full list of sources used and abbreviated records of, currently, c. 4,850 individuals; see <http://www5.open.ac.uk/ou5/Arts/chemists>.

<sup>16</sup> Not all graduates joined any of our societies. Preliminary calculations suggest that just over 50% of graduates in the period up to 1956 joined one or more of them—55% of male graduates and 30% of female graduates. Many graduates became school teachers—13% of all graduates, but 21% of female graduates—and are under-represented in the societies. Others not joining, who are harder to trace by our methods, may have left chemistry altogether, which seems to have been the case especially for women. Although some 9% of graduates over the period were women, they were not admitted to membership of the societies until late in the First World War. They comprised 4.8% of those for whom we have career information joining in the period 1944-1956 and 3.3% of those joining in the period 1957-1971. Despite these known 'missing' categories, we are aware that this still leaves a considerable percentage of graduates that did not join the societies at all. Nor did everyone who joined the societies have a degree. About 5% of those joining the societies across our period had neither a degree nor a professional qualification of the Royal Institute of Chemistry. Roughly 10% of joiners in our earlier period had no university degree but held an RIC qualification, defined as honours-degree equivalent at the time. In order to keep membership

increasing and to better reflect the reality of industrial employment, the Institute added a new grade of professional membership in our final period, admitting individuals who did not have an honours degree level qualification. This resulted in some 30% of joiners of all the societies in our final period with a professional qualification, which, depending on grade, was not necessarily honours-degree equivalent. A fairly consistent 45% held higher degrees. The figure for those who had both a degree and a professional qualification fell from 60% to 42%.

<sup>17</sup> It should be noted that our data on the 1957-1971 cohort is less complete. The average age of joining the societies was 25, so many members of this cohort are only now reaching the end of their careers. Furthermore, by definition, obituary material, on which we rely heavily for information, is less available for this cohort. We are none the less reasonably confident that we have captured accurately the experience of a significant portion of this group.

<sup>18</sup> Our full data contains two further, numerically less important categories of chemists: independent consultants who were an important group in the early twentieth century, but of declining numerical importance (only 10% of chemists after the Second World War) and a small miscellaneous group of 'Others', whom we are ignoring for now. On independent consultants, see Anna E. Simmons, "Working in a Transitional Territory? Chemical Consultants in The United Kingdom, 1870-1914", elsewhere in this volume.

<sup>19</sup> Great Britain, Central Statistical Office, *Standard Industrial Classification* (London: HMSO, 1981).

<sup>20</sup> Gerrylynn K. Roberts and Anna E. Simmons, "The Overseas Dimensions of British Chemical Societies, 1890-1950," *Historia Scientiarum*, 16 (2007): 224-43.

<sup>21</sup> Gerrylynn K. Roberts and Anna E. Simmons, "British chemists abroad, 1887-1971: the dynamics of chemists' careers," *Annals of Science*, forthcoming.

<sup>22</sup> We have designated Northern Ireland, Scotland and Wales as separate regions and divided England into 10 regions, with Greater London being one of them.

<sup>23</sup> On Scotland for an earlier period, see R. L. Mackie, "Counting Chemists: The Distribution of Chemical Expertise in Scotland in the First Half of the Twentieth Century," *Journal of Scottish Historical Studies*, 27 (2007): 48-74.

<sup>24</sup> See for example, R. J. Buswell and E. W. Lewis, "The Geographical Distribution of Industrial Research Activity in the United Kingdom," *Regional Studies*, 4 (1970): 297-306.

<sup>25</sup> Robin Mackie and Gerrylynn K. Roberts, "Career Patterns in the British Chemical Profession during the Twentieth Century," in *Origins of the Modern Career*, ed. D. Mitch, J. Brown and M.H.D. van Leeuwen (Aldershot, England: Ashgate, 2004), 317-36.

<sup>26</sup> Robin Mackie and Gerrylynn K. Roberts, "Career Patterns".

<sup>27</sup> Royal Society of Chemistry, "Digest of Statistics of Chemistry Education, 2007," <http://www.rsc.org/Education/Statistics>, accessed 28 April 2008.

<sup>28</sup> Steve McCormack, "Chemistry returns to its element in universities," *The Independent*, 25 January 2007; online edition (<http://education.independent.co.uk/higher/article2181876.ece>), accessed 1 February 2007.

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## Memory and History: The Mexican Community of Chemists Tells Its Story

*José A. Chamizo,\* Andoni Garritz,\*  
Mina Kleiche Dray\*\**

A seminar with the name of the title of this proceeding has been organised by the National University of Mexico, the French Institut de Recherche pour le Développement and the Mexican Chemistry Society. The main challenge of the seminar is to give the floor to the protagonists and witnesses of relevant facts in the building of chemistry history in Mexico during the XX century. The first session began on March 12<sup>th</sup> 2007. The main objective of the seminar was to understand and explain how scientific knowledge is constructed, to elicit its role in the progress of societies, giving elements to establish science policies.

The research in History and Sociology of Science has clearly shown that science is a core criterion for the economical and social development of communities and countries (Kuhn 1983; Ben-David 1997; Bourdieu 2001; Shinn y Ragouet 2005).

However, the history of science in Mexico has been widely studied in relation to centuries XVI to XIX (Trabulse 1983), but only few books have been written about the twentieth century (Garritz 1991). The organizers of the seminar think that this history deserves to be documented and known by the general public. The testimonies should help to characterise the Mexican chemistry practice inside the context of international science evolution and to identify its singularities.

Thanks to the preliminary historical “*periodisation*” investigation, a full set of six periods and topics was identified (Casas, R. and Kleiche-Dray, 2006) and proposed to the members of the Mexican Chemical Society with 50 or more years old.

1. The National School of Chemical Sciences (1916-1935).
2. The petroleum expropriation (1936-1940).
3. SYNTEX and the Institute of Chemistry of UNAM (1941-1975).

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4. Chemists as academics (1941-1970).
5. Chemical Engineering and chemical industry in México (1941-1970).
6. The strengthening and diversification of chemistry industry and education in Mexico in the seventies.

We received answers from 25 members who attended the first session of the Seminar, opened on March 12<sup>th</sup> 2007 (as is presented in figure 1).



**Figure 1.** Mina Kleiche Dray (IRD) is introducing the Seminar during the first session. She is accompanied (from right to left) by Alejandro Villalobos, from Pemex-Petrochemistry, José Luis Mateos (Mexican Chemistry Society) and Andoni Garritz (Faculty of Chemistry, UNAM).

After some discussion, an agreement was achieved to the following eight periods and topics:

1. The National School of Chemical Sciences (1916-1935).
2. The petroleum expropriation (1936-1940).
3. Syntex and the pharmaceutical industry (1943-1975).
4. Institute of Chemistry at UNAM (1941-1965).
5. The Faculty of Chemistry at UNAM (1965-1980). The case of the “coopérant français” and the teaching of Analytical Chemistry.

6. Chemists as academics. The formation of groups of basic research (1941-1980).
7. Chemical Engineering and chemical industry in México (1941-1970).
8. The strengthening and diversification of chemistry industry and education in Mexico in the seventies.

It was proposed and accepted to separate the Institute of Chemistry history and the case of Syntex as a model industry with the participation of research as main ingredient. The fifth topic was also considered important due to the influence of France on the development of the Analytical Chemistry area at the National University of Mexico, substituting the Analysis curriculum, and also by the participation of IRD in this research.

After this decision on the topics that will be covered, the starting session developed on topic number 7, and particularly on 'Petróleos Mexicanos, its petrochemical portion' —the public-sector establishment founded in 1940, just after the petroleum expropriation, and still the most important industry in Mexico. Alejandro Villalobos was our witness and the title of his presentation was: "Remembrances on the development of Mexican Petrochemical industry and a proposal to keep on its headway". He focused on the reasons why petrochemistry has declined, as can be seen in the figure 2 of this proceeding.

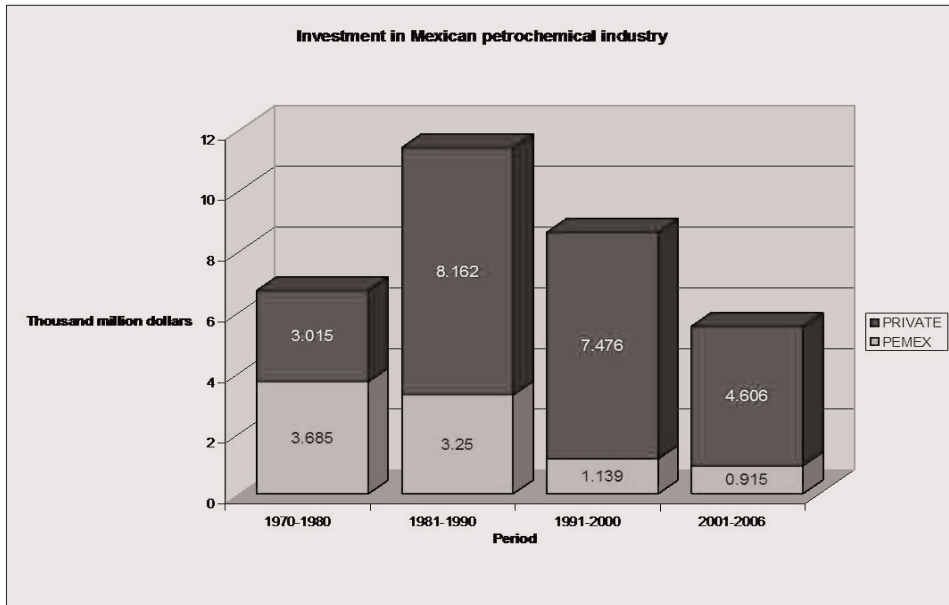


Figure 2. Investment in Mexican Petrochemical industry in periods (Taken from the presentation of Alejandro Villalobos).

Villalobos said that in spite of the world use of hydrocarbons as source to elaborate chemicals since the first decades of last century, the petrochemical industry in Mexico started in 1947 when Guanomex extended its action to the synthetic fertilizer field. In that year ammonia was produced from natural gas by the first time in our country, in an installation in Cuautitlán. Is in 1958 when the Law concerning the exploitation of natural resources was revised, establishing the direct participation and control of petrochemical industry by the Mexican State. Supported on this normative scheme, Petroleos Mexicanos (PEMEX) started from 1959 to 1994 an ambitious programme of development. In about thirty years 7 Complexes and 3 Petrochemical Units composed by more than 60 plants were constructed. Villalobos resumed the importance of Mexican petrochemical industry, including its diagnosis and positioning by product chains. He also made a proposal to continue with its development, limited by diverse structural problems, of prices and raw materials availability, complicated with a short-sighted vision of the governmental authorities.

The subject of the second and third sessions was also related to the seventh topic in the mentioned list, in the second with Enrique Bazúa, talking on 'Evolution of fertilizer industry in Mexico, pillar of industrialization and its present crisis' and Nicolás Rodríguez on 'My work as a Chemical Engineer in PEMEX. The opportunity, the experience and some reflections'.

Bazúa mentioned that in a first stage governmental and private fertilizer industries exist side by side until the creation of FERTIMEX, a unique state company. This booming industry supply Mexican countryside with the necessary items to improve agricultural yield, although phosphoric rock was still imported. Unfortunately, the economical crisis of the eighties brought a new model of development, by which the fertilizers' groups manufactured fewer products and become importers.

In the third session participated engineer Eduardo Rojo with the title 'Industrial promotion of the National Bank of Mexico (Banamex, the petrochemical permission given to Quimica General S. A. and Univex)', and Joaquín Palacios, under the title of 'Engineering plastics'.

Our plan is to start topic number 3 in the aforementioned list, with the participation of the son, José, and daughter, Carmen, of Francisco Giral, an outstanding pharmonochemist, in a forth session (on June), followed by Maricela Plascencia and Rafael Gual, witnesses of the Pharmaceutical industry, in a fifth session (on September). From this point we will dedicate to the emergence and development of Syntex, as well as other industries, as Sosa Texcoco, in the forties (see figure 3).

The case of Syntex is remarkable because the tuberoso barbasco plant, was found in the Mexican State of Veracruz, with lots of the steroid called "diosgenina"





**Figure 3.** The “snail”. An enormous solar evaporator (its diameter is 3.4 km) constructed for the Texcoco-valley-subterranean-waters’ concentration of salts, in the industry “Sosa Texcoco”, founded in 1943.



**Figure 4.** Inauguration of the ‘Escuela Nacional de Química Industrial’ on September 23th, 1916. To the right of the flag Juan Salvador Agraz, first Director.

the transformation of the Analytical Chemistry area in Mexican Universities, several researchers will be invited to cover the periods without survivors, as was the creation of the National School of Chemical Industry (see figures 4 and 5).

which Russell Marker, an American researcher, knew how to transform into “progesterone”, the pregnancy hormone. This fact opened the possibility of industrially obtaining important steroids products, as was the case in Mexico in the decade of the forties last century, with the creation of the initially Mexican industry, Syntex, and the interaction of the researchers of this corporation and others from the whole world with university researchers at the recently created Instituto de Química of UNAM.

After a session (in November) giving the floor to the French and Mexican witness of the participation of cooperants in



**Figure 5.** The photo of three nice female students in the gardens of the School of Chemistry (Asociación de Egresados de la Facultad de Química).



The Seminar will be open to the participation of external (national and international) sites connected by the videoconference technology from January 2008, when the Mexican educative institutions will be analysed one by one.

Besides collecting the written presentations from each participant in order to build up written archives and publish them, the sessions are also being filmed to share these priceless testimonies with distant and future audiences. A sample of the podcasts can be found in the following URL, <http://podcast.unam.mx/?cat=4>.

## References

- Ben-David, J. *Elementos d'une sociologie historique des sciences*. (France: Puf, 1997).
- Bourdieu, P. *Science de la science et réflexivité*. (Paris : Raisons d'agir, 2001).
- Casas, R. y Kleiche-Dray, M. "La construcción de la comunidad de químicos en México. Perspectivas sociológica e histórica", *Congreso Iberoamericano de Ciencia, Tecnología, Sociedad e Innovación, CTS + I*, México D. F. 19 al 23 de junio de 2006.
- Garritz, A. (comp.). *Química en México. Ayer, hoy y mañana*. (México: Universidad Nacional Autónoma de México, 1991).
- Kuhn, T. S. *La structure des révolutions scientifiques*. (France: Flammarion, 1983).
- Shinn, T. y Ragouet, R. P. *Controverses sur la science*. (Paris: Raisons d'agir, 2005).
- Syntex S.A. *Una corporación y una molécula. Historia de la investigación en Syntex*. (México: Syntex, 1967).
- Trabulsee, E. *Historia de la ciencia en México*. (México: Conacyt/Fondo de Cultura Económica, 1983).

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## Getting to the Heart of the Matter: The Changing Concepts and Names of Western Chemical Elements in Late Qing Dynasty China

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When modern Western chemistry was introduced into nineteenth-century China, chemical elements were then defined as ‘the elementary substances which composed all matters in nature’. For the Chinese, however, significant knowledge already existed regarding chemistry and its elements –including their properties, their preparations, and their compounds. For some time the Chinese had taken the view that an understanding of the elements was a natural first step in the study of chemistry. Moreover, given that the role of the elements in chemistry was so important to the Chinese, their discovery prior to the introduction of Western concepts of matter, helps to illustrate the advances that chemistry had undergone in China from earlier times. Taken together, this historical background is helpful when seeking to distinguish between the development of chemistry in China and in the West. More specifically, it places in better context the assertion that the differences in chemical terms and descriptions between China and the West can be used to explain why Chinese alchemy could not be easily transformed into chemistry.

Even though the earliest translators of chemical textbooks realised the importance of introducing the elements into Chinese chemistry, translation difficulties were not easily solved. This was partly due to the fact that most of the names of elements which were discovered in the West during the mid-nineteenth century had not been translated into Chinese. One of the first challenges to the translator, therefore, was to give the elements a Chinese term. However, no matter what method of expression was used, the names of these new elements were, at first, strange. In fact, many elements were given four or five different Chinese names before 1895. Moreover, given that the Chinese interpreted the nature of these elements based on Chinese natural philosophy, the influence of these names was hard to estimate. Nevertheless, any language limitations could have been overcome if experiments had also been conducted. However, if there was a flaw in the

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Chinese scientific method, it was in its lack of practical experimentation and observation –instead preferring to study and research through the reading of books. Thus, although the Chinese studied the nature of the elements, they neglected the importance of quantitative detail, which is the basis of good chemistry.

### **The introduction of chemistry into nineteenth century China**

There were three main channels for the introduction of modern Western chemistry into nineteenth-century China, namely, through medical missionaries, Tongwen College, and the Jiangnan Arsenal.<sup>1</sup>

In 1855 Benjamin Hobson (1816-1873), an English medical missionary, made mention in his influential book –*Bowu Xinbian* (New Treatise on Natural Philosophy and Natural History)– of 56 elements found in nature. In the book he also briefly introduced the properties and preparation of oxygen, hydrogen and nitrogen. This has long been considered to be the beginning of the introduction of modern chemistry into China. Along with these developments, 17<sup>th</sup> century Jesuit missionaries, keen to influence Chinese belief in their Christian God, decided to replace astronomy with medicine as the chief means through which to proselytise.<sup>2</sup> They reasoned that, in the training of medical practitioners, knowledge of chemistry was considered a basic requirement.

In 1870, John Kerr (1824-1901), an American missionary, and Liaoran He collaborated to publish a textbook of chemistry –*Huaxue Chujie* (Elements of Chemistry)– which proved to be one of most significant publications on chemistry during this period. Liaoran He, a Chinese scholar, learned Western medicine from Hobson during the 1850's; while Kerr, the founder of the Chinese Medical Missionary Association established in 1887, played an important role in introducing Western medicine and chemistry into late Qing-dynasty China.

A second path for the introduction of chemistry into China came through the Tongwen College. In order to train translators the Qing government, after a series of military defeats, established Tongwen College in 1862, the earliest modern government school in China. After intense arguments with Chinese high officials, Tongwen College students began, in 1867, to learn Western natural science. One year later, William Alexander Parsons Martin (1827-1916), an American Presbyterian missionary, published the first textbook on natural philosophy in Chinese –*Gewu Rumen* (Elements of Natural Philosophy and Chemistry). This book contained seven volumes; the first five volumes dealing with natural philos-

ophy, the sixth with chemistry, and the seventh with mathematics. The sixth volume of *Natural Philosophy* was later republished as *Jiaohui Xinbao* (The News of the Churches) in 1869 under the title *Huaxue Rumen* (Introduction to Chemistry) –and is the version used in the current study. In 1873, Anatole Billequin (1837-1894), a chemistry teacher from France working at Tongwen College, also published a textbook on chemistry for their students.

The third channel for the introduction of modern Western chemistry into late Qing China came through the influential organisation known as the Jiangnan Arsenal.<sup>3</sup> The department of translation at Jiangnan Arsenal was founded in 1867, and in 1871 the department published its *Huaxue Jianyuan* (Mirror of Chemical Science) –which was translated by John Fryer (1839-1928) and Xu Shou (1818-1884). *Mirror of Chemical Science* became the most influential chemistry textbook in nineteenth century China.<sup>4</sup> Both Fryer and Xu cooperated to translate many other works on chemistry and related subjects: including *Huaxue Jianyuan Xubian* (Continuation of Mirror of Chemical Science, 1875); *Huaxue Jianyuan Bubian* (Supplement to Mirror of Chemical Science, 1880); *Huaxue Kaozhi* (Qualitative Chemical Analysis, 1883); and, *Huaxue Qioushu* (Quantitative Chemical Analysis, 1883).<sup>5</sup>

Interestingly, the original versions of “*Continuation of Mirror*” and “*Supplement to Mirror*” were based on Bloxam’s work on inorganic and organic chemistry –see *Bloxam’s Chemistry: Inorganic and Organic, with Experiments and a Comparison of Equivalent and Molecular Formulas* (London: John Churchill and Sons, 1867) –and were to become the first and most meaningful textbooks on organic chemistry in nineteenth-century China. Moreover, *Bloxam’s Chemistry* became the basis for *Huaxue Cailiao Zhongxi Mingmubiao* (*A Chinese-English Vocabulary of the Names of Chemical Substances*). This book, the first dictionary on chemical substances, was published in 1885 by the Jiangnan Arsenal. Following the translation of *Bloxam’s Chemistry*, no other textbooks dealing with organic and inorganic chemistry were more advanced or abundant than this book, at least before 1895.<sup>6</sup> *Huaxue Kaozhi* and *Huaxue Qioushu* which were translated from the *Manual of Qualitative Chemical Analysis* (New York: John Wiley, 1875) and *Quantitative Chemical Analysis* (London: John Churchill, 1876) of Karl R. Fresenius (1818-1897) are respectively the first Chinese publications on qualitative and quantitative chemical analysis.<sup>7</sup>

Thus the *Mirror of Chemical Science*, although nothing more than a book of elements, nonetheless became the most influential publication on chemistry during the late Qing-dynasty. On the other hand, as to the original works, no publica-

tions had more influence than *Wells' Principle* and *Bloxam's Chemistry*. Wells made use of the arrangements, classification of subjects, and illustrations, from the works of Faraday, Miller, Graham, Regault and Hayes. For example, utilising Miller's *Elements of Chemistry* (Part I-III, New York: J. Wiley, 1871-74), and Graham's *Elements of Inorganic Chemistry* (Philadelphia: Blanchard and Lea, 1858), Wells began by devoting four chapters to matters of physics, covering 155 pages. The remainder of the book devoted eleven chapters to inorganic chemistry, and ten chapters to organic chemistry. The great general principles of chemistry, and the more important properties of the elements and their compounds, were, accordingly, very fully discussed. On the other hand, the custom adopted by many other text-books of enumerating and describing compounds which have little or no practical value or scientific interest, was disregarded.<sup>8</sup>

Alongside the work of Wells, *Bloxam's Chemistry* consisted of 453 paragraphs, headed by a topic title, covering 676 pages. Part I, the introduction, contained only three paragraphs; Part II, dealing with the chemistry of the non-metallic elements, had 179 paragraphs; and, Part III, dealing with metallic elements, comprised paragraphs 183-323. The last part dealt with organic chemistry.

### Translation(s) for the term “element”

Aristotle's four-element concept was introduced by the Jesuits into China at the beginning of the sixteenth-century.<sup>9</sup> The term ‘element’ was first translated as *yuanxing* (primary phase), under the influence of the term *wuxing* (five phase) –which contained metal, wood, water, fire and earth. In *Qiankun Tiyi* (On the Structure of Heaven and Earth), published by the Jesuit Matteo Ricci (1552-1610) in 1608, the term element was defined as ‘a substance that is pure’.<sup>10</sup>

During the nineteenth century there came to be two translations in use for the term ‘element’; one was *yuanzhi* (primary substance), and the other was *yuanzhi* (original substance). Although the sound of the two *yuan* is the same, their characters are different. The former was translated by Hobson in his *New Treatise*, and the latter was used by Martin, Kerr, Fryer and Billequin. However, the term for element used by the Chinese today –*yuan**su* (primary substance)– is a Japanese-Chinese combined character, introduced at the beginning of the twentieth century.

## The number of elements

Prior to 1895, 65 elements had been introduced into Chinese chemistry. However, before 1875, the introduction of *Mirror* said that there were only 64 elements in nature. The 65th element, gallium, discovered by the French chemist using spectroscopy in 1875, was only introduced at the back of the *Supplement to Mirror of Chemical Science*.<sup>11</sup>

Prior to this, Hobson's *New Treatise* mentioned that 56 elements had been discovered in the world. However, Hobson only briefly introduced four elements –namely oxygen, hydrogen, nitrogen and carbon. In his *Introduction to Chemistry*, Martin mentioned that 62 elements had been discovered, but only 42 elements were listed –and only 25 of these were named in Chinese. In Kerr's *Elements of Chemistry*, he stated that 65 elements had been analysed in chemistry. However, Kerr only listed 63 elements that had been translated into Chinese. The four elements, cesium, rubidium, thallium and indium, discovered during 1860 to 1863, were named in *Elements of Chemistry*, but not introduced in the text.<sup>12</sup> Their discoveries, and the meaning of their names and their properties, were first described in Fryer's *Mirror*.

In 1858 *Wells's Principles* listed 62 elements, with pelopium and ilmenium wrongly mistaken as elements. Some other non-English named elements - for instance, stibium (antimony), natrium (sodium), kalium (potassium), wolfram (tungsten) - were also listed in his table. However, it is clear that the four elements - cesium, rubidium, thallium and indium –which were on the elements list in Fryer's *Mirror*, were not listed in *Wells's Principles*. They were noted from *Bloxam's Chemistry*. Moreover, like most other textbooks, the elements were listed by chemical name in *Wells's Principles*; whereas the elements in *Bloxam's Chemistry* were classified by their chemical properties: non-metallic and metal elements.<sup>13</sup>

*Supplement of Mirror*, a translation of inorganic chemistry, was more readily available and more advanced in chemistry than *Mirror*, and this was the only publication which introduced the element gallium into nineteenth-century China. So why didn't *Supplement* become as popular as *Mirror*? One of the reasons is probably that it was more difficult to understand and because there were only a few chemists in China who could explain the content in Chinese.

When the Chinese began to study modern Western chemistry in the nineteenth century, the introduction of the history of science was definitely helpful for them. *Wells's Principles* and *Bloxam's Chemistry* both had the same characteristic, namely that the histories of the elements were also mentioned, including the meanings of terms and their discoveries. But Wells paid greater attention to the

history of science than was heretofore customary in elementary text-books. He did this primarily to enable students to understand more clearly that chemistry relates to the past and present progress of civilisation, and to the industrial operations of the age. What the Chinese thought of the discovery of the element was, in some respects, an essay on the progress of civilisation. In the late Qing-dynasty China, Westerners were still considered as barbarians. Thus the scientific progress of the West, particularly as it related to chemistry, was beyond the imagination of most Chinese. Moreover, when the Chinese studied Western mathematics, introduced by the Jesuits during the late Ming dynasty, they came to the view that Western study had originated in China.<sup>14</sup> This view was to persist well into the late Qing period.

### Translation difficulties

The task of translating the names of about 50 new elements into Chinese was probably the first thorny problem facing the introduction of modern chemistry into China. Elements such as *jin* (gold), *yin* (silver), *tong* (copper), *tie* (iron), *xi* (tin), *qian* (lead), *gong* (mercury), *tan* (carbon) and *liu* (sulphur), were already well known in ancient China, so these names were kept in the list of elements.

In 1849 Hobson first introduced oxygen and nitrogen air in his *Tianwen lyuelun* (A Digest of Astronomy). Oxygen air was named *yangqi* (nurturing gas) because due to its obvious ability to sustain life. Yet, if there was too much oxygen in the atmosphere, it was instead destructive to life. Thus the role of nitrogen was seen as being able to harmonise oxygen, making it more suitable for all life-forms. Nitrogen, therefore, was called *danqi* (diluting gas).<sup>15</sup> In 1855, Hobson added the role of hydrogen air in his *New Treatise*. He named hydrogen as *qingqi* (light gas), referring to its property of being the lightest of the chemical element.<sup>16</sup> Hobson's translation of *yang* (nurturing, oxygen) and *qing* (light, hydrogen) was accepted by all translators before 1895. Moreover, his translation of *danqi* (diluting, nitrogen) was also accepted by Kerr and Fryer. Interestingly, Hobson's translation of *yang* (oxygen), *qing* (hydrogen) and *dan* (nitrogen) are still in use today.<sup>17</sup>

Starting from this base, translators began to give Chinese names to new elements by either referring to existing terms, or by using the meanings or pronunciation of Western terms and their chemical properties. Martin, Wilhelm Lobscheid (1822-1890), Kerr, Fryer, Daniel Jerome MacGowan (1814-1893), and Billequin, all used different principles for translating the names of new elements during this period. As a result, some elements had as many as six different



names in Chinese. In attempting to overcome these translation difficulties, the most significant rule for translating elements was that used by Fryer and Xu in *Mirror of Chemical Science*, namely that each element be assigned by a radical-syllable coined character.

Martin named some new elements by translating their meanings –for instance, *guangyao* (light medicine) for phosphorus, *shijing* (stone essence) for calcium. He also used existing names for already discovered elements in China –for example, *xinshi* (arsenic); *mengshi* (manganese). Specifically, he used the character *jing* (essence), which is the second character in the name –for instance, *fanjing* (alum essence) for aluminum, and *pojing* (glass essence) for silicon. His names for elements, therefore, should have been easily accepted by the Chinese, given that he coined no new characters and used only the existing names. However, only 39 elements were termed by Martin.

Almost in the same year, Wilhelm Lobscheid, a Germany missionary, used the character *xing* as a basis for coining 22 new elements. He did this by inserting a character, which was usually the meaning of the Western term, or the chemical properties of the element, in the middle of *xing* (element) – for example: bromine, carbon, chlorine, fluorine, hydrogen, iodine, nitrogen, oxygen, phosphorus, potassium, selenium, silicon, sodium, strontium, tellurium, thorium, titanium, tungsten, uranium, vanadium, yttrium, zirconium.<sup>18</sup>

#### The formation of Lobscheid's character



Lobscheid was the first to present new elements through a single coined character. However, while Lobscheid's combination character may have had an impact on Fryer's translation principles for elements, his characters showed a definite relationship to MacGowan's terms. In 1872 Daniel Jerome MacGowan and Hengfang Hua (1833-1902) transcribed the English sounds of the elements in their *Jinshi Shibie* (Discrimination of Mineral). Barium, for instance, was *bei-er-yi-en*; calcium, *gai-er-x-ien*; magnesium, *mei-he-ni-xien*. These first characters were applied to combine with a metal radical to form characters by Fryer and Xu. MacGowan was an American Baptist missionary and a translator. Hua was well known as a mathematician as well as a translator. Although *Discrimination of Mineral* was published in 1872, its chemical terms were already translated by 1868.<sup>19</sup> Its original version was the *Manual of Mineralogy* by Dwight Dana (1813-1895).

Before the publication of *Mirror of Chemical Science*, Fryer's translation of the names of the elements had already influenced another translator. In 1869, Fryer sent his names of elements to Kerr for his opinion and comment. In turn, Kerr applied Fryer's ideas for the translation of elements in his *Elements of Chemistry*.<sup>20</sup>

#### Fryer-Xu's radical-syllable combination



Out of the 65 elements, Fryer and Xu used nine traditional names and four from other translators. Of the remaining 52 only two were translations: one being *chou*, which means stink with water radical for bromine; and the other being *bo*, which mean white with metal radical for platinum. The names of the other 48 elements were assigned by a radical-syllable character. Today, 41 of their translations are still in use.<sup>21</sup>

### The Influence of Chinese Natural Philosophy

The concept of elements in ancient China differs from that in Western culture. There were five elements in China –namely, metal, wood, water, fire and earth– which form an interdependent cycle relationship. For example, in the Chinese view, fire brings forth wood, and wood brings forth water. According to the Chinese view of nature, fire represents not only real fire, but also the substances belonging to the character of fire. Chinese philosophers emphasised that everything in nature changes and transforms perpetually. Thus, when the four-element theory was introduced into China by the Jesuits during the late Ming-dynasty (1368-1644), they were understood only through the prism of the Chinese five-phase theory. This situation led to many arguments as Chinese scholars and Jesuits attacked each other with their own concepts and doctrines. During this controversy over five-elements- and four-elements theory, one of the crucial debates to emerge was that over whether or not air was an element. For Chinese philosophers air was not a substance, because it could not be seen and man could

not measure its weight. On the other hand, the Jesuits provided several observations and measurements demonstrating that air was really an element.<sup>22</sup>

The term element was first translated as *yuanxing* (primary phase), under the influence of the term *wuxing* (five phase). In *Qiankun Tiyi* (On the Structure of Heaven and Earth), published by the Jesuit Matteo Ricci (1552-1610) in 1608, the term element was defined as a substance that is pure.<sup>23</sup>

During the nineteenth century there were two translations in use for the term 'element'; one was *yuanzhi* (primary substance), and the other was *yuanzhi* (original substance). Although the sound of the two *yuan* is the same, their characters are different. The former was translated by Hobson in his *New Treatise*, and the latter was used by Martin, Kerr, Fryer and Billequin. However, the term for element used by the Chinese today –*yuansu* (primary substance)– is a Japanese-Chinese combined character, introduced at the beginning of the twentieth century.

In Hobson's *New Treatise*, the concept that air was an element in itself was particularly emphasized:

"Substance, man thinks, is that which he can see, and gas (*qi*) is that which he cannot see. However, there is evidence to believe that gas (*qi*) is substance. In the universe, gas (*qi*) transforms to substance, and substance also transforms to gas (*qi*)."<sup>24</sup>

*Zhi* or substance, the second character in the Chinese name, not only shows its modern definition, but also indicates the Chinese acceptance of the Western concept. Although the Chinese scholars accepted the definition of element in modern chemistry, they always interpreted the chemical meaning of elements under the influence of such Chinese thinking as natural philosophy. The Chinese translations are definitely one factor to result from such phenomenon. When the Chinese learned the principles of the chemical elements, they always understood them from the meaning of their terms. More importantly, when the Chinese studied Western mathematics, introduced by the Jesuits during the late Ming dynasty, they came to the view that Western study had originated in China.<sup>25</sup> This view was to persist well into the late Qing period. Moreover, the similarity in Chinese and Western natural philosophy promoted the formation of this concept. That, therefore, provided the Chinese scholars with the opportunity to claim that the Chinese classics of natural philosophy correspond to the principles of Western modern chemistry –for example the dualism and yin-yang theories.

Therefore, the elements of gas –such oxygen, nitrogen, hydrogen, fluorine, and chlorine– were regarded by the Chinese as having already been discovered, and were interpreted under the concept of *qi*:

“*Yang* (oxygen) is known as a living gas; *qingqi* (hydrogen air) as a light gas; *lyuqi* (chlorine air) as a yellow-green gas; and *Fuqi* (fluorine air) as a malicious gas. These substances are well known in China as chemical elements. And everyone knows that they are Chinese substances.”<sup>26</sup>

The concept of *qi* is one of the oldest and most influential natural philosophies in China. *Qi* is said to be invisible yet present in all substances. Thus *qi*, invisible matter, was used to explain how water, earth, electricity and magnetism could have energy or power in order to cause a change of state. Hence, electricity and magnetism were named as electrical *qi* and magnetic *qi*. Xi Zhu (1130-1200), a Chinese scholar in the Southern Song Dynasty (1127–1279), complemented the theory of *qi* with his principle of *li*. He said that *li* and *qi* were the origin of all living things and that *qi* was derived from *li*. Moreover, *li* was also at the root of the creations of all living beings.<sup>27</sup>

In addition, since the chemical element is defined as the simplest substances from which all living being are composed, the Chinese always connected their properties with the Chinese ancient alchemical achievement. Indeed, the sixty-four elements discovered and mentioned in Fryer’s *Mirror*, were regarded by the Chinese as the sixty-four *gua* (trigrams) in the *Yijing* (Book of Changes) The sixty-four trigrams are the total number of combination of trigrams in *Book of Changes*:

“In the early antiquity Fu Xi invented the trigrams. *Yin* and *yang* exchange each other. The firm and the yield displace each other. The foundation of sixty-four trigrams is like that of the sixty-four chemical elements. Shen Nong tested hundreds of herbal medicines, suffered from seventy-two poisons in one day, and was divine in the transformation they wrought. *Bencao Jing* (Classic of Materia Medica) indicated gold, silver, copper and iron smelted by sulfur. Seventy-two stones melted in saltpeter. Sulfuric acid and nitric acid are the evidences. In his book *Suwen Jing* (Classic of Plain Questions), Xuan Yuan wrote that water is yin, fire is yang. Yang is transformed into gas, yin is transformed into solid. The principle of their transformation is the same as that for the transformation between vapor, liquid and solid. It also said that heat makes deconstructive, and blend makes productive that creates all beings. Thus the rulers of the action and creation of substances is clear.”<sup>28</sup>

Fu Xi, who is also said to have invented the one hundred Chinese family names, is the first of three noble emperors in Chinese mythology. His successor, Shen

Nung, who is also one of the three, is named as the “God of Herbs.” The Classic of Materia Medica which is the earliest Chinese herbal book, was published before the Western Han-Dynasty (206 BC to 24 AD). However, the author of this book is unknown. Xuan Yuan, the last of the three noble emperors, is considered to be the ancestor of all Han Chinese. Speaking to the change of matters, the Chinese were inevitably disturbed by the Chinese natural philosophy, for example, the influence of the concept of *yin-yang* on the learning of physical changes.

### **From alchemy to chemistry**

As in the West, Chinese alchemy pursued the search for the elixir of life and the transmutation of gold. Western alchemy, however, had transformed into modern chemistry by the end of the eighteenth century. When the Chinese first learned about modern Western chemistry, many students would raise questions in the chemistry class. Questions such as: How did chemistry in the West develop? Or, how did alchemy transform to chemistry in the West? Or, what are the differences in alchemy between China and the West? The discussions that followed these questions were useful to the Chinese in assisting their understanding and learning of modern Western chemistry. Moreover, under Martin’s influence, the Chinese began to consider that the discovery of the elements was the single most meaningful distinction between alchemy and chemistry.

As the author of the first textbook on chemistry in China, Martin had an ongoing and sustained interest in Chinese alchemy, believing that Western alchemy had probably itself been brought from China.<sup>29</sup> He was very interested in the questions that the Chinese raised, and himself raised more detailed questions on the comparisons between alchemy and chemistry in the back of his *Introduction to Chemistry*.

At first, Martin compared the differences of the methods in both alchemy and chemistry. He argued that, in ancient times, alchemists sought a good place to build stoves and smelt their base metals in order to feel the ‘inspiration of the constellation’. Moreover, in their efforts to manufacture medicine, the alchemists always operated according to the Five Phase principles. However, Martin went on to assert that even if the alchemists had ‘eaten the radiance of the moon and taken the red cloud of the morning’, they could never have obtained the essence of matter. So what, then, was the means of chemistry? Martin explained:

“Today’s chemists start from an analysis of elements. They resolve not only to understand the different elements, but also to understand the principles lying

behind the different combinations of elements. To combine new compounds or to decompose substances, these are the principles of chemistry; and these theories are apparent.”<sup>30</sup>

What Martin mentioned above said nothing about the method of analysis or the functions of the equipments used. He probably intended to say that chemical apparatus could be used to resolve more elements. However, although Martin could not indicate the exact means of chemistry, he did reinforce the view that it came about through the discovery of elements and the use of chemical instruments.

Following on from this, Martin spoke about the comparisons of their theories. The alchemist, he said, considered that all metals had the same property. If they could transmute each other, the baser metals could be raised to gold, and the nobler one could be reduced to lead. Alchemists also mentioned that all substances grow as seeds, formed in the earth –much like the combination of sperm and egg. On the other hand, Martin explained that chemistry reduces into more meaningful theories:

“However, the chemist regards that all metals are different. Although they attract each other, they can absolutely not exchange mutual. From lead, silver is extracted, because silver is originally mixed with lead. In cinnabar, mercury is obtained, because cinnabar originally combines with mercury. When the element which is always resolved in the medicine of the manufacture or extracted in the smelting gold, because that this element is at first contained this substance. There is not the so-called mutual exchange of nature.”<sup>31</sup>

Finally, Martin shifted to a discussion of the results of alchemy. Many of the mysterious alchemical operations had no the basis in fact, and were merely a desire to acquire more wealth. By contrast, the success of chemistry lay in its application of the analysis of elements, and in medicine. Nevertheless, while Martin compared alchemy and chemistry in their methods, theories, and results, he did not exactly name the alchemists, the chemists, nor the elements in his explanations. Yang Yuhui, meanwhile, a Chinese scholar and a student of Shanghai Polytechnic Institute, has given us a more detailed discussion on this subject:

“It is said in Chinese alchemy that *heche* was water, *zhuque* was fire. That is absurd. It is said that *huangjing* is food, and *baishi* is provision. It is preposterous. The refining of medicine and the analysis of matter in alchemy is the same as in chemistry. Phosphorus in chemistry was discovered by the alchemist Brandt. The more we can improve our chemical research, the clearer will be our understanding of the properties of substances. Phosphorus, for example, was discovered when it burns in the air, giving off a very bright flame, and extremely vio-

lent combustion, especially in oxygen air. The element phosphorus was discovered accidentally, so it really should not be regarded as a success for chemistry. However, the discoveries of other chemical elements does occasionally take place, Iodine, for instance in the form of bluish-black scales, was found by Courtois in the sea-weeds. Bromine, an extremely volatile substance, was occasionally obtained by Ballard from sea-water. These results occurred unexpectedly. The way of chemical success is gradually achieved from rough to perfect. For instance, the research of borax, this substance was found but its constitution was not clear. It was not until Homberg's experiment that man knew that boron was an element. Soon after, man knew after Gay-Lussac's success that boron was an element. However, man did not still realize the properties of boron. After unceasing investigations mutually, its properties was finally to be understood. The scientific results can not be immediately reached. Chemistry succeeds after alchemy; the understanding of the analysis of elements in alchemy leads to chemistry. Alchemy occurred before chemistry; alchemy is the origin of chemistry."<sup>32</sup>

*Heche*, *zhuque*, *huangjing* and *baishi* are the obscure symbolic language of the Chinese alchemists. The alchemist, the chemists and the discoveries of the elements which Yang mentioned came from Fryer's *Mirror*.

Returning again to Martin's explanation on the subject, one arrives arrive at the question: How did the Western countries make such rapid developments in chemistry? Martin had asserted that Western governments prohibited the alchemist's attempts to make precious metals in order to prevent swindles. In addition, the activities and organisation of chemistry was encouraged through the arrangement of teachers and the establishment of laboratories in the West. As a result, many students learned chemistry and chose this as a profession. For Martin, this had a great impact on the development of chemistry.<sup>33</sup> However, it was in the area of scientific education that the Chinese struggled initially to realise its function and influence. For them, study should focus on how to govern the country, not on the so-called "arts" or "techniques".

## Conclusion

Fryer and Xu were definitely the most significant partners in the introduction of modern chemistry into late Qing China. Their *Mirror* textbook dominated learning about the chemical elements during this period, mainly because it was the first and most detailed textbook, with the introduction of 65 elements. Moreover, Fryer and Xu also paid much attention to the historical development of modern chemistry, and were the first, and only ones, to introduce the five elements



cesium, rubidium, thallium, indium and gallium into nineteenth-century China. Nevertheless, despite the earlier influence of Fryer and Xu, it was Wells' *Principles*, although not as advanced and academic as *Mirror*, that became the most influential textbook on chemistry.

Although gallium, the 65<sup>th</sup> element, was introduced into China via *Supplement to Mirror* (1880), only 64 elements were well known in China before 1895. The naming of the elements in Chinese illustrates the shortcoming of chemical communication in China. As suggested here, one of the reasons was that, as there was no analysis of, or experimental confirmation on, the basic principles of chemistry, the Chinese had difficulty in understanding the subject. And this was to prove a great obstacle to the development of science in China.

At the beginning of the twentieth century, Fryer and Xu's single radical-syllable character principle for the translation of elements finally became the standard for Chinese chemical nomenclature. Such coined radical-syllable characters were so strange and exotic at that time that almost no Chinese had the basic English ability to understand them. But after repeated efforts, and a series of publications, this principle became the most important method for translating the names of new substances in chemistry.

As the Chinese gradually learned of Western scientific knowledge during the late Qing, they always sought to understand and interpret this knowledge through the rationale of Chinese natural philosophy. However, as with the discovery of the elements, the Western study of chemistry could not be interpreted through the natural philosophy of the Chinese due mainly to the concept of Chinese origins of Western science. For example, the mistaken Chinese belief, in the late Qing dynasty, that they had discovered oxygen, nitrogen, hydrogen, fluorine and chlorine long before their discoveries in the West.

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## References

- <sup>1</sup> James Reardon-Anderson, *The Study of Change: Chemistry in China, 1840-1949* (Cambridge: Cambridge University Press, 1991), 17-74, on 34.
- <sup>2</sup> William Lockhart, *The Medical Missionary in China* (London: Hurst and Blackett, 1861), 11-21.

- <sup>3</sup> Kunhou Li, "Jiangnan zhizaoju yu zhongguo xiandai huaxue (The Jiangnan Arsenal and modern Chinese chemistry)," *Zhongguo kexueshi lunji*, ed. Zhiping Lin (Taipei, 1958), 43-66.
- <sup>4</sup> Qinglian Zhang, "Xu Shou yu Huaxue jianyuan (Xu Shou and *Huaxue jianyuan*)," *Zhongguo Keji Shiliao* (The Chinese Journal for the History of Science and Technology) 6 (1985): 54-56.
- <sup>5</sup> Arthur Bennett, *John Fryer: The Introduction of Western Science and Technology into Nineteenth-Century China*, (Cambridge, Mass.: Harvard University Press, 1967), 56-71.
- <sup>6</sup> Guangding Liu, "Qingdai huaxue shujie mulugao (Catalogs of chemical translation of the Qing dynasty)," *Guoli Zhongyang Tushuguan Guankan* (National Central Library Bulletin) 25(1992): 205-217, on 210.
- <sup>7</sup> Hanqing Yuan, "Zuizao yicheng zhongwen di liangbu fenxi huaxueshu ji yuanzuoze xiaozhuan (The First Two Books on Analytical Chemistry Translated into Chinese and a Short Biography of the Author)," *Huaxue Tongbao* (Chemisches Fachbulletin) 2(1982): 115-117, on 116.
- <sup>8</sup> David Ames Wells, *Wells's Principles and Applications of Chemistry* (New York: Ivison & Phinney, 1858), preface, 1.
- <sup>9</sup> Kuangtai Hsu, "Mingmo xifang siyuansushuo de chuanru (The transmission of Western Four-Elements Theory in late Ming China)," *Tsinghua Xuebao* (*Tsing Hua Journal of Chinese Studies*), new series 27(1997): 347-380.
- <sup>10</sup> Matteo Ricci, *Qiankun Tiyi* (On the Structure of Heaven and Earth), in *Siku Quanshu* (Complete Collection in Four Treasures), ed. Yuan Ji (Taipei: Commercial Press, 1984), vol. 787, 763-764.
- <sup>11</sup> John Fryer and Shou Xu, *Huaxue Jianyuan Bubian* (Supplement to Mirror of Chemical Science) (Shanghai: Jiangnan Arsenal, 1880), vol. 6, 20a-b.
- <sup>12</sup> John Kerr and Liaoran He, *Huaxue Chujie* (Elements of Chemistry) (Canton: Canton Hospital, 1870), 6a-6b.
- <sup>13</sup> Charles Bloxam, *Bloxam's Chemistry: Inorganic and Organic, with Experiments and a Comparison of Equivalent and Molecular Formulas* (London: John Churchill and Sons, 1867), 1.
- <sup>14</sup> Hansheng Quan, "Qingmo de 'Xixue yuanchu zhongguo' shuo (The late Qing theory that 'Western studies originated in China')," *Lingnan Xuebao* 4(1935):57-102.
- <sup>15</sup> Benjamin Hobson, *Tianwen lyuelun* (A Digest of Astronomy) (Canton: Huiai Hospital, 1849), 10b.
- <sup>16</sup> Benjamin Hobson, *Bowu Xinbian* (New Treatise on Natural Philosophy and Natural History) (Shanghai: The London Missionary Society Press, 1855). 10a.
- <sup>17</sup> Hao Chang, "Yang qing dan de fanyi (The Chinese Translations of Oxygen, Hydrogen and Nitrogen: 1896-1944)," *Ziran Kexueshi Yanjiu* (*Studies in the History of Natural Science*) 21(2002): 123-134, on 124.
- <sup>18</sup> William Lobscheid, *English and Chinese Dictionary, with the Punti and Mandarin Pronunciation* (Hongkong: Daily Press Office, 1866-1869), 376.
- <sup>19</sup> Daniel MacGowan and Hengfang Hua, *Jinshi Shibie* (Discrimination of Mineral) (Shanghai: Jiangnan Arsenal, 1871), preface, 1.
- <sup>20</sup> Yangzong Wang, "Guanyu huaxue jianyuan he huaxue chujie (On *Mirror of Chemical Science: A Source-Book and First Steps in Chemistry*)," *China Historical Materials of Science and Technology* 11(1990): 84-88.
- <sup>21</sup> Hao Chang, "Chinese Terms for Chemical Elements: Characters Combining Radicals and Phonetic Elements," *Chemistry International*, 26 (2004): 10-13.

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- <sup>22</sup> Kuangtai Hsu, "Mingmo xifang siyuansushuo de chuanru (The transmission of Western Four-Elements Theory in late Ming China)," *Tsinghua Xuebao (Tsing Hua Journal of Chinese Studies)*, new series 27(1997): 347-380.
- <sup>23</sup> Matteo Ricci, *Qiankun Tiyi* (On the Structure of Heaven and Earth), in *Siku Quanshu* (Complete Collection in Four Treasures), ed. Yuan Ji (Taipei: Commercial Press, 1984), vol. 787, 763-764.
- <sup>24</sup> Benjamin Hobson, *Bowu Xinbian* (New Treatise on Natural Philosophy and Natural History) (Shanghai: Mohai Shuju, 1855), 10a.
- <sup>25</sup> Hansheng Quan, "Qingmo de '“Xixue yuanchu zhongguo”' shuo (The late Qing theory that "Western studies originated in China")," *Lingnan Xuebao* 4(1935):57-102.
- <sup>26</sup> Ji Ouyang, in *Kezhi Shuyuan Keyi* (Themes of the Shanghai Polytechnic Institute), ed. Tao Wang, (Shanghai: Kezhi Shuyuan, 1886-93), Spring 1890, vol. 2, 29.
- <sup>27</sup> Joseph Needham, *Science and Civilisation in China. Volume II, History of Scientific Thought* (New York: Cambridge University Press, 1980), 472-485.
- <sup>28</sup> Danluo Cheng, in *Kezhi Shuyuan Keyi*, Spring 1890, vol. 2, 20.
- <sup>29</sup> William Martin, *The Lore of Cathay* (New York: Fleming H. Revell Company, 1901), 52.
- <sup>30</sup> William Martin, *Gewu Rumen* (Elements of Natural Philosophy and Chemistry) (Beijing: Tongwen College, 1868), vol. 6, 78.
- <sup>31</sup> Martin, *Elements of Natural Philosophy and Chemistry*, vol. 6, 78-79.
- <sup>32</sup> Yuhui Yang, "Zhongxi liandan huaxue yitongshuo (What are the differences and similarities between alchemy and chemistry in China and in the West?)," in *Gezhi Zhiping Tongyi* (General Discussion on the Development of National Science), ed. Yuhui Yang (Shanghai: Hungbao Shuju, 1901), vol. 5, 1.
- <sup>33</sup> Martin, *Elements of Natural Philosophy and Chemistry*, vol. 6, 78-79.

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## The Spread of “Chymia” and Lavoisier’s Views in the Greek Speaking Regions

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### Introduction

The purpose of this paper is to report a study of the spread of new scientific ideas, especially on chemical issues, in the Greek speaking regions of the Ottoman Empire during the seventeenth and eighteenth centuries. It is of great historiographical and educational interest to examine the transmission of scientific knowledge from the “center” to the countries at the “periphery” in Europe.

The way in which the dialectic of new chemical ideas was accepted in the Greek intellectual milieu, in order to formulate a discourse on chemical philosophy, is being researched through the textbooks of the representative scholars participating in the didactic traditions shaped in Greek speaking regions.

Certain modern historians of science have investigated the spread of scientific theories from the center to the periphery of Europe and they have designated education as the main characteristic of Greek intellectual life in the 18<sup>th</sup> century.<sup>1</sup> Also the writing or the translation of textbooks was oriented to this aim. However, most of them, influenced to a different degree from the sociology of knowledge, treat the multidimensionality of the process in a such a way as to be focused on the “specificity” and the “priority” of the local communities. On the contrary, we try to comprehend the spread of new scientific approaches in a dynamic interactive process between periphery and center, considering the “integrating and unifying power of the Scientific Revolution and its Enlightenment underpinnings taken as a whole”.<sup>2</sup> For this reason, we adopt Althusser’s thinking according to which the social whole it should be investigated further under the approach of the theory of the complexity of social totality, where elements and levels have an indicator of effectiveness.<sup>3, 4</sup> Finally, we try to investigate the claim of Bensaude-Vincent that the Greek scholars :“selectively picked what was of use for their local purposes”<sup>5</sup> also, we try to investigate if in these textbooks, beyond social, pedagogical and cultural purposes for which they were written, there exist

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also and elements of dialectic rationality that would be possible to constitute a unified corpus of theoretical practice, a philosophize practice of science or a more ‘theoretical scientific practice’, a ‘scientific-theoretical production’. That is to say, why certain scientific theories are preferred in relation to some others or if it can exist any productive compatibility between rival theories, since Europe was in a process of an ongoing evolution of scientific reason. In Europe, as far as chemistry is concerned, there already exists a fluidity of rival theories as Bensaude-Vincent reports: “the echoes of the chemical revolution in Italian, Spanish and Portuguese textbooks convey the view that Lavoisier’s chemistry was perceived as a paradigm shift only by a minority of chemists whereas many chemical communities in Europe rather described and spread it as a partial change that did not affect wholesale”.<sup>6</sup> For the above mentioned reasons, the investigation of the constitution of chemistry, as well as the situation of its prescience state, are proved a very fertile field.

It must also be noted that the great majority of the reading material and textbooks concerning the acceptance of chemistry in Greek speaking regions has not as yet been studied exhaustively and this remains a fruitful area for the research which is in progress. The results presented here are the preliminary findings.

### **The didactic tradition of “Chymia” in the Greek-speaking communities before the coming of Lavoisier**

The evolution of knowledge about “*chymia*” emerged in the Greek-speaking regions before the onset of dissemination of Lavoisier’s work in these areas, and two different didactic traditions were formed. One was the “*system of chymists*” and the other the *Newtonian tradition*.

In the first half of the eighteenth century in the Greek-speaking communities the first didactic tradition, to be discussed here is the “*system of chymists*”,<sup>7</sup> a systematic field, alternative to that of Neo-Aristotelianism, which was emphasising the significance of “*principle*”. This didactic tradition was associated with the theory of the five “*chymical principles*” was shaped through the work of Anthrakitis, Damodos and Kavalliotis. This was presented in three handwritten epitomes of natural philosophy, which has only recently come to light. Namely, in the *Philosophical Note Book* of Anthrakitis<sup>8, 9</sup> in the *Physiology* of Vikentios Damodos<sup>10, 11</sup> and in the *Natural Treatise* of Theodoros Kavalliotis<sup>12, 13, 14</sup>. In these three epitomes, regarded as “*chymical principles*” were the following five “*chymical elements*”: “*mercury*” (“*ermis*” or “*spirit*”), “*sulphur*” or “*divine*”, “*salt*”, “*flegma*” or “*water*”, and

“earth” (“earth tamed” or “dead head” or “spondos”(offering)).<sup>8, 15, 16</sup> The first three principles were defined as “energetic” or “particular” and the other two as “passive” or “material”. Their existence was obtained through experience, which was through the mediation of the senses. “Subject”, moreover, of the “chymical art” is «η ανάλυσις του φυσικού σώματος, εις τα αυτού μέρη αισθητώς ανόμοια από το ίδιον αναλελυμένον σώμα» (the analysis of the natural body into its parts which are observably unlike the body being analysed).<sup>17</sup>

This tradition, named “the system of chymists”, was close to the Boylean-Cartesian tradition and contrary to Aristotelianism. It accepted, the five “chymical” principles and also the analytical ideal, but the crucial issue for this tradition remained the “mechanical” principles, which were considered as the “thinnest molecules” in the theory of Descartes, were under the influence of the metaphysical nature of the Aristotelian principles. These were: the matter, the species, and the impenetrability. Thus, “chymia” was regarded as a particular “system”, as a distinguishable field of representation of “elements”, which, however, came under the metaphysics of principles. That is to say, that, even though it is implied, a rupture did not take place with the Neo-Aristotelian tradition (See Voulgaris’ case).

A second didactic tradition presented itself some decades later in the framework of Zerzoulis, Voulgaris, Theotokis, and Psalidas. They emphasised a distinct Newtonian orientation, but a relation of continuity or rupture between this tradition and the one previously discussed did not seem to exist. In Theotokis’ “Stoicheia Physics” (*Elements of Physics*),<sup>18</sup> only the *atoms* were defined as “principles” and “elements”. No particular chemical principles were considered to exist other than ‘chymical’ processes, which were afforded by physics.

A particular case of the second didactic tradition is seen in the work of Eugenios Voulgaris.<sup>19, 20</sup> He appeared however, to attempt to combine the previous tradition, in which the Boylean-Cartesian characteristics were predominant, in agreement with the Neo-Aristotelian tradition, and the Newtonian thinking, especially in the case of *mixt*, which was under the influence of attractive or repulsive forces of its constituent’s parts.

### After the publication of Lavoisier’s work, *Traité Élémentaire*

After the publication of Lavoisier’s work, *Traité Élémentaire*, was formed the Newtonian chymistry of Psalidas and the chemical texts of Fourcroy, Brisson and

Adet were translated into Greek. At this point, it should be noted that the classical work of Lavoisier, *Traite Elementaire* has never been translated into Greek.

### **a) The Newtonian chymistry of Psalidas**

Athanasios Psalidas (1767-1829), was a major Greek-speaking representative of Enlightenment. The first introduction of modern chemistry established by Lavoisier and his co-workers was presented to Greek speaking regions, by Athanasios Psalidas under the dominance of “*Newtonian dream*”. His textbook “*Peri Physikis en Geni* (“*On Physics in General*”) was published in 1795, it was an instructional book.<sup>21</sup> Psalidas tried to understand the systematisation of chemistry based on the model of the naturalists’ classification. He distinguished Nature in plant, animal and mineral. For his aim expounds certain cardinal themes of Lavoisier’s problematics, i.e. the “*oxigène*” theory, and the theory of combustion. In regard, however, of what we now designate as a chemical bond, Psalidas was under the influence of the “*Newtonian dream*” for a unified exact science for chemical and physical phenomena. He maintained a discursive stance in keeping “*chemical attractions*”-“*chemical affinities*” as problematic.<sup>22</sup> Psalidas also introduced the mathematical atomism of Boscovich, according to which the elementary texture of matter could be causally explained within this complex architecture of mathematical “*punkta*”.

Psalidas’ observations on the “generic principles” and on chemical nomenclature are of great interest:

1. He overthrew the Aristotelian assertion of four elements and he proved that these elements are not *Simple* but *Complex*. The elements, according to Psalidas’ classification, were divided in ‘*metaphysical*’ and ‘*chemical*’. As ‘*chemical*’ he defined those which were products of chemical analysis and ‘*metaphysical*’ were those which are only mentally perceived. With regard to the constitution of elements from atoms he accepts the Boylean thinking.
2. He considered *fire* as a “simple element” while Lavoisier treats it as an elastic and weightless matter and he identified it, with the “matters” of heat and light.
3. Water is constituted from ζωτικό (zotiko) (oxygen), and phlogiston. ζωτικό emanates from the Greek word ζωή which means *life*. He also identified “phlogiston” with “hydrogen”.



5. Although Psalidas considered phlogiston was identical to hydrogen, he did not accept the alchemical assertion that at the reduction of metals phlogiston was added to the metal calx. To the contrary, he claimed that ζωτικό (zotiko) (oxygen) was disengaged during the reduction because this appears to have lower affinity with the combustible body.
6. For acids, Psalidas maintained rather a qualitative definition, based however on questioning of Lavoisier's nomenclature; according to the degree of saturation of oxygen in acidifying substance he characterises acids as "oxydata" (rather oxides), "incomplete acids" and "enteli" (full) acids.
7. Lastly he distinguished salts in "acids", "alkaline" and "middle". Thus in the nomenclature of salts he does not follow the Lavoisier's theory and preferred to use the alchemic nomenclature, such as, the "salt of Glauber" etc.

While Psalidas accepted the significance of an element, according to Lavoisier's theory, and realised the role of oxygen in the combustion, he did not adopt the modern nomenclature of chemistry and the question of chemical affinity. Thus, he dealt more with the problem of "mixing", the "solution" and the "dissolution" as given emphatically in the chapters "on the affinity of bodies" and "on analysis and precipitation of bodies".<sup>22</sup>

### ***b) The translation of Fourcroy's work: "Philosophie Chimique"***

The first handbook of chemistry which was translated into Greek, in 1802, was the handbook "Philosophy of Chemistry or fundamentals truths of modern chemistry (*Philosophie chimique ou vérités fondamentales de la chimie moderne, disposées dans un nouvel ordre 1792*)" by the French chemist, Antoine Fourcroy. The translator was the Theodosios Iliadis and the editor, Anthimos Gazis. The handbook was printed in Vienna. This work of Fourcroy's adopts the core of Lavoisier's questioning and meanings on the elements, oxygen and caloric, it was however reported that Lavoisier avoided dealing in the *Traite* with the chemical and elective chemical affinity. This handbook as Bensaude-Vincent reports:<sup>23</sup> "exemplifies the systematisation of chemistry on the model of naturalist's classifications" giving the opportunity to the translator, in his comments in the preface of the translation,<sup>24</sup> to focus on its empirical points, stressing the interpretation of concepts concerning the chemical affinity. More specifically, on the concepts of *affinity of cohesion*, *affinity of adhesion*, *affinity of synthesis*, *double elective affinity*.

**c) The translation of the chemistry of Brisson and of Adet**

In 1801, the eminent Greek enlightener, the monk Dimitrios-Daniel Filippidi (1755-1832), translated into Greek, the *Elements or Physicochemical Principles* of Brisson, and that of Adet: "Leçons Élémentaires de Chimie, à l'usage des Lycées" by the heading "*Chemistry Epitome*" from K. Koumas, in 1808. The former also translated the "Logic" of Condillac in 1801. Both of translators, in their comments in the prefaces of their translations focused on the chemical nomenclature. Examining their questioning is a fruitful way to get to the core of their thinking.

**(i) The preface of Filippidis in the handwritten translation of the instructive handbook, "Elements or Physicochemical Principles" of Brisson, under the title "Origins, progress, top (acme) and decline of sciences in general, and partially of "χυμική" (chymistry)"<sup>25</sup>**

Here, «χυμική» (*chymistry*) was placed in the vanguard of *experimental philosophy*. He adopted and commented on the new chemical nomenclature that was established, on the new chemical truths, and it was shaped as a "*methodical language*", capable of removing each "*hypothesis*", each "*subject irrelevant to the chymical knowledge*" ... "*where it should be spoken to all the sciences... to which it should write the philosophers*".

Consequently, the chemical nomenclature constitutes a means for the establishment of a radically new teaching and at the same time shows the real possibility of total change in the basic education. On this subject Filippidis agreed with Lavoisier, who in the "*Thoughts for the public education*", in 1793, proposed an essential transformation for the instructive regime because it does not strengthen the "*basic human rights of people*", "*since it provides and repeats prejudices...*" and it must be replaced by another. One, where the dissemination of knowledge will codify the results of its "*natural*" production - via an experimentation that reconstructs the natural order – in such a way that not only ("*human cultivation is achieved*"), but also "*the gradual perfection of mental abilities of human kind is activated*". This proposal was inspired by the more general hope for the completion of political emancipation, for the establishment and stabilisation of Democracy, and it is combined with the aim of national education, which would attribute socially advantageous knowledge, providing, by means of "*large institutions*", a mechanism capable of ensuring "*the extension of economic exploitation for the industry*", and the nation with "*an increasing and permanent supremacy in all its commercial relations with neighbouring nations*".<sup>26</sup>

**(ii) Koumas comments in the preface of the translation of chemistry by Adet<sup>27</sup>**

This book of Adet, which was used as a handbook of chemistry in the Lycée of France, was published in 1804 and was translated into Greek in 1808. In the preface of the translation, Koumas elaborated on the chemical affinity with a perception of ancient Greek admiration. Moreover Koumas adopted the Lavoisiers' chemical nomenclature but with rather more grammatical perception than a conceptual criticism. Characteristic of this are:

1. He called gases, bodies according to the new nomenclature. Thus, he called hydrogen, *hydrogen gas* and oxygen, *oxygen gas*. On the contrary, he disagreed with the term *azotic gas* because he considers that in Greek, *azotic* gas means, nothing. For this reason he names the base, *nitrogen* and gas, the name *πανσιζωο* (pafsizoo), which means that it stops life.
2. Koumas was also in opposition to Lavoisier for the name of oxygen. Lavoisier had named it *oxigine* (from οξύς, acidum and γίνομαι, gignor) and Fourcroy *oxigene*. Specifically he reports: "*The first is a caricature, because a Greek name oxigine never existed,....from which Lavoisier could have translated it into his language as oxygine the term of Fourcroy also, if it is translated from oxygenes, has a contrary meaning to what it should have in the framework of the new theory of chemistry · oxygenes means this which is made by acids and not this from which acids are made*". He supported that the correct name was "οξύγονο" as the word oxygen is called in Greek today. Koumas also calls oxygen as "ζείδωρο", a word from Homer word, which means gift of life.

**Conclusions and questions**

In accordance with these didactic traditions, especially on chemical issues, the present paper is focused on and endeavors to answer the following historiographical questions:

1. How was the knowledge transmitted to the Greek speaking regions?
2. What was the interaction of the new scientific ideas with the local scientific culture?
3. What were the continuities or ruptures of the chemical thought in the didactic traditions created by the Greek scholars in the process of shaping a new scientific discourse?
4. What were the scientific (rational), philosophical, social or cultural terms, which dominated this diffusion of new ideas?

5. Was there any rupture or discontinuity with the Aristotelian tradition?
6. Was there rupture or a discontinuity between the scientific traditions as far as they were shaped?

It must be mentioned here that in Greek-speaking regions under the Ottoman dominance there were no Universities, Institutes or Academies, but only primary and secondary schools.

Our conclusions to these questions are as follows:

1. The emergence of a new urban class, which claimed a Greek State, and its peculiar relationship with the Church, specifically the Fanariot elite, the Ottoman Empire, and the Aristotle philosophy, shaped the tendency of Greek scholars into forming a scientific discourse based on empiricism but not in adversity with the Aristotelianism and theology.
2. This is obvious for the didactic tradition of “*chymia*” of Vulgaris and that of Damodos-Anthrakitis–Kavalliotis which stressed the “*chymical*” principles as distinguishable from the Aristotelian elements, however, it accepted the mechanical principles which were influenced by the metaphysics of Aristotelian principles. Thus ruptures are not seen between the new scientific discourse and Aristotelianism but there were some enlightened points of objection to scholastic Aristotelianism adopted by the practice of theology.
3. The main goal of the Greek scholars seems to have been the efforts of shaping, through the mediation of the new scientific ideas, a discourse at a time when no organised research structures were present. Under the dialectics of scientific, social and philosophical terms lead to didactic traditions based on Nature and not on the transcendental and metaphysical frames of theology. To this end, it seems it was the adequate the support of empiricism, which ensured a rational operation of the issues and not a more advanced and theoretical attempt. These objectives were nevertheless objectives of an emerging class.
4. Thus, the Newtonian tradition, that is the chemical affinities dominated and continued as a main stream as has been traced in the work of Psalidas and in the comments in all the translations, such as those of A. Fourcroy and Adet.

At a philosophical level, the Greek scholars in the 18<sup>th</sup> century were attached to or had adopted the empirical philosophy, of Lock and Kant.<sup>28</sup> In this context they tried to achieve their political and scientific purposes:

1. As it is traced in the works of Koumas and Filippidis they used the Lavoisiers’ chemical nomenclature as a paradigmatic language for learning. They adopted the driving principle of the chemical language and rather than the questioning of its epistemological or conceptual requirements. It seems that the

main goal of the Greek scholars was the creation of ideological mechanisms of education. Thus, the Filippidis' approach to chemistry, was based on his empiricist - philosophical considerations and emphasised the perfect structure of the chemical language, aiming, in general, to the adoption of a new learning language. In other words, he expressed, as it has been reported by Masere and Balibar, the necessity of the upgrading class for a "*cultural revolution*" reducing education to "*the main paper of the ideological superstructure seeking the reproduction of its hegemony*".<sup>29</sup> It was not accidental, that the narrations of Filippidis' mainly meet the educational proposal of Lavoisier. The comment on the use of language as a "*tool of learning*" by Filippidi in the preface of the translation of "Logic" of Condillac,<sup>30</sup> posed first time in Greece the most important of the question, not what dialect should be proposed as common national language, but what language can function as a "*tool of learning*" in an education free from the tyranny of scholasticism.

2. The compatibility of their political purposes with the scientific or educational purposes it is traced in the case of Filippidis,<sup>31</sup> in the context of his Lockean empiricism, where it seems possible the incorporation of Lavoisier's thinking in the speculation of chemical affinities.

Moreover the scholars seem not to be interested in the methodological shifts, another perception as the one that instigated Lavoisier emanating from Condillac, about the knowing subject and prompted him into theoretical constitution of modern chemistry.<sup>32</sup>

## References

1. For a detailed study see C.A. Lertora Mendoza, E. Nicolaidis & J. Vandermissen, (eds.) *The spread of the Scientific Revolution in the European Periphery, Latin America and East Asia*, (Brepols Publishers [Collection of Works of the International Academy of the History of Science. Proceedings of the XXth International Congress of History of Science (Liege, 20-26 July 1997), vol. V., K. Gavroglou, "Sciences in the Neohellenic Enlightenment and their problems of interpretation", *Νεύσις* 3 (1995): 75-86, D. Dialetis, K. Gavroglou & M. Patiniotis, "Sciences in the Greek Speaking Regions During the Seventeenth and Eighteenth Centuries. The Process of Appropriation and the Dynamics of Reception and Resistance" in *The Sciences in the European Periphery During the Enlightenment*, ed. K. Gavroglou (Kluwer Academic Publishers, Dordrecht, 1999) and M. Patiniotis, "Textbooks at the Crossroads: Scientific and Philosophical Textbooks in 18<sup>th</sup> Century Greek Education," *Science and Education*, 15 (2006), 801-822.
2. John McEvoy, "Priestley responds to Lavoisier's nomenclature- Language, Liberty and Chemistry in the English Enlightenment" in "*Lavoisier in European Context*" ed. B. Bensaude-Vincent and F. Abbri (Watson Publishing International, USA, 1995).
3. Louis Althusser, *Positions* (in Greek), (Athens, Themelio, 1977).

4. John McEvoy, "In search of the chemical revolution: interpretive strategies in the history of chemistry," *Foundations of Chemistry*, 2 (2000), 47-73
5. Bernadette Bensaude-Vincent, "Textbooks on the Map of Sciences Studies", *Science and Education*, 15(2006), 667-670, on 670.
6. Bensaude-Vincent, "Textbooks on the Map of Sciences Studies", 669.
7. E. Bokaris and V. Koutalis, The "system of Chymists" and the "Newtonian dream" in Greek speaking communities in the 17<sup>th</sup>-18<sup>th</sup> centuries, *Science and Education*, in press.
8. M. Anthrakis, "Philosophical Notebook" manuscript in the Greek National Library (end of 18<sup>th</sup> century), number 1331.
9. K. Petsios, "Methodios Anthrakitis: Reconstruction on His Works and Biography. An unknown Philosophical Notebook: The Physics", in the volume *"The scholarship of the Eastern Zagori"*, Ioannina (2002), 69-120.
10. V. Damodos, "Causal Physiology" ms number 2342 Greek National Library (1754), 1-179b.
11. Bobou-Stamati, "Damodos" (Athens 1998), on 129-130.
12. Th. Kavalliotis, "Treatise on Physics", ms in the Library of Siatista, (terminus ante quem 1755)
13. E. Kekridis, "Theodoros A. Kavalliotis (1718-1789)", (Kavala-Greece, 1991), pp. 111-114.
14. K. Petsios, *The Discussion "On the Nature" in Modern Greek Thought: Aspects of the Philosophical Investigation from the 15<sup>th</sup> to the 19<sup>th</sup> Century* (in Greek, Ioannina, 2002), 261-270.
15. V. Damodos, "Causal Physiology" 3a, 77a.
16. Th. Kavalliotis, "Treatise on Physics", 8.
17. V. Damodos, "Causal Physiology" 35b-36a.
18. N. Theotokis, "Stichia Physikis" (in Greek, printed by Vreitkopf, Vienna, 1766), vol. I, on 3-20, §§1-11
19. E. Voulgaris, "The Favorites of Philosophers", (in Greek, printed by George Vendotis, Venice, 1805) on 90, 284-286, 292, 356.
20. E. Voulgaris, "Elements of Metaphysics", (in Greek, printed by Nikolaos Glykis, Venice, 1805), on 134-168.
21. A. Psalidas, "Peri Physikis en Geni" ("On Physics in General") manuscript, number 4 at the library of Archimandrite's church in Ioannina (~1795), 1a-82b.
22. For the "Newtonian dream" for chemistry see Isabelle Stengers, "Ambiguous Affinity: The Newtonian Dream of Chemistry in the eighteenth Century", in Michel Serres (ed.), *A History of Scientific Thought. Elements of a History of Science*, Oxford, Cambridge Mass. 1995, 372-400; Alistair Duncan, *Laws and Order in Eighteenth-Century Chemistry*, Oxford 1996.
23. Bernadette Bensaude-Vincent, "A view of the Chemical Revolution through Contemporary Textbooks: Lavoisier, Fourcroy and Chaptal", *British Journal for the History of Science* 23 (1990), 435-460.
24. A. Gazis, "Preface" in the translation (in Greek by Iliadis, Vienna, printed by Frantz Antonios Shraimvl, 1802) of the "Philosophie Chimique ou verites fondamentales de la chimie moderne, disposees dans un nouvel ordre, (1792)" of A. Fourcroy entitled "Chemical Philosophy by Fourcroy".
25. Demetrios-Daniel Fillipidis, "Preface" in the translation (in Greek) of "Elements or Physico-chemical Principles" of Brisson, manuscript number 10, Library of Milees (1801).
26. Antoine-Laurent Lavoisier, *Oeuvres*, I-VI, (Imprimerie Nationale, Paris, 1863-92) on 518, 522 and 527.

27. K. Koumas, "*The Translator to the Greeks*", in the translation by Koumas of Adet's work: "*Lecons Elementaires de Chimie, a l' usage des Lycees*" by the heading "*Chemistry Epitome*", (printed by George Vendotis, Venice, 1808).
28. P. Kondylis, "*The Greek Enlightenment*" (Themelio, Athens, 2000).
29. Masere and Balibar, "*A materialistic approach of Literature and Language*", Transl. K. Mitsotaki, (Agonas, Athens, 1981).
30. "*Logic or the First Growths of the Art of Thinking*". *Elementary treatise written by Condillak and translated into Greek by the monk Daniel Dimitrios Filippidis from the town of Milies of Pilion, were added notes, a concise Report of mechanism of reason, and a speech about Schools. Firstly published under the care of Archimandrite Anthimos Gazis in Vienna of Austria* and printed by Frantz Antonios Shraimvl.
31. In a forthcoming paper.
32. John McEvoy, "Priestley responds to Lavoisier's nomenclature...", 130.





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## Aspects of John Hyacinth de Magellan's Scientific Network Between Britain, Flanders and France

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The former Augustinian monk and abbot, the Portuguese João Jacinto de Magalhães,<sup>1</sup> later usually known as John Hyacinth de Magellan, had during his lifetime a vast network of scientific and philosophical correspondents throughout Europe and America. The knowledge of his activities is mainly available in the period after he obtained a secularisation brief from the Pope, probably during 1758. In his early days it is known that he travelled abroad and visited several European countries, staying for some time in Paris where he joined the circle of friends of the Portuguese physician Ribeiro Sanches and was also introduced to the Parisian astronomers' circle. In late 1763, he moved to London and stayed there till the end of his days, although he travelled frequently to the Continent as is clear from his correspondence.<sup>2</sup> His scientific interests, namely those connected with instruments (barometers, octants and sextants) were soon evident also in England where he soon became recognised for his knowledge. By 1770 he became a member of the Royal Society of Arts, and after other academies/ societies such as the Académie Royale des Sciences in Paris, of which he became a corresponding member, and the Royal Society in London. Many important names were Magellan's proposers of Magellan's election to the Royal Society, namely Mathieu Maty, Benjamin Franklin, Joseph Priestley, William Hunter and Joseph Banks.<sup>3</sup>

Magellan's surviving correspondence and publications are mainly from the seventies and eighties. One can distinguish different levels of networks when considering the main affairs with which Magellan's interlocutors dealt with: manufacturers; instrumentalists; mathematicians; astronomers; physicians; chemists; politicians, high dignities; philosophers in general. His scientific communications were also be made at distinct levels, institutionally or in a semi-private way (personal laboratories of dignities or else), in coffee houses / clubs, instrumentalists workshops.

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Magellan established correspondence with a very large number of people and deals with a diverse list of topics, fundamentally concerned with science, technology and medical subjects and on the most recent developments and/or publications that appeared in England or elsewhere that could be of interest to his correspondents, friends and acquaintances or permit exchange of ideas and discussion with them.

### **Magellan's correspondence with regard to Priestley's discoveries**

In the present paper particular attention is paid of some of his correspondence concerned with Priestley's discoveries on gases and its communication in Flanders, France and Holland at Priestley's request.

With regard to Flanders he was in contact with the Duke of Arenberg, Leuven University members (Thijsbaert), Academicians of the Brussels Imperial and Royal Academy of Sciences (Needham, Marcy, Chevalier, Mann), official and governmental bodies (Tackoen, Crumpipen), individuals / astronomers (Pigott), clergymen (Needham, Marcy, Mann, Chevalier). In Holland, he was in touch with some Professors (Gaubius, Allamand, van Swinden, Ingenhousz, van Marum), with dignities (Gallitzin, others) as well as with merchants and what were termed at the time "curious people". When considering France it is impossible not to mention Lavoisier, but also Trudaine de Montigny, Pierre Macquer, Ribeiro Sanches, among several others.

During the seventies, Priestley developed several experimental studies on gases and their nature as in that way it would be possible to examine them in a closer manner towards their primitive elements. Concerning Magellan, Priestley wrote:

*"My friend M. Magellan who makes several trips to the Continent; who maintains there a very large correspondence, so that he is well acquainted with the recent physicists' works; and who committed himself with the mission of initialising several skilled foreign physicists in the practice of experiments of this type,<sup>4</sup> gave me advice that many others persons, of whom the public still doesn't know their names, are at the moment very much occupied with the same subject. It is a domain so fertile that one cannot doubt that in these circumstances, many of the difficulties I have left without a solution will be soon solved ...".<sup>5</sup>*

As Magellan wrote, Priestley asked him to communicate abroad his experiments. So in several letters Magellan gave details of Priestley's work and those of other philosophers concerning the 'airs', and established a chain of discussion that

involved personalities in France, Flanders, Holland, and so on. Carbon dioxide (fixed air) proved to be a very interesting topic not only because of its recent discovery and the analysis of its physical and chemical properties, but mainly because to some of them seemed to be peculiar in regard to organic preservation, and its use in medicine.

Turning back to the several spheres from which one can observe Magellan's activities it is possible to verify that he maintained good relations with some of leading figures of society, mainly through their common philosophical interests or tolerance on them and also after he entered the British and French learned societies.

Magellan's renowned expertise, his contacts and a natural propensity to maintain them enabled his implementation of a vast list of correspondents and the use of some privileged diplomatic mail systems to speed up his letters and parcels of books, instruments, even some small novelties useful for some of his friends, to the seeds and plants from Siberia, America or Portugal for production elsewhere, or for the improvement of botanical gardens. The mail sent through Trudaine de Montigny (1733-1777) facilitated access to Pierre Macquer,<sup>6</sup> Lavoisier,<sup>7</sup> Le Bègue du Presle, Messier, Sanches,<sup>8</sup> etc.

The topics on chemistry and pneumatics began to appear in Magellan's correspondence in the early seventies (according to the remaining letters). On 20th March 1771, Magellan wrote to Macquer (1718-1784), on some experiments made by Scheele on 'fluores' (silicon fluoride) that produced real quartz. At the same time, he informed that at the Royal Society they were continuing reading Priestley's excellent memoir on fixed and mephitic airs and also another one on the application of fixed air on a patient with a putrid fever and the way they did it.

Other messages were exchanged with Macquer, namely on the elastic gum that he was trying to obtain from Portuguese correspondents in Brazil. Magellan foresaw some good uses for it in the areas of mechanics as well as on arts and crafts and considers that Macquer will be able to find out what its nature is (4 May 1771). In this same long letter he told him that the English edition of Macquer's *Dictionnaire de Chimie* was being produced and that he was very curious to read the French second edition, Magellan knows is being prepared in Paris. Later, in that same year, he gives information on some new interesting experiments that may be repeated and studied by Macquer. They were concerned with recent experiments made by 'M<sup>r</sup>. Mayer, apothecary in Osnabruck', 'D<sup>r</sup>. Black from Edinburgh'; and 'M<sup>r</sup>. Rousseau in Muncken, Baviere', all them concerned with a special 'air', Black entitled 'fixed air'. Magellan also mentioned two booklets,

recently appeared in Wien, one by Jacquin (1769) and another by Crantz (1770), he offered to send to Macquer in case they do not exist in Paris. In addition he reported on the experiments made by Cavendish and Lane, just published in the *Philosophical Transactions* (25 October 1771).

Two letters were sent in the same period, to Trudaine de Montigny, dated 5 and 7 July 1772.<sup>9</sup> Magellan said to Trudaine that the recent studies on fixed air had become of public interest and that he thinks one of his letters should be printed and circulated broadly in France. And proceeds, mentioning the experiments made by Sir John Pringle about fixed air properties against putrefaction, on Macbride's interpretation of its properties and the use of 'beer must' as an anti-septic to preserve against and cure scurvy as experimented upon and confirmed in the British vessels. Black's experiments with calciferous stones and substances that also possess a large amount of fixed air are also referred to as well as the discoveries of Dr. Brownrigg on the waters, such as those of Pyrmont and similar, whose special acidic taste was according to him due to that same air. Magellan further pursued his detailed descriptions, referring Priestley's researches presented at the Royal Society namely on that same air, on the mephitic air and on the inflammable air. That Priestley had discovered a way to impregnate common water with fixed air, and confirmed similar qualities previously identified in the 'beer must' and some mineral waters. He proceeded, adding that while reading Priestley's booklet carefully he thought he could make it easier to be used either on board of a vessel, or on earth. That he then tried it and made many tests before several friends who found it easier and more manageable than the previous method, although not that different in essence. Magellan next gave a detailed description of Priestley's booklet.

By the end of 1772, one of Magellan's correspondents in Brussels was the astronomer Nathaniel Pigott<sup>10</sup> who was undertaking a precise geographical determination of the locations of the main towns in the Low Countries. Magellan provided him with several instruments and news he should also share with other friends in common and acquaintances in Flanders. By middle January of 1774, he was giving him notice of the Gold Medal awarded to Priestley because of his discoveries on gases. And that he was sending Pigott a copy of the speech made at the occasion by Sir John Pringle (18 January 1774).

Some days later, he details Priestley's air discoveries in a letter to Thijsbaert,<sup>11</sup> the Professor of Philosophy in Leuven. This time the subject was concerned with the discovery of nitrous air and its properties. Magellan wishes that Thijsbaert perform those experiments on gases together with his colleagues, considering that it will be easy to use Priestley's apparatus and that it will be amazing to observe

the “*action of nitrous air on common air*” (25 January 1774). The same day, he wrote to his French correspondent at the Académie Royale des Sciences, Gabriel de Bory, about the presentation at the Royal Society of Lavoisier’s memoir *Sur les emanations elastiques qui se degagent de differents Corps*. Magellan complained that he would like to see Lavoisier performing those important experiments, and referred his desire of possessing Lavoisier’s book, whose title enables him to think that a systematic treatment based on experiments will be presented. Meanwhile he detailed two new experimental improvements Priestley had achieved, one on the decomposition of air from its acid using the electric fire and the other one on the nitrous air and the way he proposed to prove the quality of air with it. Magellan mentioned their publication in 1772 in the *Philosophical Transactions*, adding that the experiment was astonishing. Then he left some questions to be solved by philosophers concerned with the results observed, namely on the disappearing of volumetric quantities and the conjectures on what happened (25 January 1774).

Still on the same day, Magellan wrote to Pigott on several subjects and again on airs and the speech made at Priestley’s award, and will send the letter together with a box of fresh seeds from North American trees he wishes to offer to the Duke of Arenberg for his plant nursery in Edingen.

The improvements attained by Priestley, above mentioned, are again introduced with a request that they will be presented at the Brussels Academy of Sciences, as well as to Needham, the Duke of Arenberg, the British plenipotentiary minister, Gordon, and other common friends (25 January 1774).

Magellan’s correspondence direct with Lavoisier also began in 1774, as far as can be seen. In that same year Magellan told Lavoisier of his great satisfaction with Lavoisier’s book (*Sur les emanations elastiques...*) and also as in the note he wrote on Venel, where he gave a clear view on the merits that should be attributed to those who contributed to the identification of fixed air in the acidic waters, much in accordance with Magellan’s opinion. He added, that he almost quarrelled with Rozier as he considered Venel to be the inventor of ‘fixed air’ whilst Venel was just using Stephen Hales’ interpretation (11 February 1774).

As already mentioned, sometimes Magellan used the mediation of other friends to circulate novelties (books or others) and this happened also with Lavoisier to whom he sent on May 1774 Priestley’s last publication, the first volume on the “Different kinds of airs”. There, Lavoisier could already see the citation to his own work on the subject, with the merits stated. This time the messenger was the Irish chemist, Peter Woulfe, another Magellan’s close friends (16 May 1774).

The circulation and spreading of knowledge by Magellan is again evident when he mentions to his friend Sanches in Paris that he had accepted the invitation to stay some few days with Priestley and Lord Shelburne where they would speak about 'fixed air', that Priestley's second volume on airs is being printed, and that new experiments and results were coming out. At the same time told him about the last meeting at the Royal Society where some experiments on animals heat had been read introducing some corrections to Boerhaave's conclusions on the heat that can be supported by men and animals without damage (7 July 1775).

He wrote to Lavoisier, communicating that Priestley had offered him the second volume of *Experiments and Observations on Different Kinds of Air*, before its public distribution, as a gift from the author, in order that Magellan could profit from a "*Mail that was sent at M. Trudaine's address in Paris*" where Duchesne (or Messier) would deliver it to Lavoisier. Magellan also told that he could not find the two books on "*Nitre*" that Lavoisier requested from him, through his wife (1775<sup>12</sup>).

Magellan's connections with Flanders were also flourishing in this period as referred to above. Several instruments for experimental physics<sup>13</sup> (which included the study of gases) and astronomy were acquired in England through Magellan's advice. And at the new Academy of Sciences in Brussels some discussions were held, some connected with Magellan and others on the recent philosophical subjects.

Magellan's friendly relationship with the 5<sup>th</sup> and 6<sup>th</sup> Dukes of Arenberg, and staying at their palace in Brussels and other residences, opened the doors to new friends and acquaintances. In their palaces, he could perform several experimental sessions on airs and electricity, all before those interested in philosophical matters rather than in the futilities of social life. The news on the experiments on fixed air and others, and on the achievements of the second Cook's voyage to the Southern hemisphere, were also given in detailed accounts to the Duke of Arenberg.

In an early letter to Thijsbaert, Magellan informed that very soon he would send him Priestley's second volume on airs that appeared in 1775, and which contained several interesting novelties related with oxygen (dephlogisticated air)<sup>14</sup> and the process of respiration.

On October 1776, Magellan sent a letter to his friend Sanches, giving him notice of his trip to the Netherlands and Flanders while accompanying his friend Arbuthnot and his sons. Magellan acted as a tutor of Arbuthnot's boys, namely in the Portuguese language in order they could later be sent to the Navy and Eastern India commerce. Magellan reported that he went to Amsterdam he was



very well received by several curious individuals of experimental physics, one of them being the uncle of the Arbuthnot boys. After that he had gone to Haarlem to visit Dr. van Marum and his new electrical machine, and that two days later he was in Utrecht where he made acquaintance with *“several professors of whom I received a very honourable treatment”*. Then he left for Den Haag where he dined with the Russian ambassador, Prince Gallitzin (1738-1803), who asked him to stay a few days to meet the Leiden Professors Allamand (1713-1787) and Gaubius (1705-1780) who desired to see the recent experiments on airs. Magellan stayed in Leiden for a few days where Allamand received him at the University and the experimental meeting was then arranged with Gaubius and Allamand at Gallitzin's palace, in Den Haag, where Magellan performed several experiments on the different kinds of airs, following Priestley's method.

Magellan's Dutch trip did not end in Leiden as after he went to Rotterdam, and then to Antwerp and Mechelen in Flanders, where a friend, Mr. G. Tackoen, who was counsellor at the Great Council of Mechelen (Brabant), had invited him. From there travelled to Brussels and Leuven where Thijsbaert was also waiting for him (31 October 1776) and for whom he performed, during two days, several experiments on airs that would be used in his physics course (15 November 1776).

The same time Magellan mentioned to Sanches that at that moment (November) he was staying at Prince's d'Arenberg palace in Brussels and comments on the satisfaction and astonishment reaction of Professors Allamand and Gaubius about the experiments he performed before them and their desire of maintaining contact with him. Magellan continued, telling Sanches that in Brussels he also arranged and performed the main experiments on airs in presence of Needham, Director of the Royal Academy, its secretary as well as others. And manifests his hope that *“they will be convinced on the importance of this new branch of experimental Physics”* (15 November 1776).

This enlarged trip and his achievements concerning pneumatics and other philosophical subjects was again the object of a letter to Priestley where Magellan mentioned his visit to Allamand and the performance of Priestley's experiments before him, Gaubius and Gallitzin (30 November 1776). At the same time, Magellan told Priestley that soon he would inform him personally on some improvements he made on the glass to impregnate water with fixed air that take much less time as well as on two new eudiometers to test air salubrity.

As already mentioned, the interesting and very recent experiments that were being made and interpreted during this decade concerned with gases and others,

were well circulated in Magellan's network. For instance, to Thijsbaert on 25 February 1777, he said:

*"... I have just sent you by means of our good friend M. Needham, a small parcel in a box that I have sent him to Brussels with the glasses for the experiments on different kinds of air. It contains a small phial with copper crystals and a box with two dozens of pith balls: they will enable you with two new experiments for your lectures, which are pleasant & new ..."*

And continued, saying that he was tempted to send him some more delicate glasses with S-shaped tubes, as may be, he had already broken them while performing the experiments on airs. In that same letter informs he will send Thijsbaert his book on the glass-apparatus for making mineral waters and the one on the new eudiometers, together with one of those instruments (25 February 1777).

## Conclusion

Detailed evidence has been presented on J. H. de Magellan, as a disseminator and important vehicle in the introduction of Priestley's discoveries on gases in Flanders, France and Holland.

The protagonists in the debates on the new gases were leading figures of chemistry such as Priestley and Lavoisier, although the focus herein has not fixed on them, but rather on the communication network established through Magellan with some other secondary figures that were significantly important in the appreciation and discussion of the new evolving topics of chemistry and their acceptance, namely because of their public utility. Magellan's interest in different philosophical matters allied with his natural propensity to communicate favoured this kind of commitment and the circulation of knowledge. The circulation of which enabled others to gather and improve chemistry from the discussions that took place. It is also evident that natural philosophy was being developed through a great emphasis put on experiments as well as by Magellan's idealism. It is considered that some of the above mentioned experiences were really new, in France, Flanders and Holland and that after the initial communications with Magellan, some of his interlocutors joined the discussions and the studies of gases.

In addition to the progress facilitated in the science, the scientific connections with Flanders were improved and this allowed Magellan to repay a debt of gratitude to the land that had received him as a member not only of its Imperial

Academy but also as a member of Soignies' Chapter with the benefits that those bodies conveyed.<sup>15</sup>

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## References and foot notes

<sup>1</sup> Aveiro, 4 November 1722, London/Islington, 7 February 1790.

An early study on Magellan's life and work was developed by Isabel M. C. de O. Malaquias, in, *A obra de João Jacinto de Magalhães no contexto da ciência do séc. XVIII* (Aveiro: Universidade de Aveiro, 1994).

R. W. Home's entrance on *Jean Hyacinthe de Magellan* to the *Oxford Dictionary of National Biography*, (Oxford: Oxford University Press, 2004) Vol. 37: 115-117.

<sup>2</sup> The publication of all the extant correspondence of João Jacinto de Magalhães is being prepared by Rod W. Home, Manuel F. Thomaz and Isabel Malaquias and we hope it will soon be available.

<sup>3</sup> He became member of about ten prestigious societies / academies: Royal Society of Arts in 1770; Académie Royale des Sciences in 1771; Royal Society in 1774; Academiae Scientiarum Imperialis Petropolitanae in 1778; Academia Ciências de Lisboa in 1779; American Philosophical Society of Philadelphia in 1784; Hollandsche Maatschappij der Wetenschappen (Haarlem) in 1784; Koninklijke Academie voor Wetenschappen (Brussels) in 1785; Akademie der Wissenschaften (Berlin) in 1786; Real Academia de Ciencias Exactas, Físicas Naturales de Madrid.

<sup>4</sup> Pneumatics was the subject.

<sup>5</sup> Joseph Priestley, *Expériences et observations sur différentes Espèces d'Air*. Ouvrage traduit de l'Anglois de M. J. Priestley, ... (Paris, 1780), Tome IV, Préface de l'Auteur, on xj-xij.

<sup>6</sup> Letters to Pierre Macquer at the Archives Nationales.

<sup>7</sup> I.M. Malaquias "A comunicação científica entre João Jacinto de Magalhães e Antoine-Laurent Lavoisier", *Química - Boletim da Sociedade Portuguesa de Química*, n°52 (1994): 24-29.

Letters to Lavoisier at Archives de l'Académie des Sciences.

<sup>8</sup> Letters to Sanches at the Österreichische Nationalbibliothek.

<sup>9</sup> Letters to Trudaine at the Archives de l'Académie des Sciences.

<sup>10</sup> Letters to Pigott at the Royal Astronomical Society.

<sup>11</sup> Letters to Thijsbaert at Liège University Library.

<sup>12</sup> Letter without a complete specification of date.

<sup>13</sup> Geert Vanpaemel, *Eccho's van een wetenschappelijke revolutie. De mechanistische natuurwetenschap aan de Leuvense Artesfaculteit (1650-1797)*, (Paleis der Academien, Brussel, 1986) Jaargang 48, Nr. 173, on 151, 156.

Geert Vanpaemel, "Jan Frans Thijsbaert (1736-1825) en de School voor Experimentele Fysika te Leuven", *Tsch. Gesch. Gnk. Natuurw. Wisk. Techn.* 7, 4 (1984): 172-182, on 179, 181, 182.

Paul Bockstaele, "Experimental Physics at the old University of Leuven", *João Jacinto de Magalhães Conference* (Coimbra: Museu e Departamento de Física da Universidade de Coimbra, 1990), 99-111, on 107, 108.

Malaquias, *A obra*, Aveiro, 1994, on 75, 79, 135.

André Cresens, "Het laboratorium" in *De blinde hertog, Louis Engelbert van Arenberg & zijn tijd 1750-1820*, Derez, Dillemans, Duvosquel, Roegiers, Tytgat, Verbrugge (Gemeentekrediet, 1996), on 183-192.

<sup>14</sup> "In 1775 Priestley wrote letters dated 15 March (to Sir John Pringle), 1 April (to Dr. Price), and 24 May (to Sir John Pringle, misdated 25 May in the printed paper), which were later printed in the *Philosophical Transactions* as "Account of further Discoveries in Air", dealing with dephlogisticated air, vitriolic acid air (sulphur dioxide), nitrous air, and vegetable acid air (acetic acid vapour). The 1<sup>st</sup> letter, on dephlogisticated air, was read to the Royal Society on 23 March. (...)". In J. R. Partington, *A History of Chemistry*, (Mansfield: Martino Fine Books, 1996 repr) vol. 3, on 256. "In October 1774, Priestley visited Paris with Lord Shelburne and told Lavoisier at dinner of his discovery of dephlogisticated air". In Partington, *A History* vol. 3, 402.

<sup>15</sup> Ame Demeuldre, *Le Chapitre de Saint-Vincent à Soignies - ses dignitaires et ses chanoines*, (Soignies: Imprimerie Felix Noefnet, 1902), on 161-162.

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## Neighbours and Territories: What Do Creativity, Intelligence and Responsibility Have in Common? or Historical and current considerations about the socio-political responsibility of Science

*Helmut Ringsdorf\**

At scientific meetings and academic jubilees, results and achievements are always praised but responsibility is seldom discussed. Nevertheless, we know that to be a sound scientist requires more than being able to deliver research results and to teach the subject. In this respect also history of Science has to be more than the description of facts from yesterday. Especially those of us located in central Europe should know something about the traps of over-emphasising scientific achievements [1-3]. Fritz Haber [4], Adolf Butenandt [3], Richard Kuhn [5] and even Hermann Staudinger [6] are historically interesting scientists, and they were active in politically extremely difficult times.

Although the autonomy of our universities in Europe is no longer endangered in a political sense, they exist today in a turbulent atmosphere, driven by absurd saving plans, sometimes bizarre elitism and influenced by evaluation games and over-bureaucracy. And there is one more problem, surely at the heart of the matter: It is difficult for our universities to avoid being pulled into the nowadays only profit-driven speculations of the overdeveloped Neo-liberalism (Milton Friedman, Nobel Prize for Economy in 1976) with the sometimes brutally acting stock market as its accomplice. The “absolute open-market economy” and “laissez-faire capitalism” regard research and responsibility only as a money oriented short term amusement for our Fun-Society [7]. Certainly competition and world-wide activities do change our local situations. But many negative economic developments [8-10] cannot simply be justified by the slogan of globalization [11, 12] (Joseph Stiglitz, Nobel Prize for Economy in 2001).

What is the intrinsic value and meaning of knowledge? This question is nowadays often replaced by the question of what type of knowledge do we need to fulfil pre-determined functions. This increasing misuse of Science –i.e. its transformation to

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mainly develop and support technology often for purely stock market effects—threatens to destroy its critical, purely truth-oriented function. The increasing connection between KNOWLEDGE and INTEREST—first discussed socio-critically by the German philosopher Jürgen Habermas— [13, 14] seems to have become the norm to such an extent that the value of Science is endangered to vanish in goals and reasons defined outside science: “Truth is what is useful”. Does this not demand a response from our universities and research institutions?

Where is all this taking us? We are in a transition state: In Europe we are under pressure to restructure our shaking Industrial Societies into Knowledge Based Societies! Because we cannot keep the basic industrial production in our developed countries, knowledge, originality, and richness of ideas are more in demand than ever for further developments. Thus, we need a science education system that is able to nurture creativity, and an uncomplicated fast and open exchange of scientific and technological aspects with industry. We cannot allow our universities to be instrumentalized: Neither politically—as in the Third Reich— nor now political-economically, e.g. by “laissez-faire capitalism”. We are all responsible together for what is to come [15].

What is to come? “The best way to predict the future is to invent it” [16]: Independent universities and research institution are a prerequisite for the education of creative, courageous, non-aligned scientists, willing to accept their responsibility as citizens and as professionals [17].

## References

- [1] E.Y. Hartshorne, *The German Universities and National Socialism* (London: Unwin Ltd., 1937).
- [2] Ute Deichmann, *Angew. Chem. Int. Ed.* 41(2002): 1310-1328.
- [3] Doris Kaufmann, *Das Blut von Auschwitz und das Schweigen der Gelehrten. Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus. Bestandsaufnahme und Perspektiven der Forschung* (Göttingen: Wallstein Verlag, 2000), vol. 1, 189-226 – Two volumes ordered by the Max-Planck-Society.
- [4] Roald Hoffmann, P. Laszlo, “Coping with Fritz Haber’s Somber Literary Shadow”; *Angew. Chem. Int. Ed.* 40 (2001): 4599-4604.
- [5] Lothar Jaenicke, Richard Kuhn, “Ein Talent doch kein Charakter“, *Nachrichten aus der Chemie* (2006): 54.
- [6] Helmut Ringsdorf, „Hermann Staudinger and the Future of Polymer Research: Jubilees—Beloved Occasions for Cultural Piety“, *Angew. Chem. Int. Ed.* 43 (2004): 1064-1076.
- [7] Klaus Hofmeister, L. Banerochse, *Machtworte des Zeitgeistes* (Würzburg: Echter Verlag, 2001).
- [8] N. Chomsky, *Profit over People* (Hamburg: Europa Verlag, 2001, 6<sup>th</sup> Edition).

- [9] a) G. Soros, *Die Vorherrschaft der USA – eine Seifenblase* (K. Plessing Verlag, 2004, 2<sup>nd</sup> Edition).
- b) G. Soros, *The Bubble of American Supremacy* (New York: Public Affairs, 2003).
- [10] G.D. Deans, F. Kröger, St. Zeisel, *Merger Endgames – Strategien für die Konsolidierungswelle* (Wiesbaden: Gabler Verlag, 2002).
- [11] a) Joseph Stiglitz, *Globalization and its Discontents* (New York: W.W. Norton Comp., 2002).
- b) Joseph Stiglitz, *Der Schatten der Globalisierung* (Berlin: Siedler Verlag, 2002).
- [12] Joseph Stiglitz, *Die Chancen der Globalisierung* (München: Siedler Verlag, 2006).
- [13] Jürgen Habermas, *Technik und Wissenschaft als Ideologie* (Frankfurt: Edition Suhrkamp, 1968).
- [14] Jürgen Habermas, *Erkenntnis und Interesse* (Frankfurt: Edition Suhrkamp, 1973).
- [15] Karl Popper, *Alles Leben ist Problemlösen. Über Erkennen, Geschichte und Politik* (München: Piper Verlag, 1999).
- [16] *A quote of Richard Feynman*, Physic Nobel Prize 1965.
- [17] But isn't every professional a responsible citizen anyhow? Yes, but see the beginning of this abstract! Too often we scientists exchange our "citizen jacket" against our "lab-coat" as soon as we enter our laboratories and offices.





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## Scientific Communication During a Major Change in the Approach to Empirical Research: *Annales de chimie* vs *Observations sur la physique*/*Journal de physique* (1789-1803)\*

Angela Bandinelli\*\*

The founding of several scientific journals during the second half of the 18<sup>th</sup> century spawned interesting controversies on chemical issues. In the *Observations sur la physique*, in particular, it is possible to scrutinize in detail the evolution of important discussions concerning crucial questions: “Inflammation or Combustion?”, “Pure Air or Oxygen Gas?”, “Fire or Caloric?”. In the *Annales de chimie*, the discussions were inspired by other questions such as “Simple or Compound substances?”.

Some of the specific comments and questions raised by the participants involved were as follows: “In order to stop all these philosophical debates dividing chemists, how is it possible to define combustion?”;<sup>1</sup> “Should we consider the word *combustion* according to the meaning proposed by M. Arejula in his well done Memoir published in the issue of October of the present year? And, consequently, for example, considering *combustion* the combination of pure air with nitrous air, or, as we have already said, the combination of pure air during respiration, etc. I don’t think so”.<sup>2</sup> “Unfortunately gentlemen, in order to support a system, you wanted to change the overall language. [...]. Let’s take, for instance, the pure air, first of all, Priestley has called it dephlogisticated air; Scheele, fire air; Bergman, pure air; Turgot, vital air, the new nomenclature called it oxygen gas”.<sup>3</sup> “Is the combined or isolated caloric distinct from the matter of fire and of light?”.<sup>4</sup> “Besides, this distinction among *un-decomposed substances* and *simple* or *elementary substances* should have been explained. Maybe is it another distinction of convenience?”.<sup>5</sup>

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The evolution of all these debates helps us understand how the panorama within the scientific community gradually changed. Thanks to the reconstruction of the ongoing disputes between the two journals, we can understand how a growing number of chemists, physicists and naturalists, followed the developments in experimental chemistry, decided to adopt the principles of the Lavoisierian analysis.

Furthermore, the availability of new data on the constitution of bodies, both organic and inorganic, had significant effects on the more general domain of the life sciences. At the beginning of the 19<sup>th</sup> century, Jean Baptiste de Lamarck, one of the most active naturalists in the Parisian scientific community, ruled out his own original ideas concerning the applicability of “chemistry” to the study of living beings and accepted the new chemical image of “organized bodies” as natural systems of predictable operations.

### **The Problems of interpreting Laboratory Results**

In 1788, a pro-Lavoisierian memoir by Louis Lefèvre Gineau was published in the Parisian journal *Observations sur la physique*. In his memoir, Gineau’s analysis regarding the experiment of water dripped into an incandescent iron tube differed from the traditional interpretation.<sup>6</sup> Contrary to the opinion of the editor of the journal, Jean-Claude Delamétherie, according to which this experience confirmed the existence of the phlogiston/principle of inflammability in iron,<sup>7</sup> Gineau maintained that it was a further proof that metals were simple bodies.

Although diametrically opposed, both interpretations perfectly justified the iron’s increase in weight: according to the traditional hypothesis, it was caused by water entering the body, thus triggering the expulsion of phlogiston (perfectly in line with the Priestleian perspective presented in the same volume).<sup>8</sup> According to the new theory, the increase in weight of the iron was due to the blending of the metal and the oxygen base produced by the decomposition of water.

Furthermore, according to Gineau it was possible to prove quantitatively that water decomposed into two gaseous constituents.<sup>9</sup> Thus the oxygen component was responsible for oxidizing the metal while the hydrogen component was released in a vapour state. The logical consequence was that the hydrogen came from the water and not, as wrongly believed, from the iron.

This controversy was not, as may appear at first sight, limited to a mere discussion of the cause/effect of the processes at stake. If that were the case, Gineau’s

memoir, though perhaps more detailed and sophisticated, could be considered one more study among others within a substantially equivalent course of investigation. Actually, the comparison emphasized two contemporary and incompatible notions of the chemical art, i.e. two different approaches to laboratory research in relation to the information provided by the senses, as became explicit in later years:<sup>10</sup> “It will be said, how is it possible to reconcile the result of this experiment with that of M. Lavoisier and his friends?”.<sup>11</sup>

### **Divergences and subsequent reconciliations within the European chemical community in the period 1789 to 1803**

In his treatise of 1789 Antoine-Laurent Lavoisier explained his position regarding the phlogiston/hydrogen equivalence theory formulated by the traditional chemists. He sustained that it did not provide any information on the processes really occurring during combustion. Consequently, while Lavoisierian chemists were emphasising the distinction between *presuming* an idea and *proving* it;<sup>12</sup> the traditional chemists were certain that phlogiston existed because its reality was proved by means of the sensory experience (i.e., smell, taste and colour).<sup>13</sup>

In the same year, Lavoisier published a memorable paper on the *Observations* of Delam  therie in which he presented a new theory of “vegetation” that was in dispute with the traditional ones. While Ingen-Housz, Priestley and Cavallo assumed a mere mechanical function of water in contact with the plants, Lavoisierian theory was based on two “facts”: water is a compound of 15 parts hydrogen and 85 parts oxygen; carbon dioxide is a compound of 72 parts of oxygen and 28 parts of carbon.<sup>14</sup>

The inability at the time to determine the real mechanisms of combustion fuelled disagreement within the chemical community. The divergences of opinion grew to the point that, in his preliminary speech for the year 1789, Delam  therie declared a state of “scientific crisis”. In the same year, as the anti-Phlogistonists felt that the editorial policy of the *Observations sur la physique* misrepresented them, the Lavoisierian school founded its own scientific journal: the *Annales de Chimie*, an explicitly anti-Phlogiston journal. This new scientific publication aimed at updating the chemists on the discoveries made in the applied chemistry without omitting the philosophical or general considerations underlying each specialised branch of knowledge, as emphasised in the introduction for the new course in 1797.<sup>15</sup>

The Lavoisierian memoir on combustion of iron stood out among the large number of interesting papers discussed in the first volume of the *Annales*. In his account Lavoisier sustained that in nature, combustion without flames did occur<sup>16</sup>. Thus, he clarified the distinction between ordinary burning and combustion: an issue on which the majority of traditional chemists were confused. The need for accuracy and precision in laboratory practice was emphasised in his study, as it was a means to determine quantities rather than assuming them.

This division gave rise to two different series of debates concerning the combustion of bodies. European scientists involved in these discussions expressed their views in their journal: articles in the *Observations* debated the “presumed” theory of the decomposition of water as well as that of fixed air,<sup>17</sup> whereas accounts in the *Annales* reflected the belief that those same theories were not merely probable but certain, and therefore could lead to further scientific research. Consequently the concerns of the arguments in the *Annales* were entirely different from those in the *Observations*; in addition, other issues were discussed: such as the simple nature of carbon<sup>18</sup> and of nitrogen.<sup>19</sup> Furthermore, the approach to solving the above-mentioned debates differed profoundly: on one hand, the Phlogistonists continued to base their reasoning on old Aristotelian assumptions rather than to accept the new definition of “combustion”.<sup>20</sup> On the other hand, the anti-Phlogistonists refused to consider matters that were impossible to verify and focused on experimental procedures. The Dutch group, for example, devoted itself to the reproduction of several combustion experiments by means of sophisticated apparatus and claimed that many distinguished scientists, including Priestley, Wiegleb and Wurzer, lacked professionalism as they had confused results with accidental products.<sup>21</sup>

Even the anti-phlogiston Christoph Girtanner received harsh criticism from Claude-Louis Berthollet for deriving his conclusions from poorly implemented experiments.<sup>22</sup> On the other hand, the abbot Spallanzani was presented to the European scientific community as “one of the most brilliant Italian naturalists”. It was due to his precise eudiometric experiences that professor Goettling’s thesis against the new system was disproved.<sup>23</sup> In particular, Lazzaro Spallanzani had undertaken specific quantifications that refuted the German thesis that nitrogen gas is an oxygen compound. Spallanzani placed pure gases (nitrogen, hydrogen and carbonic acid) in contact with phosphorus inside a eudiometer of Giobert. The Italian researcher verified that no light was produced thus indicating that phosphorus could not be ignited in the presence of substances other than oxygen. Spallanzani’s investigations also included an analysis of organic substances containing phosphorus, such as those found in fireflies.<sup>24</sup> He concluded that lumines-

cent bodies showed the same characteristics of common phosphorus, with additional interesting peculiarities: the chemical reaction is the same for a live or dead firefly. In particular, when alive the firefly's light shines brighter owing to its respiration. Spallanzani argued that this was the result of a slow combustion of hydrogen gas and "carbonic hydrogen gas", the components of animal and vegetable substances occurring in all luminescent bodies.

Although the discoveries of pneumatic chemistry (that organic matter was composed of carbon, hydrogen, oxygen and nitrogen) enabled further investigation of vital phenomena (a matter that brought to a halt other naturalistic traditions), it was necessary to avoid any chemical reductionism that the growing number of applications of the new system seemed to instigate. This was reflected in a dispute between Antoine-François de Fourcroy and Friedrich Alexander von Humboldt. While the German naturalist thought it was possible to explain vitality as the chemical equilibrium of the organic constituents,<sup>25</sup> Fourcroy considered that "Mr. Humboldt proceeded too quickly in his explanations".<sup>26</sup> Humboldt's reply to Fourcroy came promptly: "We are going on two different roads: while you analyse matter in which the vital principle is extinct, [...] I confine myself to describing phenomena observed in organized matter".<sup>27</sup> The Frenchman's blunt response followed shortly: "In this series of premature conclusions and forced applications, I have understood neither your experiments nor your useful results".<sup>28</sup> In a memoir published the following year on the application of chemistry to medicine, Fourcroy confirmed his position by refusing to accept "the inappropriate explanation of the phenomenon of animal life by means of a chemical force".<sup>29</sup> In fact, immediately following this clash, Humboldt went to Vauquelin and Fourcroy's laboratories in Paris where he sought to gain experience on the new French methodology. Thanks to this training, Humboldt wrote a memoir on the earth's absorption of oxygen that was soon to become famous. This memoir is also the evidence that Humboldt had joined the new way of reasoning in chemistry.<sup>30</sup>

However, the majority of the European naturalists were not as keen to abandon the long-standing philosophy used to set apart living beings from the rest of the physical world. For them, living beings were such by virtue of an unknown organisation of matter, the result of an extra-natural vital principle. In 1794 Jean Baptiste de Lamarck published a work, written eighteen years before, that described natural phenomena, both organic and inorganic, without relying on the recent chemical discoveries.<sup>31</sup> Incidentally, this was the year of Lavoisier's death. The Lamarckian researches, praised by Delamétherie,<sup>32</sup> sustained the illegitimacy of the chemical analysis applied to vital phenomena as these were elusive by

nature. In 1796 Lamarck joined actively the debates in progress by publishing his refusal to accept the pneumatic theory.<sup>33</sup> Lamarck thus confirmed that his researches of 1794 were not an isolated event, but actually the beginning of a coherent project meant to provoke a naturalist reaction against chemistry. Later, in 1799 (when Spallanzani died), Lamarck published a memoir in Delamétherie's journal on the subject of fire, in which he reaffirmed his absolute distrust of chemical analysis.<sup>34</sup>

Once the new century started, Lamarck surprisingly ended his opposition to the Lavoisierian system and, in his researches of 1802 (when the chemical debates were nearly defined in favour of the new doctrine), he silently borrowed the information about organic transformations from chemical analysis thus exhibiting his own obscure conversion.<sup>35</sup>

Unlike Lamarck, the Italian naturalist Spallanzani never attacked the new French methodology. On the contrary, perfectly aware of the limits of traditional technology, he had readily adopted the new method of reasoning/experimenting and applied it to living beings in their diversity. He was thus able to prove that living organisms were regulated by identifiable processes of decomposition/recomposition of material. Spallanzani's scientific manuscripts, currently at the Municipal Library of Reggio Emilia, are of great interest, as they describe his experimental procedures. Parts of the manuscripts were later used by the scientist/librarian Jean Senebier<sup>36</sup> in his edition of Spallanzani's memoirs on respiration. In these laboratory books, the organism is analysed as a natural system open to exchanges of matter with the environment rather than a body endowed with extraordinary qualities that do not exist in inorganic matter.<sup>37</sup> Spallanzani's readiness and courage in following the new methods resulted in valuable contributions of new data to the life sciences and in an innovative approach well ahead of Lamarck's establishment of the "science of living bodies", or *Biologie*.<sup>38</sup>

In the literary news for the year 1803, Delamétherie introduced Spallanzani's memoirs as work full of "well done experiments", praised the new chemical-physiological researches, and underlined that "everything written by Spallanzani's pen is made to interest the scientists".<sup>39</sup> This leads to the conclusion that the Italian scientist had succeeded in the outstanding accomplishment of resolving the conflict of twenty years in favour of the new system. Spallanzani confirmed the Lavoisierian model of organic matter characterized by the oxidative regularities by presenting experimental results that began to be accepted as scientific data. His results appeared in Delamétherie's journal and inaugurated a new way to investigate the living world beyond the hypothesis of an absolute hiatus from the inorganic world.



## Reflections on the late eighteenth-century chemical debates

What is the historical-scientific meaning of the above account of the years between 1789 and 1803? First of all, the examination of the debates recorded in the two journals unquestionably shows a disparity in the process of scientific production in consequence of which every attempt to equate those who produced *accurate data* and those who considered them *probable* is historically inappropriate.

The new doctrine built up knowledge thanks to a modern definition of the concept of scientific “fact”, *i.e. a definite relationship among different terms*, which made it possible to avoid confusing oneself with opinions which were often in contradiction. It also redefined by the end-of-the-century scientific communication by ensuring a previously unimaginable collective understanding.

Following this line of research it became possible to distinguish facts from testimonies, combustions from inflammations, aggregates from compounds. *Theory* and chemical *practice* were reconnected thanks to the adoption of a new analytical method (or procedural method from “known to unknown”). The traditional domain of combined physical-chemical knowledge redefined itself as a unique thermochemical relationship.<sup>40</sup> The scientific debates became independent of literary discussions.<sup>41</sup>

This *ensemble* (complex network) of redefinitions cannot be reduced to an innovative interpretation of the cause/effect relationships at stake during chemical reactions and marks the beginning of a way of reasoning and experimenting in chemistry which leaves out of consideration unquantifiable entities.<sup>42</sup> In this regard, in Lavoisier and Laplace’s memoir on heat (1783) we read:

“Here we will limit ourselves to comparing the amounts of heat that are evolved in combustion and respiration with the corresponding changes in the oxygen, without considering whether that heat comes from the air or from the combustible bodies and the animals that breathe. In order to determine these changes we performed the following experiments”.<sup>43</sup>

## Notes

<sup>1</sup> “Pour terminer tous les débats philosophiques dont la chimie est partagée; comment définir la combustion?” Henry Rebol, “Lettre à M. De La Métherie sur la combustion, le 8 Décembre 1788”, *Observations sur la physique, sur l’histoire naturelle et sur les arts* (Paris: Le Jai, 1773-1793), 34 (1789): 124-126, 124.

<sup>2</sup> “Doit-on donner à ce mot *combustion* toute l’étendue que propose M.Arejula dans son beau Mémoire inséré dans le mois d’octobre de cette année? Et appeler, par exemple, *combustion* la

combinaison de l'air pur et de l'air nitreux, ou comme on avoit déjà dit, la combinaison de l'air pur dans l'acte de la respiration, etc. Je ne le crois pas." Jean-Claude Delamétherie, "Discours préliminaire", *Observations sur la physique* 34 (1789): 3-55, 31.

<sup>3</sup> "Mais malheureusement Messieurs, pour soutenir un système, vous avez voulu changer toute la langue [...]. Prenons par exemple l'air pur: M. Priestley l'appela d'abord air déphlogistiqué, Schéele air du feu, Bergman air pur, Turgot air vital, la Nouvelle nomenclature gaz oxygène." Delamétherie, "Réponse à M. Hassenfratz", *Observations sur la physique* 33 (1788): 385-388, 386.

<sup>4</sup> "Le calorique combiné ou isolé, diffère-t-il de la matière du feu et de la lumière?" Michel Dizé, "Mémoire sur la matière de la chaleur", *Journal de Physique, de chimie, d'histoire naturelle et des arts* (Paris: Cuchet, 1794-1823), 6 (1799): 177-202, 178.

<sup>5</sup> "Au reste, cette distinction de *substances non-décomposées* d'avec les *substances simples* ou *élémentaires*, auroit mérité d'être expliqué. Ne seroit-ce pas encore une distinction de convenance?" Delamétherie, "Réponse à MM. Adet et Hassenfratz sur la Chimie des Pneumatistes, le 21 Février 1787", *Observations sur la physique* 30 (1787): 218-226, 219.

<sup>6</sup> Louis Lefèvre Gineau, "Mémoire lu à la Séance publique du Collège Royal le 10 novembre 1788: Dans lequel on rend compte des expériences faites publiquement dans ce même collège aux mois de Mai, Juin et Juillet de la même année, sur la composition et la décomposition de l'eau", *Observations de physique* 33 (1788): 457-466. In 1794 Delamétherie changed the name of his journal to *Journal de Physique, de chimie, d'histoire naturelle et des arts* and its publication was interrupted from 1795 to 1797.

<sup>7</sup> Delamétherie, *Essai analitique sur l'Air pur et les différentes espèces d'air* (1785), 2<sup>e</sup> édition, 2 vols., (Paris: chez Cuchet libraire, 1788).

<sup>8</sup> Joseph Priestley, "Expériences et Observations relatives aux principes d'Acidité, la Composition de l'Eau et le Phlogistique", *Observations de physique* 33 (1788): 103-110.

<sup>9</sup> By calculating the increase in weight of iron and adding the hydrogen released, the result is more or less equal to the weight of the vanished water.

<sup>10</sup> A few years later Priestley admitted that in chemistry there was divergence in the way of interpreting and doing the same research: "La doctrine du phlogistique et celle de la décomposition de l'eau a depuis long-temps attiré l'attention des chimistes philosophes; et l'expérience a semblé favoriser tantôt l'une, tantôt l'autre." Priestley, "Dernières expériences relatives à la décomposition de l'Air déphlogistiqué et de l'Air inflammable", *Observations sur la physique* 40 (1792): 91-97, on 91. In this regard, see Ferdinando Abbri, "J. Priestley e A.L. Lavoisier: il diverso significato di uno stesso esperimento", in *Scienza e storia: analisi critica e problemi attuali*, eds. Silvano Tagliagambe, Antonio Di Meo (Roma: Editori Riuniti, 1980): 147-167; Maurice P. Crosland, *Scientific institutions and practice in France and Britain, c. 1700-c.1870* (Aldershot: Ashgate, Variorum, 2007). See also, Abbri, "Alchemy and Chemistry: Chemical Discourses in the Seventeenth Century", *Early Science and Medicine* 5 (2000): 214-226.

<sup>11</sup> Priestley, "Farther Experiments relating to the Decomposition of Dephlogisticated and Inflammable Air, read April 7, 1791", *Philosophical Transactions: Giving some account of the present understandings, studies and labours of the ingenious, in many considerable parts of the world* (London, 1665-1862) LXXXI (1791): 213-222, on 217.

<sup>12</sup> "Quelques chimistes d'un ordre très-distingué se persuadent que l'hydrogène est le phlogistique de Stahl, et, comme ce célèbre chimiste admettait du phlogistique dans les métaux, dans le soufre, dans le charbon etc. ils sont obligés de supposer qu'il existe également de l'hydrogène fixé et combiné dans toutes ces substances; ils le supposent; mais ils ne le prouvent pas, et, quand ils

le prouveraient, ils ne seraient pas beaucoup plus avancés, puisque ce dégagement du gaz hydrogène n'explique en aucune manière les phénomènes de la calcination et de la combustion. Il faudrait toujours en revenir à l'examen de cette question: le calorique et la lumière qui se dégagent pendant les différentes espèces de combustion sont-ils fournis par le corps qui brûle ou par le gaz oxygène qui se fixe dans toutes ces opérations? et certainement la supposition de l'hydrogène dans les différents corps combustibles ne jette aucune lumière sur cette question." Antoine-Laurent Lavoisier, *Traité élémentaire de chimie*, in *Œuvres de Lavoisier publiées par les soins de S.E. le ministre de l'Instruction publique et des cultes*, 6 vols. (Paris: Imprimerie Impériale et Nationale, 1862-1893), vol. 1 (1789), 154.

<sup>13</sup> "Il est impossible de méconnoître l'existence d'un principe inflammable dans beaucoup de corps, à moins que l'esprit soit absolument égaré par des préjugés. [...] Mais serait-il raisonnable de mettre l'existence de ce principe en doute, parce qu'on ne peut le recueillir immédiatement? Je réponds par la négative car l'expérience prouve que pendant la calcination des métaux, ou pendant que d'autres corps brûlent avec une flamme, il se répand dans l'air une matière particulière sensible à l'odorat, et qui doit être la même dont dépend l'inflammabilité de ces corps; car ces derniers ayant été dépouillés de ce principe, sont ou entièrement consumés, ou cessent d'être inflammables." Johann Christian Wiegleb, "Doctrine de Stahl sur le Phlogistique, rectifiée et appuyée par des preuves, en opposition au nouveau Système chimique des François, dont on cherche en même tems à démontrer le peu de solidité, Extrait des Annales de Chimie de Crell", *Observations sur la physique* 41 (1792):81-85, on 84-85.

<sup>14</sup> Lavoisier, "Réflexions sur la décomposition de l'Eau par les substances végétales et animales", *Observations sur la physique* 34 (1789): 460-471, on 460.

<sup>15</sup> "Avant-Propos", in *Annales de Chimie, ou recueil de mémoires concernant la chimie et les arts qui en dépendent par MM. de Morveau, Lavoisier, Monge, Berthollet, Fourcroy, Dietrich, Hassenfratz et Adet* (Paris: chez Cuchet, 1789-1815), 19 (1797, An V): iii-xi, on ix. Crosland has claimed that for a clear understanding of the Chemical Revolution the new journal of the *Annales de chimie* can be rightly considered as fundamental as the *Traité élémentaire de chimie*. Crosland, "Lavoisier, the two French Revolutions and 'The Imperial despotism of oxygen'", *Ambix* 42 (1995), 101-118. See also, Crosland, *In the Shadow of Lavoisier: The Annales de Chimie and the establishment of a new science* (Oxford: British Society for the History of Science, 1994).

<sup>16</sup> Lavoisier, "Mémoire sur la combustion du fer", *Annales de chimie* 1 (1789): 19-30.

<sup>17</sup> Concerning this, it is interesting to note that in his preliminary speech for the year 1799 Delamétherie admitted that water was decomposable and falsely declared to have always supported that theory: "J'ai toujours supposé la décomposition de l'eau; néanmoins, j'avoue que, quoiqu'il y ait un grand nombre de faits en sa faveur, elle ne me paroît pas encore démontrée. [...] C'est moi, néanmoins, qui a fait la première expérience sur la combustion de l'air pur et de l'air inflammable." Delamétherie, "Discours préliminaire", *Journal de physique* 5 (1799): 3-99, on 97.

<sup>18</sup> The dispute arose from the observations of the Italian Landriani, subsequently confirmed by the Dutch van Marum, about the presence of flammable gas in carbon. Consequently carbon could not be classified as simple. Berthollet solved the misunderstanding stressing that the term "*carbone*" could not be confused with the term "*charbon ordinaire*": the first one indicated a simple element, the second one indicated a substance combined with extraneous earth, hydrogen and nitrogen. See Martinus Van Marum, "Extrait d'une lettre écrite par Van-Marum à M.Berthollet, Harlem, le 5 Décembre 1788", *Annales de Chimie* 2 (1789): 270-277.

<sup>19</sup> The dispute involved the Germans Girtanner, Wiegleb, Goettling and Wurzer who sustained the compound nature of nitrogen and some new chemists who experimentally confuted these

hypotheses. In particular, See Christoph Girtanner, "Extrait d'une lettre de M. le conseiller Girtanner au citoyen Van-Mons sur l'analyse de l'azote, Gottingue, le 26 Décembre 1799", *Annales de chimie* 33 (An VIII): 229-231; idem, "Mémoire dans lequel on examine si l'azote est un corps simple ou composé; par l'auteur à Van Mons", *Annales de chimie* 34 (An VIII): 3-40.

<sup>20</sup>According to Reboul, combustion had been redefined as a pure process of "mutual combination" between the combustible body and the vital air yielding a new body whose weight equals that of the constituents. Reboul, "Lettre de M.Reboul de l'Académie de Toulouse à M.De La Métherie sur la Combustion", *Observations sur la physique* 34 (1789): 124-126, on 125. Concerning this, it is useful to remember the dispute initiated on Delamétherie's journal by the Lavoisierian chemists about the new meaning of "combustion" (or, the combination of bodies producing loss of their original qualities) different from the pre-Lavoisierian one. In the case of water, for example, they claimed that "l'eau n'est point un simple mélange de gaz inflammable et d'air vital; elle est le produit de la combinaison de deux bases de ces deux fluides élastiques; or, la base du gaz inflammable étant saturée d'air vital, doit former un composé qui ne doit plus avoir d'affinité avec ce dernier corps, comme nous voyons le soufre constituant l'acide vitriolique par son union avec l'oxygène, ne plus avoir de tendance à se combiner avec lui une fois qu'il en est saturé". Pierre-Auguste Adet, Jean-Henri Hassenfratz, "Lettre de MM. Adet et Hassenfratz à M. De La Métherie sur la Chimie des Pneumatistes, le 21 Février 1787", *Observations sur la physique* 30 (1787): 215-218, on 217-218. According to the historiographical hypothesis of Holmes, instead, the Lavoisierian redefinition of combustion did not constitute a moment of discontinuity within the eighteenth-century chemistry as the meaning of the term itself changed "gradually" during the century: "Like other terms in eighteenth-century chemistry, "combustion" was gradually acquiring a broader meaning abstracted from its original applications." Frederic L.Holmes, *Lavoisier and the Chemistry of Life. An Exploration of Scientific Creativity* (Madison: University of Wisconsin Press, 1985), 126.

<sup>21</sup>The Dutch anti-phlogiston group constituted itself on the reproduction of the experiments of analysis/synthesis of water by electricity; see Adriaan Paets Van Troostwyck, Johann Rudolph Deiman, "Lettre de MM.Paets van Troostwyck et Deiman à M.De La Métherie sur une manière de décomposer l'Eau en Air inflammable et en Air vital", *Observations sur la physique* 35 (1789): 369-378. The Dutch group of experimenters then disproved the Priestleian conclusions on the newly defined gas "oxide gazeux d'azote"; see Deiman, Paets Van Troostwyck, P.Nieuwland, Nicolas Bondt, Anthoni Lauwerenburgh, "Mémoire sur la nature de l'oxide gazeux d'Azote, nommé par Priestley gaz nitreux déphlogistiqué", *Observations sur la physique* 43 (1793): 321-333. Then, followed the refutation of Wiegleb and Wurzer's hypothesis that nitrogen was obtainable from water in Deiman, Paets Van Troostwyck, Lauwerenburgh, "Recherches sur l'origine du gaz qui est produit par le passage de la vapeur aqueuse à travers des tubes rougis", *Annales de chimie* 26 (1798): 310-334; Deiman, Paets van Troostwyck, Lauwerenburgh, Vrolik, "Expériences nouvelles sur la prétendue conversion de l'eau en gaz azote", *Annales de chimie* 29 (An VII): 225-236. See Roberts's recent studies on the Dutch Enlightenment. In particular, Lissa Roberts, "Science Becomes Electric. Dutch Interaction with the Electrical Machine during the Eighteenth Century", *Isis* 90 (1999): 680-714.

<sup>22</sup>Claude-Louis Berthollet, "Remarques sur le Mémoire dans lequel M.Girtanner examine si l'azote est un corps simple ou composé", *Annales de chimie* 35 (An VIII): 23-31.

<sup>23</sup>Lazzaro Spallanzani, "Extrait de l'Examen chimique des expériences de M.Goettling sur la lumière du phosphore dans les différens gaz (Modena, 1796) par le citoyen Venturi", *Annales de chimie* 22 (1797): 246-257.

- <sup>24</sup> In regard with the phenomena of phosphorescence during the previous seventeenth-century, see in particular Jan Golinski, "A Noble Spectacle. Phosphorus and the Public Cultures of Science in the Early Royal Society", *Isis* 80 (1989): 11-39.
- <sup>25</sup> Friedrich Alexander von Humboldt, "Lettre au citoyen Van-Mons sur le procédé chimique de la vitalité", *Annales de chimie* 22 (An V): 64-71.
- <sup>26</sup> Antoine-François de Fourcroy, "Extrait d'une lettre au citoyen Van-Mons au sujet de celle de M.Humboldt", *Annales de chimie* 22 (An V): 77-80, on 77.
- <sup>27</sup> Humboldt, "Lettre au citoyen Fourcroy sur l'application prématurée de quelques découvertes chimiques à la médecine", *Annales de chimie* 27 (An VI): 62-66, on 65. We must remember that in the French literature of the 18th century the term *organisation* was considered synonymous with *life*.
- <sup>28</sup> Fourcroy, "Réponse", *Annales de chimie* 27 (An VI): 67-71, on 67.
- <sup>29</sup> "Mais, si j'annonce avec sécurité l'espoir d'une heureuse et prochaine révolution dans l'art de guérir [...] Je repousse également et la prétendue suffisance de la doctrine Brownienne pour toute théorie de l'art de guérir, et l'indiscrete explication du mécanisme entier de la vie animale par une puissance chimique." Fourcroy, "Mémoire sur l'application de la chimie pneumatique à l'art de guérir, et sur les propriétés médicamenteuses des substances oxigénées", *Annales de chimie* 28 (An VII): 225-281, on 232.
- <sup>30</sup> Humboldt, "Mémoire sur l'absorption de l'oxigène par les terres", *Annales de chimie* 29 (An VII): 125-160.
- <sup>31</sup> Jean Baptiste Pierre Antoine de Monet de Lamarck, *Recherches sur les causes des principaux faits physiques* (Paris: Maradan, 2 vols., 1794).
- <sup>32</sup> Delamétherie, "Nouvelles littéraires", *Journal de Physique* 1 (1794): 400-403.
- <sup>33</sup> Lamarck, *Réfutation de la théorie pneumatique, ou de la nouvelle doctrine des chimistes modernes* (Paris: chez l'Auteur, Agasse, 1796).
- <sup>34</sup> Lamarck, "Mémoire sur la matière du feu, considéré comme un instrument chimique dans les analyses", *Journal de Physique* 5 (1799): 345-361.
- <sup>35</sup> "Cela étant ainsi, il me sera facile de faire voir dans un instant que la *chaleur*, cette mère des générations, cette âme matérielle des corps vivans, [...] a pu être le principal des moyens qu'emploie directement la nature pour opérer sur des matières appropriées, un acte de disposition des parties, d'ébauche d'organisation, et par suite, de vitalisation analogue à celui de la fécondation sexuelle." Lamarck, *Recherches sur l'organisation des corps vivans* (Paris: chez Maradan Libraire, An X, 1802), 102.
- <sup>36</sup> Spallanzani, *Mémoires sur la respiration par Lazare Spallanzani traduits en français d'après son manuscrit inédit par Jean Senebier* (Genève: J.J. Paschoud, 1803).
- <sup>37</sup> In his manuscripts the Italian naturalist Spallanzani claimed that he always adopted the surest method for his original chemical-physiological researches: "Mi son dunque appigliato al metodo più esatto, e sicuro." Spallanzani, *Mss Regg B* 49, 153 (306], Biblioteca Municipale "Panizzi", Reggio Emilia.
- <sup>38</sup> Lamarck, *Hydrogéologie* (Paris: chez l'Auteur, Agasse, An X, 1801-1802), 8.
- <sup>39</sup> Delamétherie, "Nouvelles littéraires", *Journal de Physique* 13 (1803), 476.
- <sup>40</sup> According to Partington, in their *Mémoire sur la chaleur* of 1783 Lavoisier and Laplace established a fundamental law of thermochemistry according to which *all changes in heat, whether real or apparent, suffered by a system of bodies during a change of state recur in the opposite sense when the system returns to its original state*. See James Riddick Partington, *A History of Chemistry*, 4 vols. (London: MacMillan, 1962), vol. 3, 428. See also Virginia M. Schelar,

“Thermochemistry and the Third Law of Thermodynamics”, *Chymia* 11 (1966), 99-124; Angela Bandinelli, “The Isolated System of Quantifiable Experiences in the 1783 *Mémoire sur la chaleur* of Lavoisier and Laplace”, *Ambix* 54 (2007): 274-284.

<sup>41</sup> These redefinitions occurred during the late eighteenth-century, thereby confirming the classical interpretation of Herbert Butterfield regarding the tardy development of the scientific revolution in chemistry. See Herbert Butterfield, *The origins of modern science* (London: G. Bell, 1958).

<sup>42</sup> According to the tradition inaugurated by Holmes, instead, the Lavoisierian principle of heat would come from the Stahlian phlogiston: “this elusive matter [of fire], so simply defined in the abstract, became in practice as malleable as ever its predecessor, phlogiston, has been reputed to be.” Holmes, *Lavoisier and the Chemistry of Life*, 37.

<sup>43</sup> Lavoisier, Laplace, *Memoir on Heat, Translated with an Introduction and Notes by Henry Guerlac* (New York: Neale Watson Academic Publications Inc., 1982), 30.

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## The Establishment of the *Journal Acta Chemica Scandinavica* in 1946

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The *Scandinavian Peninsula* consists of Norway and Sweden, but the notion *Scandinavia* also includes Denmark. However, the journal *Acta Chemica Scandinavica* was open to contributions from the *Nordic countries*: Denmark, Finland, Iceland and Sweden plus the self governed Faroe Islands (Denmark) and Åland (Finland). Original research from Nordic chemists, and foreign guest chemists reporting work carried out in a Nordic chemical laboratory, could be accepted written in English, French and German. *Acta Chemica Scandinavica*, in colloquial terms is called ACTA which is used in this paper, was founded in 1946 for reasons to be presented here.

### Nordic chemistry journals

In the 19<sup>th</sup> century chemists normally published in national journals for the natural sciences. At the end of the century specialised journals in physics, geology, botany etc became more common. The first national chemistry journal to be published in one of the Nordic countries was the Swedish Chemistry Journal, *Svensk kemisk tidsskrift*, which was founded in 1889. An earlier attempt in Sweden, *Kemiska Notiser*, only lasted from 1887-9. Finland followed the Swedish example in 1893; Norway in 1904 and Denmark in 1920 (see the table below). These national journals were supported by the chemical societies in each country.

Before the Second World War (1939-45) the chemists in the Nordic countries mainly wrote their most significant scientific papers in German and published them in German journals as for example in *Annalen der Chemie* or *Zeitschrift für Analytische Chemie*. Each country also had their national chemistry journals for papers mainly written in the native language where, what were considered to be, less important papers were published. These journals also contained popular articles on the development of chemistry and articles discussing the industrial and

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educational development in each country and in Europe. The name and a brief history of each journal, from the start to the present day, is given in the table below. These local chemistry journals still exist but the more ambitious, purely Nordic chemistry journals are no longer published. However they continued to be published much longer than the founding fathers of ACTA thought in 1946.

**Table 1**  
**The National Chemical Journals in the Nordic Countries**

<i>Country</i>	<i>National chemistry journals</i>
Denmark	<i>Kemisk Maanedssblad</i> 1 (1920) continued as <i>Dansk kemi</i> from 43 (1962) - still published.
Finland	<i>Finska kemistsamfundets Meddelanden</i> 1 (1893) - 82 (1973) continued as <i>Kemia-Kemi</i> 1 (1974) - still published. <i>Suomen Kemistilehti</i> 1 (1928) – 46 (1973) succeeded by <i>Finnish chemical letters</i> 1 (1974) - 15 (1988) publication ceased.
Norway	<i>Tidsskrift for kemi og farmasi</i> 1 (1904) - 17 (1920) continued as <i>Tidsskrift for kjemi og bergvesen</i> 1 (1921) and from 1940 also included <i>metallurgi</i> - still published now called <i>Kjemi</i> .
Sweden	<i>Svensk Kemisk Tidskrift</i> 1 (1889) - still published now called <i>Kemivärlden-Biotek</i> med <i>Kemisk Tidskrift</i> . <i>Arkiv för Kemi, Mineralogi och Geologi</i> 1 (1903) - 26 (1949). <i>Arkiv för Kemi</i> 1 (1950) – 33 (1974) succeeded by <i>Chemica Scripta: an International Journal on Progress in Chemistry and Biochemistry</i> 1 (1971) – 18 (1989); publication ceased.

The Danish, Norwegian and Swedish languages are so similar that native speakers of one of them can easily understand the others. However, Finnish is completely different. Earlier, many Finns spoke Swedish, and the first chemistry journal in Finland was printed in Swedish. The industries in the Nordic countries are very different, so industry has not been a source of cooperation between the Nordic chemists.

Contact between the scientists in the Nordic countries took place through meetings (skandinaviske naturforskermøter) held successively in Copenhagen,

Christiania (Oslo) and Stockholm every 5 to 6 years from 1839 to 1936. The chemists arranged their own meetings from 1920. The next ones in 1923, 1926, 1932 and 1939 before the Second World War. The meetings created a network between the Nordic chemists especially at the Professorial level.

In 1919 the founder and editor of the Norwegian chemistry journal died, and after some discussion it was continued from 1921 as a journal for both for chemistry, the mining industry (bergverk) and later metallurgy. In the discussion of what to do in 1920 the Norwegian Chemical Society contacted the chemical societies in Denmark and Sweden with a proposal to found a Nordic chemistry journal to replace the national chemistry journals, but this proposal was rejected.

The editor of the chemistry part of the Norwegian journal, Peter A.R. Sollied (1869-1955), proposed at the 4<sup>th</sup> Nordic Chemists Meeting in Oslo in June 1932 (285 participants) a closer cooperation between the Nordic chemistry journals. He also proposed that the national chemical societies should prepare the establishment of a Nordic chemistry journal for discussion at the 5<sup>th</sup> Nordic Chemists Meeting.<sup>1</sup> The meeting was held in Copenhagen in July 1939 (360 participants). Sollied was not present; he had just retired as Head of Chemistry at the Technical College in Oslo. Nothing was reported about any discussion on the establishment of a Nordic chemistry journal in the published negotiations from the meeting, but it may have been discussed informally.<sup>2</sup>

After the war in 1945 many Nordic chemists turned away from German and Germany and wanted to write and publish their work in English, but the number of journals available without restrictions was limited. The next Nordic Chemists meeting was planned to be held in Lund in Sweden, but was delayed to 1947 because of lack of foreign currency for travelling after the war. But some chemists were impatient and wanted action before 1947.

In Norway Professor Nils A. Sørensen (1909-87) at the Technical University (NTH) in Trondheim, took an initiative in May 1945. In Denmark Professor Jens A. Christiansen (1888-1969), then president of the Danish Chemical Society, was working on a plan to establish *Acta Chemica Danica*. The result was that representatives from the Nordic chemical societies met in Copenhagen, March 20-21, 1946, and decided to establish a Nordic chemistry journal and name it *Acta Chemica Scandinavica*. The news was published soon afterwards in all the national chemistry journals.<sup>3</sup>

The Nobel laureate in chemistry of 1945, Professor Artturi Virtanen (1895-1973), summed it up:

*Our hope and firm conviction is that this decision will promote chemical research in the Nordic countries and give the rest of the world a focused impression of the basic research in chemistry in Scandinavia.*

The owners of ACTA, responsible for its finances, were the chemical societies in Denmark (Kemisk Forening and Selskabet for Analytisk Kemi), Finland, (Soumalaisten Kemistien Seura and Finska Kemistsamfundet), Norway (Norsk Kjemisk Selskap) and Sweden (Kemistsamfundet). The ownership was divided equally among the countries. However, the expenses were divided among the owners based on the relative number of pages published from each country.

The first number was published in January of 1947 and until 1974 ten issues were published in each volume. The editors from each country in this period are given in Table 2. Karl Myrbäck (1900-85), Professor of Biochemistry in Stockholm, was the editor-in-chief in the period from 1947 to 1973. The reputation of the journal grew with the years and the number of subscribers increased slowly, but the economy in the first thirty years was always frail, and at times the chemical societies needed external support to cover the publishing expenses.

**Table 2**  
**Editors of *Acta Chemica Scandinavica* from its foundation**

Demark	1947-62	Jens Anton Christiansen
	1963-73	Carl Johan Ballhausen
Finland	1947-66	Artturi Ilmari Virtanen
	1967	Eero Tommila
	1968-73	Heikki Soumalainen
Norway	1947-56	Odd Hassel
	1957-65	Olav Foss
	1966-68	Alf Wickström
	1969-73	Olav Notevarp
Sweden	1947-73	Karl Myrbäck

In 1999 ACTA merged with the British chemistry journals Dalton and Perkin<sup>1</sup>, published by the Royal Chemical Society in London. In the first five years, after 2000, *Incorporating Acta Chemica Scandinavica* was printed on the front cover of each journal.

The reasons for the merger, have been discussed by Harnung and Terjesen.<sup>4</sup> This paper has concentrated on the establishment of the first and only Nordic che-

mistry journal intended for an international audience. It was built on a network of Scandinavian Professors of Chemistry, nourished by their meetings in the main cities of the Nordic countries: Copenhagen, Gothenburg, Helsinki, Oslo and Stockholm. The editors in the period when Myrbäck was editor-in-chief were all Professors of Chemistry. Their names are given in Table 2. The establishment of ACTA, and the running of the journal, was the most substantial cooperation between the chemists in the neighbouring Nordic countries in the last sixty years.

The content of ACTA has been digitalized and a beta-version is available on the Internet: <http://actachemscand.dk/>

## References

- <sup>1</sup> P. R. Sollied: *Short remarks about a Nordic chemistry journal (in Norwegian)*. Forhandlinger fra det 4<sup>de</sup> Nordiske kjemikermøte Oslo 27. 30. June 1932 page 35-36. (Oslo: Brøgger, 1933)
  - <sup>2</sup> Forhandlinger det 5<sup>de</sup> Nordiske kemikermøde København 3.-7. July 1939. (København: Gads, 1940)
  - <sup>3</sup> O. Hassel and N.A. Sørensen: "Meddelelse til medlemmene av Norsk Kjemisk Selskap angående Acta Chemica Scandinavica." *Tidsskrift for kjemi, bergvesen og metallurgi* (1946) 137-8.
  - <sup>4</sup> Sven E. Harnung: "Acta Chemica Scandinavica." *Dansk kemi* **82** (2001) 44-46.
- Sven G. Terjesen: *Acta Chemica Scandinavica – ACTA. Norsk kjemisk selskap 1893 – 1993*. (Oslo: Tidsskriftforlaget Kjemi AS. 1993) 42-43.

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## The Origins of Chemical Literature as a Separate Discipline of Chemistry

*F. Bartow Culp\**

Where does a scientific discipline come from? What are the forces that bring into being a particular branch of study, that define its boundaries, that establish its uniqueness from a previous undifferentiated area of study? To be sure, there is an epistemological artificiality about inserting boundaries into the flow of scientific endeavor: where does molecular biology end and biochemistry begin? However, these divisions have historically served as useful demarcations for many people, from researchers to book publishers. Certain defined fields in chemistry were derived from historical notions: “Organic” chemistry was originally thought necessarily to involve life forces, and its coalescence in the first half of the nineteenth century into a separate discipline occurred rather slowly. In contrast, it can be argued that the discipline of physical chemistry came into being over the course of a single decade, beginning with Willard Gibbs’ 1876 paper “On the Equilibrium of Heterogeneous Substances”, and being fully realised by the appearance of the journal, *Zeitschrift für physikalische Chemie*, founded in 1887 by Wilhelm Ostwald and Henricus van t’Hoff. Similarly, the subdiscipline of chemistry known variously as chemical literature, chemical documentation, chemical information, and more recently, cheminformatics, arose rather quickly during the first quarter of the twentieth century. This paper discusses the various forces and personalities that brought this about. It is shown herein that the discrete concept of “chemical literature” as a separate topic of study and instruction, and therefore a valid subdiscipline of chemistry, came about for three main reasons: (1) the convergence of intellectual interests, (2) perceived necessity, and (3) the availability of a suitable work force.

### Converging intellectual interests and perceived necessity

From its formal beginnings as a separate science in the late 18<sup>th</sup> century, chemistry has had a remarkably unified and coherent literature. In addition to its jour-

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nals, it has had, almost from the beginning, publications that summarised and abstracted its scholarly thought. Two examples of the former effort are Leopold Gmelin's *Handbuch der Allgemeinen Chemie*, begun in 1809, and Berzelius' magisterial *Jahresberichte*, published from 1821-1849. The most important example of the second type of publication was the *Chemisches Zentralblatt* (originally *Pharmaceutisches Central-blatt*), which began in 1830 to publish abstracts of the chemical literature. However, towards the end of the 19<sup>th</sup> century, the size and scope of chemical publications had grown to the point that chemists realised that they were increasingly unable to keep abreast of the current literature, even in their own area of study. In an address to the Chemical Society of London, the chemist H. E. Armstrong said "chemical literature is fast becoming unmanageable and uncontrollable from its very vastness. Something must be done in order to assist chemists to remain in touch with their subject and to retain their hold on the literature generally" .

This increase in the number of publications came as result of the growing numbers of chemists following the establishment of chemistry as a full-time profession around the middle of the 19<sup>th</sup> century. And this increase itself was partially as a result of the swift rise of the chemical industry in the last half of the 19<sup>th</sup> century, with its attendant need for research facilities and personnel.

### **The international bibliographic movement**

In the early 1880's two Belgian jurists, Paul Otlet (1868-1944) and Henri La Fontaine (1854-1943), began a collaboration that resulted in the formation in 1895 of the International Institute of Bibliography (IIB). The aim of this remarkable organisation, which exists today as the International Federation for Information and Documentation, was to collect, organise, and make available for use all of the world's published literature . This ambitious goal originally sprang from Otlet's desire to solve some of the problems created by the proliferation and disorderly state of the literature of the social sciences. The IIB, led by this energetic and visionary man and supported by the Belgian government, was successful, and by 1911 had created a collection of more than 11 million records, extensively cross referenced and separately entered on 3x5 in (75x125 mm) cards. The means of organising this vast amount of information, which was called the Universal Bibliographic Repertory (UBR) and which could be considered the forerunner of a modern electronic database, was the Universal Decimal Classification (UDC). Developed by Otlet, the UDC was an extension of the Dewey Decimal Classification (DDC), devised in 1876 by Melvil Dewey, an American librarian.

The subjects covered by the UBR were not only the social sciences, but increasingly included the natural sciences, especially chemistry. Otlet's efforts in this direction were strongly influenced by the ideas of another intellectual giant of that time, Wilhelm Ostwald.

### **Wilhelm Ostwald's long shadow**

Ostwald, co-founder with van't Hoff of the discipline of physical chemistry and winner of the Nobel Prize in chemistry in 1909, was acutely aware of the need for better organisation and communication of the scientific literature. In 1908 he wrote: "Everyone who is active in science in any way appreciates the fact the task of comprehensively organising scientific reporting is a necessity which constantly grows more urgent". And later: "it has become practically impossible for the individual to follow the total production in his science". Besides lending his authority and ideas to the IIB as a member of its advisory board, Ostwald also promoted several other ways to systematise scientific literature. One was the concept of the "monograph" article, essentially a free-standing article published on a standardised paper size, and not connected to a particular journal. It has been only recently that this idea, essentially a pdf ASAP article, has been realised. Ostwald also wrote the first book to treat specifically the organisation of scientific literature. Mostly of a theoretical nature, it was set in print in 1914 but not published until 1919.

It is not clear how directly Ostwald's and Otlet's writings and the activities of the IIB influenced the incorporation of chemical literature courses into the curricula of universities in the United States and in Europe; however, both men were tireless promoters of their ideas both in print and at learned gatherings, and it is reasonable to assume that the main thrusts of their arguments must have had some impact on their peers:

- The understanding that it was not only desirable but necessary to better organise the burgeoning flood of scientific literature.
- The realisation that there were new tools to do this, along with a new cadre of professionals to wield these tools.

There were certainly increasing numbers of documented dissatisfactions by academic leaders in America concerning the information competencies of their students. In a speech before the New York section of the American Chemical Society in 1916, Dr. Richard C. Maclaurin, president of the Massachusetts Institute of



Technology, stated that the typical chemist treats “shamefully” the knowledge that has already been garnered, and advocated that he should “reduce his work in the laboratory by increasing his work in the library”. In a report to the American Association for the Advancement of Science, R. F. Bacon stated that “the average graduate is usually almost helpless” when attempting to “digest intelligently the important contributions which have been made upon the subject”. He went on to say “the solution is to be found in the provision in the chemical curriculum...of a course of lectures on the literature of chemistry”.

### **Opportunity: emergence of librarianship as a profession in the USA**

For much of the 19<sup>th</sup> century in America, the term “librarian” was rather haphazardly defined. Librarians at academic institutes were generally faculty members who were assigned the duty of supervising the library, with no reduction in other duties and no increase in pay. Such reluctant recruits had no real understanding of the concepts of library collection development and management, nor much desire to spend time learning them. In many institutions, the book collections were dispersed around campus, generally according to their subjects, and were frequently housed in locked rooms near the department heads’ offices. How the collections were organised also varied widely among different institutions, often according to a personal system devised by the current librarian. Such conditions were certainly not helpful to the faculty and students who wanted to keep abreast of the current literature!

These conditions began to change in the last decades of the century with the growing importance of academic research, which made library resources a high priority. In addition, the appearance of librarianship as a bona fide profession provided manpower to help systematise and promote those resources. Melvil Dewey, who had created the first universal (and still most widely used) classification system for libraries, was highly critical of the apprentice system then used to train librarians. At Columbia University in 1887, he established the first program for educating and graduating professional librarians. Similar schools quickly sprang up around the country to supply the growing need for librarians, both in the public sphere and in colleges and universities. Furthermore the founding of the American Library Association (1876) and the Special Libraries Association (1907) helped support the professional aspirations of librarians in the United States.

### **“Chem lit” courses: first stirrings in the USA**

If the complaints and comments of President Maclaurin and Professor Bacon cited above are taken as true, there was little, if any, systematic instruction in the use of the chemical literature by university faculty members in the late 19<sup>th</sup> and early 20<sup>th</sup> centuries. The single exception I have found is a letter submitted in 1901 to the Library Journal by Professor H. P. Talbot of the Massachusetts Institute of Technology . In it he describes a one-credit course taught by him to chemistry majors in their junior year. The students were given practice in reading German, and were required to prepare a bibliography of the journal literature on a selected subject. While this course could not be considered as comprehensive introduction to the range of the literature of chemistry, it was at least a start in the right direction.

Such courses did not appear in any university curriculum until 1913 at the University of Illinois and 1915 at the University of Pittsburgh . The course at the University of Illinois was created by Marion E. Sparks, the Chemistry Librarian there. She was a member of the second graduating class in the University’s School of Library Science in 1899, and although untrained in chemistry, wrote and published the first book to address chemical literature and library instruction. Her small book “Chemical literature and its use” was based on lecture notes for the class she taught, and covered the literature in all the areas of chemistry. It went through two editions, was very favorably reviewed and widely accepted , and formalised the field of chemical information. In 1921, M. G. Mellon, a new instructor in the Purdue University chemistry department, was given the assignment to create a course patterned along the lines of Ms. Sparks’ course. Mellon was not a librarian, but unlike most of his peers, he became deeply interested in chemical literature as a subject worthy of being taught on its own. The result of his work was the publication in 1925 of the textbook “Chemical publications, their nature and use” . It was far more detailed than Sparks’ slim book, and established itself as the preeminent resource for this new branch of chemistry. It remained so for over 50 years, going through five editions, the last published in 1982.

It is interesting to note that the activities described above took place almost exclusively in the colleges and universities in the United States, even though the theoretical framework and initial impetus had occurred in Europe. Three main reasons are seen for this:

The rapid development of the industrial and academic chemical research infrastructure in the United States, which made the need for a proper understanding of the use of the chemical literature even more urgent;

The relatively easy adaptability of American universities to curricular changes, as opposed to the more classical and change-averse universities in Europe;

The early availability of professional librarians in the American academic system, and their subsequent acceptance by their faculties as peers, as opposed to a more stratified and hierarchical view of librarians by faculties of European universities.

For these reasons, chemical information is widely taught today in American institutions of higher learning, either as a separate course or integrated into other courses, but is still rarely encountered in European institutions. The American Chemical Society has also recognised its importance by establishing in 1943 a Division of Chemical Literature (now Chemical Information).

## Notes

<sup>1</sup> J. Willard Gibbs, *The Scientific Papers of J. Willard Gibbs: Thermodynamics*. Ox Bow Press: Woodbridge, CT, 1993; Vol. 1.

<sup>2</sup> Brian Vickery, A century of scientific and technical information. *Journal of Documentation* 1999, 55, (5), 476-527.

<sup>3</sup> Ian C. McIlwaine, The Universal Decimal Classification: Some factors concerning its origins, development, and influence. *Journal of the American Society for Information Science* 1997, 48, (4), 331-339.

<sup>4</sup> Wilhelm Ostwald, Berzelius' *Jahresbericht* and the international organization of chemists. *Journal of Chemical Education* 1955, 32, 373-375.

<sup>5</sup> Wilhelm Ostwald, Scientific management for scientists. *Scientific American* 1913, 5-6.

<sup>6</sup> Wilhelm Ostwald, *Die chemische Literatur und die Organisation der Wissenschaft*. 1 ed.; Akademische Verlagsgesellschaft GmbH: Leipzig, 1919; Vol. 1, p 120.

<sup>7</sup> Richard C. Maclaurin, Universities and industries. *Journal of Industrial and Engineering Chemistry* 1916, 8, (1), 59-61.

<sup>8</sup> Roger F. Bacon, Research in industrial laboratories. *Science* 1917, 15, 34-39.

<sup>9</sup> Michael H. Harris, *History of libraries of the western world*. 4th ed.; Scarecrow Press, Inc.: Metuchen, NJ, 1995.

<sup>10</sup> H. P. Talbot, Course in bibliography of chemistry. *Library journal* 1901, 26, 202.

<sup>11</sup> Marion E. Sparks, Chemical literature and its use. *Science* 1918, N.S. 47, (1216), 377-381.

<sup>12</sup> William A. Hamor, Bibliography, the foundation of scientific research. *Special Libraries* 1923, 14, (2), 215-220.

<sup>13</sup> Anon., "Chemical Literature and its use" (review). *Journal of the Society of Chemical Industry* 1921, 40, (4), 451a.

<sup>14</sup> Melvin G. Mellon, *Chemical publications, their history, nature, and use*. Edwards Brothers Publishers: Ann Arbor MI, 1925.

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## How Shall We Teach Chemistry. First Approaches to Didactics of Chemistry in the Nineteenth Century

Gisela Boeck\*

Confronted with relatively bad results of the medical students in chemistry the question arose, “why”. Is there a problem with the chemistry classes at school? Or, where else could the disinterest in chemistry originate? It is now well recognised that chemistry is an unpopular subject at school. Whereas the pupils start the classes with many expectations, questions and interest they change their opinion after a short time. There are different reasons for this development; chemistry school teachers, university teachers and *didacts*<sup>1</sup> are investigating this problem very seriously. They have made proposals for improvements in teachers’ education and developed new models for chemistry classes, such as, “Chemistry for life”<sup>2</sup> or “Chemistry in context”.<sup>3</sup>

It is interesting also to attempt answer the following question: How was the situation in that time in German speaking countries when school level chemical education started? The history of chemical education at universities is already investigated very well. In the literature there is much information about chemistry as *ancillary science* for medicine or metallurgy before the nineteenth century, the institutionalisation of chemical studies as a fully recognised science and further discussions about problems between universities and “Technische Hochschule” (Technical University)<sup>4</sup> during the nineteenth century.

Some aspects of the development of chemical classes at school are investigated in some older papers, too. But in the earliest reports on teaching realities the special term “chemistry” is not used, the courses were characterised as natural history, for instance, natural objects for “recreation” are described in the school program of August Hermann Francke (1663-1727),<sup>5</sup> one of the most important exponents of pietism. Further on, the acquaintance with realities is an attribute of the philanthropical pedagogy which had a long lasting influence on teaching practise in *Volksschule* (elementary school). In the German high school (*Gymnasium*) chemistry courses often failed until the end of the nineteenth century. Details about the situation in the Rhineland in the nineteenth century have been described by

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Halasik,<sup>6</sup> detailed discussions about school laws in the publications of Bonnekoh<sup>7</sup> and Schoeler,<sup>8</sup> a first overview about schoolbooks was made by Just<sup>9</sup> and can be found in “Communicating chemistry”.<sup>10</sup> A good overview on chemical textbooks used in academic teaching is the doctoral thesis of Haupt.<sup>11</sup> A more detailed analysis of German chemical textbooks on the background of the invention of *Theoretische Chemie* in the period 1775-1820 is given by Frercks and Markert.<sup>12</sup> Questions of popularisation played an important role for the development as scientific discipline as well as for chemical education.<sup>13</sup>

These different points of view were connected to clarify the question: What does chemical education mean in the nineteenth century? The influences on (chemical) education should be considered, for example the current state of scientific knowledge, the role of systematisation and knowledge classification as well as technical and economical conditions. In addition, there are several factors which accelerate or retard the development of chemical education, such as educational policy, the acceptance of the science in the society, philosophical views, progress in institutionalisation and the developments in pedagogy and didactics. The connection between chemical education (at school and/or at university) and the genesis of chemistry as a discipline should be much better shown. Furthermore, it should be interesting to see if there are persons, other than Justus Liebig (1803-1873),<sup>14</sup> who influenced chemical education (either popular, school or university education). In which way were they (academics and/or non-academics) encouraged? When and why can we find any qualitative changes in textbooks and in teaching chemistry concerning the didactics and methods? Where are the roots of the methodology/didactics of chemistry? Which tools were used in chemistry classes at school?

Concerning the question of establishing chemistry as a discipline the description of Laitko will be followed, “disciplines are self-reproducing systems of scientific activities”.<sup>15</sup> Knowledge of the discipline is necessary for the internal reproduction which is usually (in the case of scientific disciplines) taught at university. If this knowledge already has got a basis at (secondary) school the results will be much better. At the first glance it could seem that a discipline is not more than a (school or university) subject, but in reality must include the knowledge and the skills that are necessary to produce new knowledge. Around 1800 the first stage of discipline genesis for chemistry was reached, as Guntau explained there was a lot of connecting elements in the scientific thinking; the individual scientific disciplines were recognised not only by the scientific community, but by the public.<sup>16</sup> The classical disciplines (mechanics, chemistry, botany, zoology and geology) were established; natural history including the concept of the three natural “habi-

tats<sup>17</sup> became obsolete, however this was a very slow process. At the universities the knowledge no longer had a more or less fixed form; the students were confronted with developing knowledge. The faculties of Philosophy got the task to educate teachers for the humanistic High school (*Gymnasium*) or the *Realgymnasium*.<sup>18</sup> The aim of education in these schools was similar to that in the philosophical faculty, only modified by the age of the pupils. The teachers studied science, and the knowledge corresponded to the current results in science. Relevant professional skills (pedagogy, methodics, didactics, and psychology) were not taught.<sup>19</sup>

All the mentioned problems and questions can not be answered in this paper; the main focus herein is the role of chemists which could be described as early didacts of chemistry.

### **Some important German didacts of chemistry in the nineteenth century**

In this paper it is only possible to show a small part of all the outlined problems. Especially attention will be paid to the didactic work of Julius Adolph Stoeckhardt (1809-1886). After that the practise of Julius Eugen Wagner (1857-1924) will be illustrated. Stoeckhardt started to teach natural science, later chemistry in the 1840's. The endeavours for didactics in chemistry by Wagner are about 60 years younger, Stoeckhardts point of view has a touch of natural history, Wagners is characterised by an already specialised chemistry, which means physical chemistry. This can be best understood if it is recognised that Wagner was a pupil of Wilhelm Ostwald (1853-1932).

### **Julius Adolph Stoeckhardt - his biography**

Julius A. Stoeckhardt was a multifunctional character: a scientist, a chemist and a teacher.<sup>20</sup> In the space available it is not possible to discuss his role as a propagator in agricultural chemistry, about his controversy with Liebig about the role of nitrogen, about his work on toxicity of several colours used in the nineteenth century or about his results on the damage of forest by fumes. After an apprenticeship and employments in several pharmacies Stoeckhardt studied natural sciences in Berlin from 1832 to 1833. Heinrich Rose (1795-1864), Eilhard Mitscherlich (1794-1863), Sigismund Friedrich Hermbstaedt (1760-1833), Heinrich

Friedrich Link (1767-1851), Karl Sigismund Kunth (1788-1850) and Henrich Steffens (1773-1845) were his scientific teachers. After a journey through Europe with visits to famous chemists such as Michael Faraday (1791-1867), Joseph Louis Gay-Lussac (1778-1850), Antoine Laurent de Jussieu (1748-1836) and Jean Baptiste Dumas (1800-1884) he started to work in the laboratory of the mineral-water-production of Friedrich Adolf Struve (1781-1840) in Dresden. At that time he was acquainted with Georg Paul Alexander Petzholdt (1810-1889), who lectured *privatim* and who had a laboratory for his students private use. It is reported that Stoeckhardt worked in this laboratory, Franz Varrentrapp (1815-1877) also possibly worked there.<sup>21</sup> In 1837 Stoeckhardt became a teacher of natural sciences in the “Vitzthumsche Geschlechtergymnasium” [High school of the Vitzthum dynasty, donated by Rudolph Vitzthum von Apolda (1572-1639)] in Dresden, which was combined with “Blochmannsche Erziehungsanstalt” [Boarding school of Karl Justus Blochmann (1786-1855)] since 1828. It is unknown in which way Stoeckhardt taught natural sciences. But every school program from 1830 to 1837 informs about physical and chemical practical work of the pupils. In 1835 a special house could be used for laboratory work. This was very progressive for the time. It must be noted that there were three types of school: the “Progymnasium”, the “Gelehrtengymnasium” and the “Realgymnasium”.<sup>22</sup> In the school program from 1834 Snell reported about the agreement of teaching realities in “Gelehrtengymnasium”, not only with the aim to teach knowledge which can be useful for life (which is highly required by philanthropism) but on the background that realities are playing an important role as instruments in general education. Chemistry under the practical aspect was taught in the “Realgymnasium”. Snell demanded the systematic teaching in natural science, especially.<sup>23</sup>

### Stoeckhardt's Doctorate

The Doctoral thesis of Stoeckhardt at the University of Leipzig was titled: *Res Naturales, qua de causa perscrutandae, qua methodo docendae et tractandae, quomodo maturae convenienter disponendae*<sup>24</sup> (the natural objects, why should they be investigated, which method is used for teaching, in which way the nature is adequate described). The first question about the necessity of a treatment of nature was answered with words from Carl von Linné (1707-1778): “Alles was dem Menschen zum Nutzen gereicht, wird von diesen natuerlichen Coerpern hergenommen. ... Daher giebt sich von selbst zu erkennen, wie nothwendig die Erkenntniß der Natur sey.”<sup>25</sup> (Anything what is useful for human beings comes from natural matters. So it goes without explaining why the cognition of nature



is necessary). However, for Stoeckhardt the highest aim of cognition of the nature was the cognition of god in nature and the worship of god in nature. Stoeckhardt had a teleological point of view because he accepted that in all objects there is usefulness. It can be assumed that Stoeckhardt was influenced by natural philosophy (in Berlin he visited lectures of Steffens who taught natural philosophy with speculative direction) and he was influenced by the empirism of Francis Bacon (1561-1626). In the second part of his thesis Stoeckhardt made proposals about the methods which should be used in teaching natural sciences. Here one can find many similarities to opinions of August Hermann Francke<sup>26</sup> or of Ehrenfried Walter von Tschirnhaus (1651-1708).<sup>27</sup> Stoeckhardt demanded to use only a few examples, as lot of examples would strain the memory of the pupil. It is better to discuss some examples very intensively. The examples chosen should have a connection to the surroundings of the pupils (this is still very important for beginners courses today). Stoeckhardt recommended to show the natural objects and to do experimental work because this is very good for the memory. Every time the teacher should start with simple examples, further on he can use more complex ones. In the third part, Stoeckhardt explained his scheme for natural sciences after discussing the role of several schemes and the criterions for these schemes. He divided nature into matter and forces, but he stressed that forces can not exist without matter (and vice versa). Matter exists in three forms, namely, Organica, Atmosphaera and Inorganica. In the opinion of Stoeckhardt the atmosphere which includes air and water must have a special place in the scheme because life can not exist without air and water and the inorganic part is formed by water and air, too. He divided Organica and Inorganica into further disciplines that he then connected again in the three fundamental disciplines: Geologia, Biologia and Atmosphaeriliologia. Stoeckhardt believed that these are the constituent parts of natural history. The application of the term "biology" is somewhat astonishing for the period. Even though biology was "innovated" by Jean-Baptiste de Lamarck (1744-1829) and Gottfried Reinhold Treviranus (1776-1837) and in some earlier sources of the term are found it is hard to describe the exact meaning of "biology" in that time as Kanz established.<sup>28</sup>

In the same way he divided the forces into inorganic and organic forces and in *vis vitalis* (by the way, in his life Stoeckhardt never got over the question of vitalism<sup>30</sup>). Then he gave various examples for transformations of the forces (the word force is often used for energy in the modern sense). It can be established that Stoeckhardt knew of the current developments in science. Stoeckhardt expressed his opinion that schemes are helpful tools but the pupils must learn to connect the different parts again to obtain the whole view of nature. In this way Stoeckhardt

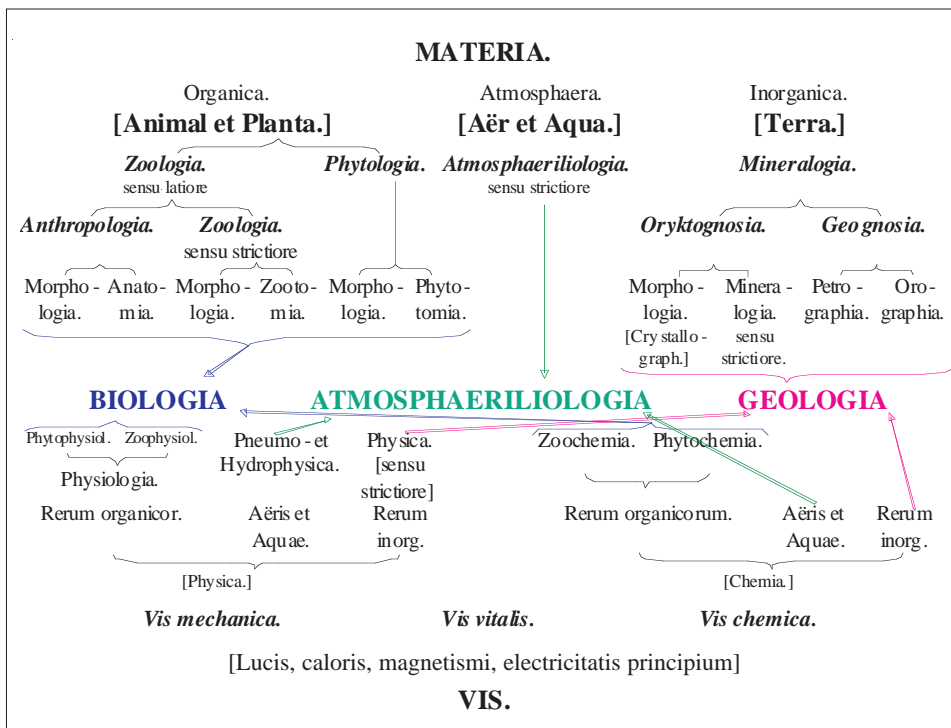


Figure 1: The scheme of Nature used by Stoeckhardt in his doctorate<sup>29</sup> (slightly modified in the format).

propagated one natural science (it is interesting that in some modern schools you can find attempts to teach natural sciences instead of chemistry, physics and biology). One can reason that Stoeckhardt followed the fundamental views in the didactics of natural sciences which were already established in the seventeenth and eighteenth centuries. He followed the general view of natural science, like in natural history, but he did not use the old three “empires” (or habitats) (mineralogical, animal and vegetable), he still regarded that matter and the forces were divided in three parts.

From 1838 to 1847 Stoeckhardt was a teacher of natural sciences at “Koenigliche Gewerbschule” (Royal Vocational School) in Chemnitz, he taught experimental physics (4 h.), experimental chemistry (4 h.), technical chemistry (3 h.), practical courses in chemistry (8 h.) and some hours on botany, mineralogy and natural history. Stoeckhardt’s view of the nature was represented in his schedule.

## **Schule der Chemie – one of the most popular books of the nineteenth-century century**

With regard to Stoeckhardt's role in didactics we must discuss his book, "Schule der Chemie" (first edition in 1846). This was a very successful book, 19 editions were made by Stoeckhardt, the book was translated to several foreign languages (the translation to Japanese was the first chemical textbook in Japan<sup>31</sup>). In the foreword to the second edition Stoeckhardt again had a critical look at the methods of teaching. He did not agree completely with Bacon, who said that it is dangerous for teaching a science to choose a strong systematic way in teaching it. Stoeckhardt defended the opinion that for beginners, every teacher should prefer a systematic course. Stoeckhardt "translated" the strong scientific language into popular descriptions. He chose examples and experiments from the surroundings of the readers. But his book was not a typical popular exposition, primarily it was a textbook, he wrote this book for use in his own chemistry lessons at "Gewerbschule". But a second reason was indeed a popular. Stoeckhardt recommended stores in the book, where the readers could buy chemical compounds and apparatus. He gave a lot of examples of experiments which were easy and connected with experiences of the readers. Some of the experiments are useful even in our days; you can find some of them in current didactic papers (for instance the experiment of boiling water with the help of ice<sup>32</sup>). In describing the chemical problems Stoeckhardt often followed the nomenclature of a "describing" science. Even in the last edition he edited of "Schule der Chemie" organic chemistry is chemistry of vegetable and animal matter. Only Ernst Lassar-Cohn (1858-1922) (who edited the "Schule der Chemie" after the death of Stoeckhardt) used in his editions the already well established definition of organic chemistry as chemistry of the carbon compounds. So we can deduce that Stoeckhardt followed only to some degree new theories in chemistry. In 1868 he discussed the new atomistic theories but in organic chemistry he used the old theory. He tried to find a system for his elements using a parabolic arrangement,<sup>33</sup> but he never mentioned the system of Dmitrij I. Mendeleev (1834-1907).

It is difficult to explain why his book was so successful. You can find other books with similar experiments, written in a popular manner. For instance Emil Postel (1813-1875) wrote the book "Laienchemie"<sup>34</sup> (Chemistry for amateurs) in 1857. He emphasised that his book was much cheaper than the "Schule der Chemie"! Postel's experiments do not differ much from those of Stoeckhardt. Postel preferred a style which connects the author with the reader (he often use "we are doing" etc). We do not know the exact difference in the price, only that Stoeckhardt's book had many more editions than Postel's. However, we know that Adolf

von Baeyer (1835-1917), Emil Fischer (1852-1919) and Wilhelm Ostwald (1853-1932) all used the “Schule der Chemie”.

### **Wilhelm Ostwald, Julius Wagner and the first Professorship of Didactics in Chemistry**

In 1903/04 Wilhelm Ostwald wrote a “Schule der Chemie”,<sup>35</sup> too. There are of course a lot of differences in the contents because the 60 year interval brought a considerable development in chemical knowledge. A big difference can be observed in style. Ostwald used a very old method, the dialog between a teacher and a pupil. Ostwald was very engaged in problems of teaching chemistry at school.<sup>36</sup> There are some legends which refer to the very good teaching style of Ostwald.<sup>37</sup> Ostwald taught mathematics and natural sciences in a school in Dorpat for one year. When he came to Leipzig in 1887 he established a practical course for teachers. He mentioned that it is not useful for teachers to have *only* an all-embracing experience in chemical analysis the future teachers must have primarily experiences in doing school experiments. This special course was supervised by Julius Wagner<sup>38</sup> who had passed a special examination for teachers but he never taught chemistry at school. Nevertheless Wagner was very successful in his courses. There is no information about the contents of these courses. We only know that Wagner lectured didactics in chemistry. There is no information about his research work in didactics; there are some Doctoral theses which Wagner had supervised. But it is difficult to distinguish if the first initiative came from Wagner or from Ostwald. However, Ostwald strongly supported Wagner activities. It was Ostwald, who applied for an extraordinary Professorship in didactics for Wagner. This was established in 1901 and from 1904 with a budget. There are only two didactic papers written by Wagner. The first was his inaugural lecture on the occasion of his Professorship<sup>39</sup>. He dealt with the chemistry lessons for beginners in school because he thought that it is useful if university teachers gave recommendations for schools since universities are confronted with the results of school teaching. He compared the different methods in teaching: the academic or systematic one, the methodical and the historical one. He underlined the importance to start with well-known things, which are easy to understand and then to go stepwise to more complicated problems (this method was used by Rudolf Arendt (1828-1902)). Wagner developed his own course, summarised as follows:

1. Distinction of substances on the basis of properties
2. Separation of substances on the basis of differences in their properties
3. Physical changes of substances (changes of the aggregation state)

4. Separation on the basis of changing the aggregation state
5. Chemical changes of substances, Separation, Synthesis
6. Quantitative laws about the constitution of a compound
7. Reverse reactions and the chemical equilibrium
8. Rate of the reaction and the acceleration of the reaction with the help of extrinsic objects, which has the name catalysis.<sup>40</sup>

It is interesting that there are similarities in the contents and in the sequence of teaching to actual strategies of teaching chemistry for beginners. When the pupil finished this course he was prepared for systematic instructions of chemistry, which should be done only at higher schools. Concerning the practical work Wagner proposed the following way:

“to measure and to weigh, to do experiments on the properties of the substances, to change the volume by changes of pressure and temperature, to do experiments on the density, to change the aggregate state, to separate substances: filtrate, distillate, sublimate, dissolve; estimation of water in the vitriol of copper, estimation of water and carbon dioxide in  $\text{NaHCO}_3$ , estimation of the amount of oxygen which is produced from mercuric oxide, instruction in the synthesis of simple organic compounds, experiments with reversible reactions, on equilibrium and on the reaction rate, simple gravimetric and volumetric determinations”.

In his second paper<sup>41</sup> Wagner demanded that university teachers had to develop teaching methods for school, they had to stress that chemical courses at school are necessary for general education. He criticised that teachers were badly educated, that teaching methods were not thought over and that good school-textbooks were not available. Wagner himself tried to improve the education of teachers. But his results in developing new methods or new textbooks were very poor. After Ostwald left Leipzig University Wagner did not write any more didactic papers but he continued to carry out the special practical courses for teacher students. With the Professorship he had the chance to do a lot for “Chemistry in school”, but he did not achieve as much as might have expected.

## Conclusions

This paper is an attempt to describe factors which influenced the chemical education in the nineteenth century in Germany. The centre point is discussion the opinions of Stoeckhardt on teaching chemistry and “nature” respectively. He picked up ideas from the pedagogy of pietism and philanthropism and he used

several schemes which seemed useful to him to describe nature for school education. Although never special trained in pedagogy or didactics he was successful in classes, in papers and in lectures.

Some 60 years later Ostwald made demands on such special training for teachers and he introduced courses for prospective teachers. He tried to institutionalise the didactics of chemistry with the establishment of the first German Professorship for this subject.

## Notes and references

<sup>1</sup> A didact (*Didaktiker*) is a university, college or school teacher who has a scientific interest in the know-how of teaching itself and the contents of teaching.

<sup>2</sup> Compare for example: Julia Freienberg, Waltraut Krueger, Gabriele Lange, Alfred Flint, "Chemie fuer Leben auch schon in der Sekundarstufe I – Geht das?", *Chemkon* 2 (2201): 67 and 1 (2002): 19.

<sup>3</sup> See for example: <<http://www.chik.de>>

<sup>4</sup> Dorothea Goetz, "Naturwissenschaftliche Aspekte der Deutschen Aufklaerung", *Jahrbuch fuer Wissenschaftsgeschichte* (1974) II: 99-120;

Christoph Meinel, "Das Forschungslaboratorium und die Organisation des chemischen Hochschulunterrichts", in *Naturwissenschaftsdidaktik: Sommersymposium Essen 1993*, Naturwissenschaft und Unterricht – Didaktik im Gespräch, Bd 21, ed. Altfried Gramm, Helmut Lindemann und Elke Sumfleth (Essen: Westarp, 1994), 187–206;

Regine Zott, *Gelehrte im Fuer und Wider* (Muenster: LIT, 2002).

<sup>5</sup> Dorothea Goetz, "Der Anteil des Pietismus an der Herausbildung des naturwissenschaftlichen Unterrichts im 18. Jahrhundert", *NTM-Schriftenreihe Naturwissenschaften, Technik, Medizin* 11 (1974): 95-102.

<sup>6</sup> Anna Margret Halasik, *Der Chemieunterricht waehrend des 19. Jahrhunderts im Rheinland, Beitrag zur Geschichte des Chemieunterrichts im 19. Jahrhundert; dargestellt an ausgewählten Beispielen aus dem Rheinland* (Witterschlick/Bonn: Verlag M. Wehle, 1988).

<sup>7</sup> Werner Bonnekoh, *Naturwissenschaften als Unterrichtsfach* (Frankfurt/Main: Lang, 1992).

<sup>8</sup> Walter Schoeler, *Geschichte des naturwissenschaftlichen Unterrichts im 17. bis 19. Jahrhundert* (Berlin: Walter de Gruyter, 1976).

<sup>9</sup> Norbert Just, *Geschichte und Wissenschaftsstruktur der Chemiedidaktik* (Muehlheim: Westarp, 1989).

<sup>10</sup> Bernadette Bensaude-Vincent, Anders Lundgren (ed.), *Communicating Chemistry: textbooks and their audiences, 1789-1939* (Canton, MA: Science History Publications/USA, 2000).

<sup>11</sup> Bettina Haupt, *Deutschsprachige Chemielehrbuecher (1775-1850)* (Stuttgart: Dt. Apotheker-Verlag, 1987).

<sup>12</sup> Jan Frercks, Michael Markert, "The Invention of Theoretische Chemie: Forms and Uses of German Chemistry Textbooks, 1775-1820", *Ambix* 54 (2007) 2: 146-171.

<sup>13</sup> Andreas W. Daum, *Wissenschaftspopularisierung im 19. Jahrhundert: buergerliche Kultur, naturwissenschaftliche Bildung und die deutsche Oeffentlichkeit, 1848-1914* (Muenchen: Oldenbourg, 1998).

<sup>14</sup> Regine Zott, Emil Heuser, *Die streitbaren Gelehrten: Justus Liebig und die preußischen Universitäten* (Berlin: ERS-Verl., 1992) and other papers and books about Liebig such as Adolf von Baeyer, *Liebigs Verdienste um den Unterricht in den Naturwissenschaften* (München: C. Wolf und Sohn, 1892); Willi Conrad, *Justus von Liebig und sein Einfluss auf die Entwicklung des Chemiestudiums und des Chemieunterrichts an Hochschulen und Schulen* (Darmstadt: TH Darmstadt, 1985); Regine Zott, "Liebig und Ostwald, Praktiker und Theoretiker von Chemiebildung", in *Chemie in der Schule* 37 (1990) 10: 369-374; William H. Brock, *Justus von Liebig* (Braunschweig/Wiesbaden: Friedrich Vieweg & Sohn VerlagsGmbH, 1999).

<sup>15</sup> Hubert Laitko, "Die Disziplin als Strukturprinzip und Entwicklungsform der Wissenschaft – Motive, Verläufe und Wirkungen von Disziplinengenese", in *Verhandlungen zur Geschichte und Theorie der Biologie*, Vol. 8 (Berlin: VWB, 2002), 19-55.

<sup>16</sup> Martin Guntau, "Wissenschaftliche Ideen und wissenschaftliche Kommunikation in der ersten Hälfte des 19. Jahrhunderts", *Rostocker Wissenschaftshistorische Manuskripte* 20 (1991): 9.

<sup>17</sup> Wolf Lepenies, *Das Ende der Naturgeschichte: Wandel kultureller Selbstverständlichkeiten in den Wissenschaften des 18. und 19. Jahrhunderts* (Frankfurt(Main): Suhrkamp, 1978).

<sup>18</sup> Education in Humanistic High Schools was focussed on Latin and Greek. In the "Realgymnasium" there education in natural sciences played a larger role. This type of school was established in the beginning of the nineteenth century and formal recognised in the middle of that century. The certificate of the Realgymnasium allowed the enrolment at university, at the beginning with some limitations.

<sup>19</sup> If we distinguish pedagogy, methodics, didactics we have to define the contents of these items: pedagogy (from the greek word „*paideia*“) is the knowledge about education, instruction/teaching and learning. Didactis (the greek „*didaskhein*“ means teach) is a part of pedagogy, in the focus we find the question of teaching- and learning matter (methodics concerns the know-how of teaching). Sometimes there the word "Mathetics" is used, too. This word is based on Johann Amos Comenius (1592-1670) who divided in didactics (the "art" of teaching) and mathetics (the "art" of learning"). I do not use this strong disjunction.

<sup>20</sup> Biographical data can be found in: Ernst Theodor Stoeckhardt, *Stammtafel der Familie Stoeckhardt*, 1883, maschinengeschriebener Auszug von Regina und Hilmar Stoeckhardt, angefertigt 1982; Otto Wienhaus, "Julius Adolph Stoeckhardt - a pioneer of applied chemistry", *Fresenius J. Anal. Chem.* 363 (1999): 139-144; Otto Wienhaus, Walter Loescher et al., "Julius Adolph Stoeckhardt - ein Wegbereiter fuer die interdisziplinäre Arbeit, die Zusammenarbeit mit der Praxis und die Popularisierung wissenschaftlicher Erkenntnisse", *Z. Chem.* 26 (1986) 8: 269-275; Friedrich Nobbe, "Julius Adolf Stoeckhardt", *Tharander Forstliches Jahrbuch* 37 (1887): 76-85. Max Wilsdorf, "Julius Adolf Stoeckhardt und seine Thätigkeit in Chemnitz", *10. Jahresbericht der Landwirtschaftlichen Schule* 10 (1887): 5-15.

<sup>21</sup> Herbert Poenicke, "Georg Paul Alexander Petzholdt; ein mitteldeutscher Naturforscher und Lehrer in Rußland (1810-1889)", *Hamburger mittel- und ostdeutsche Forschungen: kulturelle und wirtschaftliche Studien* 2 (1960): 47-70.

<sup>22</sup> "Progymnasium" is a preparatory school for the High School, "Gelehrtengymnasium" is another name for Humanistic High school, for that and for "Realgymnasium" compare 18.

<sup>23</sup> Karl Christian Snell, *Ueber Zweck und Einrichtung eines Realgymnasiums* (Dresden and Leipzig: Arnoldsche Buchhandlung, 1834).

<sup>24</sup> Julius Adolph Stoeckhardt, *Res Naturales qua de causa perscrutandae, qua methodo docendae et tractandae, quomodo naturae convenienter disponendae* (Dresden: B. G. Teubner, 1837), translation from Latin to German unpublished, my thanks for translation to Friederike Neumeyer and Daniel Faustmann.



- <sup>25</sup> Johann Joachim Langen, *Caroli Linnaei...Systema naturae, sive regna tria naturae systematice proposita per classes, ordines, genera et species = Natur-Systema, oder die in ordentlichem Zusammenhange vorgetragenen drey Reiche der Natur, nach ihren Classen, Ordnungen, Geschlechtern und Arten* (Halle: Gebauersche Schriften, 1740).
- <sup>26</sup> August Hermann Francke, *Ausfuhrlicher Bericht vom Waysen-Hause, Armenschule und uebriger Armenverpflegung zu Glauchau an Halle* (Glauchau: Verlag des Waysenhauses, 1701).
- <sup>27</sup> Ehrenfried Walter von Tschirnhaus, *Gruendliche Anleitung zu nuetzlichen Wissenschaften, absonderlich zu der Mathesi und Physica, wie sie anitzo von den Gelehrtesten abgehandelt werden* Faksimile-Neudruck der 4., vermehrten und verbesserten Auflage Frankfurt und Leipzig 1729 (Stuttgart-Bad Cannstatt: Friedrich Frommann Verlag, 1967).
- <sup>28</sup> Kai Torsten Kanz, "Biologie: die Wissenschaft vom Leben? – Vom Ursprung des Begriffs zum System biologischer Disziplinen (17. bis 20. Jahrhundert)", in *Lebenswissen, Eine Einfuehrung in die Geschichte der Biologie*, ed. Ekkehard Hoextermaann and Hartmut H. Hilger (Rangsdorf: Natur&Text in Brandenburg GmbH, 2007): 100-121.
- <sup>29</sup> Stoeckhardt, *Res Naturales*, addendum.
- <sup>30</sup> Julius Adolph Stoeckhardt, *Die Schule der Chemie oder erster Unterricht in der Chemie, versinnlicht durch einfache Experimente: zum Schulgebrauch und zur Selbstbelehrung, insbesondere fuer angehende Apotheker, Landwirthe, Gewerbetreibende etc.* (Braunschweig: Vieweg, 1881), 515 (this is the last edition made by St.).
- <sup>31</sup> Personal communication Otto Wienhaus.
- <sup>32</sup> Tönjes de Vries, "Der Eiskocher", *Chemkon* 9 (2002) 4: 199-200.
- <sup>33</sup> Stoeckhardt, *Schule der Chemie* (Braunschweig: Vieweg, 1847), 3d edition, 403-405.
- <sup>34</sup> Emil Postel, *Laienchemie oder Leichtfassliche, an einfache Versuche geknuepfte Darstellung der Hauptlehren der Chemie* (Langensalza: Schulbuchhandlung, 1857).
- <sup>35</sup> Wilhelm Ostwald, *Die Schule der Chemie* (Braunschweig: Friedrich Vieweg und Sohn, 1903).
- <sup>36</sup> For example the following papers of Wilhelm Ostwald, *Die Forderung des Tages* (Leipzig: Akadem. Verlagsgesellschaft, 1910); *Naturwissenschaftliche Forderungen zur Mittelschulreform* (Wien: Manz, 1908); "Die heutige Schule im Widerspruch zur Wissenschaft und zum Leben", *Blaetter fuer deutsche Erziehung* 11(1909) 5: 67-75; "Diskussionsbeitrag zum Biologieunterricht an hoeheren Schulen", *Verhandlungen der GDNAE* (1904), part 1: 155-56; A very good overview you can get from the letters of A.v. Baeyer, R. Abegg and W. Ostwald (Regine Zott, *Gelehrte im Fuer und Wider* (Muenster: LIT, 2002). Compare also: James Altena, Klaus Hansel (ed.), "Wilhelm Ostwald Gesamtschriftenverzeichnis Band 1", *Mitteilungen der Wilhelm-Ostwald-Gesellschaft zu Großbothen e.V.* (2003) Sonderheft 14; Konrad Krause, Ulf Messow, *Wilhelm Ostwald – sein Wirken als Hochschullehrer und seine Auffassungen zur Ausbildung von Chemikern, zum Hochschulunterricht und zum Erziehungswesen* (Leipzig: Karl-Marx-Universitaet, 1983); Regine Zott, "Liebig und Ostwald, Praktiker und Theoretiker von Chemiebildung", *Chemie in der Schule* 37 (1990): 369-374.
- <sup>37</sup> Wilhelm Ostwald, *Lebenslinien* (Berlin: Klasing&Co., 1933), vol. I, 169.
- <sup>38</sup> Biographical data: Universitaetsarchiv Leipzig, Personalakte Julius Wagner, PA 1025, f. 2; Wilhelm Boettger, "Julius Wagner", *Zeitschrift fuer angewandte Chemie* 38 (1925): 309-310; Gisela Boeck, "Julius Wagner Deutschlands erster Professor fuer Didaktik der Chemie", *GDCh, Fachgruppe Geschichte der Chemie, Mitteilungen* 19 (2007): 169-183.
- <sup>39</sup> Julius Wagner, *Ueber den Anfangsunterricht in der Chemie* (Leipzig: Barth, 1903).
- <sup>40</sup> Wagner, *Ueber den Anfangsunterricht*, 23-24.
- <sup>41</sup> Julius Wagner, "Ueber den chemischen Unterricht an hoeheren Schulen", in *Beitraege zur Frage des naturwissenschaftlichen Unterrichts an den hoeheren Schulen*, ed. Max Verworn (Jena: Gustav Fischer, 1904): 47-69.

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## Trondheim or Oslo? Territories in Early 20th Century Chemistry Education in Norway

*Annette Lykknes;\* Ola Nordal\*\**

The Royal Frederik University in Oslo (founded in 1811) and the Norwegian Institute of Technology (NTH) at Trondheim were the two main facilities for chemical education in Norway in the first half of the 20<sup>th</sup> century. The University provided traditional chemistry training for science, pharmacy and medicine students, whereas NTH educated engineers for chemical industry. From the opening of NTH in 1910 the Institute was regarded as an integrated teaching and research institution and provided with modern laboratories,<sup>1</sup> the chemistry department being one of the largest departments had its own building and four laboratories.<sup>2</sup>

Initially the NTH and the University in Oslo were not regarded as competitors, as the university did not educate engineers and NTH did not train scientists. However this position changed in the early 1920s. The University's chemistry laboratory had for a long time suffered from inadequate gas, water and ventilation systems, a subsiding building as well as cramped localities, which made it inadequate even for the elementary courses – educating chemists was reckoned as almost impossible under such conditions. Plans for a new chemistry laboratory at the new university campus, Blindern, were initiated, culminating in a new building, inaugurated in 1934/35.<sup>3</sup>

In 1922 the Norwegian Chemical Society appealed in the daily press about the need to build a new chemistry laboratory. By overtly mentioning the education of (industrial) chemists, the relationship between NTH and the University was exacerbated, creating numerous newspaper discussions. At the NTH it was felt that the University was trespassing upon their domain. This debate occurred during a time when NTH experienced a stream of professors moving from Trondheim to Oslo, as the Chairs at the University were still regarded as more attractive than

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a position at NTH. In the lecture the controversy between the two chemistry laboratories was discussed in more detail, exemplifying “neighbours and territories” within the same field and country.

## Notes

<sup>1</sup> Roland Wittje, “The Foundation of N.T.H. in 1910 in International Context,” in: Reinhard Siegmund-Schultze and Henrik Kragh Sørensen (eds.), *Perspectives on Scandinavian Science in the Early Twentieth Century* (Oslo: Novus Press, 2006), pp. 111-132.

<sup>2</sup> Annette Lykknes, Lise Kvittingen and Elisabeth Egholm Jacobsen, “Founding of a chemistry laboratory at Norway’s first Institute of Technology: Laboratory practices 1910-1936” in: Isabel Malaquias, Ernst Homburg and M. Elvira Callapez (eds.), *5th International Conference on History of Chemistry – “Chemistry, Technology and Society” – proceedings* (Estoril & Lisboa: SPQ – Sociedade Portuguesa de Química, 2006), pp. 214-222.

<sup>3</sup> Annette Lykknes, Lise Kvittingen, and Anne Kristine Børresen, “Ellen Gleditsch: Duty and Responsibility in a Research and Teaching Career, 1916-1946,” *Historical Studies in the Physical and Biological Sciences* 36 (2006), pp. 131-188.

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## Hungarian University Chemistry Buildings, 1860-2006

*Éva Vámos\**

Laboratories –as sites of scientists’ work and proofs of the correctness of their ideas, which have launched scientific schools spanning several generations– has become a focus of interest of historians of science in the past two decades.<sup>1</sup> This paper is mainly concerned with the laboratory founding activity of the great chemist, Károly Than (1834-1908). Than was one of the first generation of scientists of the epoch of the dualistic Hapsburg Monarchy (1867-1918), designing and building the most up-to-date building of chemistry of the epoch, in Hungary. This was the building in the former Botanical Garden (today, Trefort Garden, Budapest, 8<sup>th</sup> district) established by Károly Than.<sup>2, 3</sup> The building for chemistry designed upon Rudolf Fabinyi’s initiative at Kolozsvár (today Cluj, Romania) University (1883)<sup>4</sup> and the chemistry building of József Technical University (1903), in the bringing into being of which Vince Wartha played a serious part<sup>5</sup>, were also designed under the influence of this pioneering period.<sup>6</sup> The chemistry building of Budapest University of Sciences served as example for the establishment of the institutes of chemistry of Rome, Birmingham, Graz, Aachen and Boston. The choice to discuss the laboratories of the epoch of Dualism was because the direct chain of descent of today’s Hungarian Schools of Chemistry can all be traced back to them.

### The Institute of Chemistry of Budapest University of Sciences

#### *The coming into being of the new chemistry building*

The year 1860 gave rise to a fundamental change in the lives of Hungarian universities on their way of modernisation. The fall of neo-absolutism and the re-introduction of training in the mother tongue brought about the appointment of new Professors and, from 1867 on, a high-speed university reform. For chemical education this meant that, upon the recommendation of the Professors Redtenbacher (Vienna), Bunsen (Heidelberg) and Wertheim (Budapest - Graz), the 25-year-old

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**Institute of Chemistry of Budapest University of Sciences**



**Picture representing Károly Than in his young years**

Károly Than was appointed head of the then only Department of Chemistry.

As a characterisation of the situation of the time it can be said that science in Hungary, in general, and also chemistry were lagging behind Western Europe by at least half a century. Than wrote about this as follows, “... When I entered the Department of the Royal Hungarian University, I took over a small chemical laboratory, the lecture room had 90 seats, and in the corridors only 15 laboratory benches were installed. Not much later it turned out that establishing a new institute meeting the present demands of science could not be further delayed without jeopardising the success of training. As it is, from the year 1860 on, the number of students and trainees of chemistry increased so fortunately that the

number of the former exceeded 300, and that of the latter 50 persons.”<sup>7</sup> From 1863 on, Than urged a new building of chemistry.

The plan of a new chemistry building was included in the programme of the university reform which started in 1867, the Minister of Education, Baron József Eötvös, was its greatest advocate. The Monarch approved the establishment of the new building on May 29, 1867.<sup>8</sup> Károly Than, who had already travelled about Europe, was sent on a special study tour to visit modern chemistry buildings as well as to collect views and advice for establishing the new building. Than knew Bunsen’s Institute of Chemistry in Heidelberg quite well, so he visited the universities of Leipzig, Bonn and Berlin. In Berlin he invited the architect Friedrich Zastrau to help with the interior, owing to his experience, János Wágner<sup>9</sup>, who had been entrusted with the implementation of the plans.<sup>10</sup> The present author was lucky to find, in the archives of the Giessen Liebigiana, a copy of Károly Than’s letter to Liebig<sup>11</sup> in March 1870, in which he expressed his thanks to the German scientist for his advice related to the construction of the new chemistry laboratory. The first part of the letter reads as follows:

*“Much respected Baron!*

*In my laboratory I prepared tellurium from the Transylvanian ore <Nagyágit>. Hoping that you will be willing to accept it, I take the liberty of sending you a small sample of this tellurium by today’s mail. Receive it as a small token of my deep respect and gratitude for the gentle and valuable advice related to the new chemistry laboratory to be built here, you were so kind as to impart to me on the occasions of my visits. The construction of the laboratory here has, to a greater part been finished, the works of furnishing are now starting so that hopefully it will be possible to open the Institute in October. As soon as the description is ready, I’ll take the liberty to send you a copy, ...”*

The construction was started in 1868, and in autumn 1871 education started in the new building. According to Than’s objectives, “we have had to establish a building, in which 280-300 students can attend lectures in chemistry completed by experiments, and simultaneously 70 trainees – among them about 20 advanced ones or performing independent research – may participate in chemical practice. Further it has had to be made possible at the Institute that it allows carrying out independent scientific research in a comfortable way as well as the production of chemical preparations, and the performance of chemical operations on a larger scale.”<sup>12</sup>





The great lecture hall of the onetime chemistry building of the Budapest University of Sciences.

One of the aims in establishing the Institute, among others, was to make the performance of experiments of public interest possible, in a big lecture hall before a wide audience.

The new building was erected on the ground of the old Botanical Garden as first building for the Institutes of Natural Sciences. According to the habits of the epoch the building included lecture rooms, laboratories, store-rooms and residential flats, among the latter, a flat for the leading Professor. When designing the building, the saving of finances and space were to be taken into account. From the aspect of the building it was lucky that it was 70 m away from the main road, and that it stood free on every side, i.e. without any neighbours.

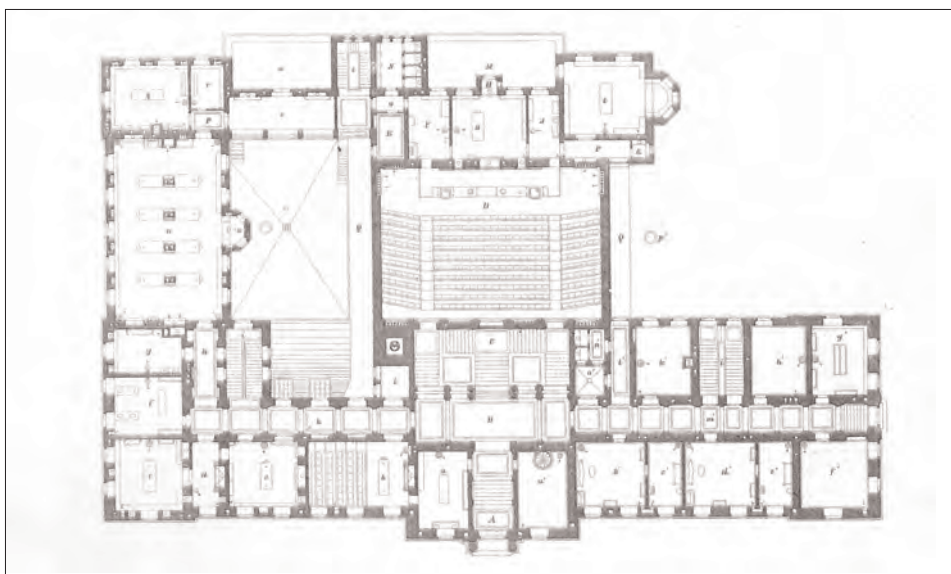
Entering the lobby, we reach, even today, the beautiful staircase, adorned by columns, where round reliefs by E. March can be found on the walls, containing portraits of the world's famous chemists.

On the landing there is a bust of Baron József Eötvös, the great supporter of the building's construction. From the first floor there is the entrance, through two doors, for students to the big lecture hall that can seat 290 persons. At the rear wall of the lecture hall were placed the portrait reliefs of Than's two great heroes, Liebig and Bunsen, who were then still alive.



### Portraits of chemists in Károly Than's chemistry building

<i>Name</i>	<i>Dates of birth and death</i>	<i>Name</i>	<i>Dates of birth and death</i>
Henry Cavendish	1731-1810	John Dalton	1766-1844
Joseph Priestley	1733-1804	Jakob Berzelius	1779-1847
Eilhard Mitscherlich	1794-1863	Wilhelm Scheele	1742-1786
Leopold Gmelin	1788-1853	Joseph-Louis Gay-Lussac	1778-1850
Heinrich Rose	1795-1864	Angus Laurent	1807-1853
		Charles Gebhardt	1816-1856
Laurent Lavoisier	1743-1794	Robert Wilhelm Bunsen	1811
Humphry Davy	1778-1803	Justus Liebig	1803



Groundplan of Than's Chemistry Building

According to Than's detailed description<sup>13</sup> the 10 big windows of the big lecture hall could be completely blacked out, for optical experiments. The bench, which served to demonstrate experiments had as its model the experimental table of the Berlin Institute. On it there were two big tubs filled with mercury and water, respectively; gas and water taps, and in front of the table a small stone socket for measuring instruments sensitive to vibrations. On the rear wall of the lecture hall there were two cupboards for chemicals, and at the left end of the bench for exper-

iments there was a sliding table which serve to prepare devices, necessary for the lectures. Close to the two doors behind the table for experiments there were two glass cases exhibiting the most important chemicals. The two doors led to a preparation room, a workshop, and a room, where the Professor could talk to the students.

Below the lecture hall, in the basement, there were two store-rooms, one for glass and chinaware, and the other for raw materials (chemicals). Besides them there was an ice-chamber, and in another room an alcohol-driven Otto-engine for operating the lathe, the compressor, the blast engine and the ventilation system. All these together made the presentation of demonstration experiments possible.

The other great hall (on the ground floor) was set up, because of Than's personal conviction, for the practical education of freshmen. There were laboratory benches for 50 persons, at one end there was a one-foot high platform with the Professor's bench for experiments. The shelves for chemicals were placed in a way that allowed a view over the whole laboratory. The aim was to enable the students to immediately repeat the experiments demonstrated by the Professor before they would forget the instructions.

Fume hoods after Hofmann's design were placed at the end of the desks. In the freshmen's laboratory there were a copper drying oven with 15 compartments, and a refrigerating apparatus.

At the disposal of the students there was also room for experiments with a big desk or bench, and three balconies (two on the ground floor, and one on the first floor), for experiments that require sunlight, or which released great amounts of vapours or gases.

The advanced students and those engaged in independent research had six smaller laboratories at their disposal. The benches there were equipped with blast-pumps after Bunsen. Every two trainees had to share a fume hood. To each two such laboratories there was a room for balances, and another equipped for experiments requiring fire.

On the first floor there were 5 rooms for more delicate experiments: one for spectroscopic investigations, one for electrolysis, one for gas analyses, one for investigations into chemical bonds, and one for experiments at high temperatures, e.g. determinations of gas densities or heating of closed tubes, etc. For studies into the chemical properties of daylight as well as for the comparison of the spectra of celestial and terrestrial bodies, a bench was erected on the highest central point of the building: on the flat roof.

In the basement there was a separate room for distilling inflammable substances. It contained two iron apparatuses, to which vessels of different materials could be connected, and hermetically sealed. The distilling devices were connected to the steam kettles. In this room it was prohibited to light a fire. (Steam kettles were placed in the kettle-house).

There was a room with furnaces, and a copper kettle for preparing distilled water. There was a larger room for larger scale experiments with a large bench, and some smaller ones. There was also an oven for heating tubes, a large water bath for steam distillations, and a quite strong filter-press. The corner room with three windows served for distillations and evaporations over an open flame.

The lodgings of the staff as well as the stores for delicate apparatuses and instruments, and the rooms, in which the latter were used, were placed in the right wing of the building. Thus, being far from the laboratories, they were spared the harmful effects of noxious vapours.

A general strong air flow was indispensable for the Institute as space in the laboratories was used to the full. When choosing the heating and air flow systems, besides economic aspects, special attention was paid to the requirements of hygiene.

Following the example of the Leipzig Institute, a combined steam & water heating was adopted, with slight modifications, manufactured by the well-known Swiss machine factory of the Sulzner Brothers in Winterthur.

The gas and water pipes as well as the sewage conduit were placed in a common channel that was easy to open.

Seven staircases and many water taps provided for security in case of fire. Talking tubes and electric bells [operated by galvanic cells] served for communication. At the time, there were distribution systems for gas and water but there was no electric lighting.

As already mentioned, the architectural designs were made by the Hungarian János Wagner, and the interior equipment was the work of the Berlin architect Frederic Zastrau. Contemporaries complained much during the construction, about the lack of sensible labour and management as well as about the building materials growing more expensive during the process of construction. Károly Than himself complained of the fact that, owing to the necessity of supervising the construction, he was compelled to learn many things that had nothing to do with chemistry, and therefore had to suspend research work for years.

***Károly Than, the chemist***

The greatest part of Károly Than's scientific work was carried out in the new building.<sup>14</sup> Among his books considered important are "The elements of qualitative chemical analysis" (1895) and the two-volume work "The elements of experimental chemistry" (1897 and 1907). He rendered great service by helping to compile the first "Pharmacopoeia Hungarica."<sup>15</sup>

Than achieved remarkable results in general and inorganic chemistry, as well as in the then new field, physical chemistry. As he published nearly all his results in Hungarian journals, his work was not known or appreciated enough abroad. In 1908 Than retired, and died some weeks later, at the age of 74.<sup>16</sup>

One of Than's students and later assistant, Béla Lengyel was appointed full university Professor and head of the newly (still during Than's life) established Institute of Chemistry No. II.<sup>17</sup> His institute operated, however, for a long time in adverse circumstances, and it was only a few years before Lengyel's death, in 1909, that it got adequate space in building F, of Trefort Gardens, after the Technical University had moved out to its new premises.<sup>18</sup>

Lengyel's textbooks ("Chemistry", 1889, and "Elements of quantitative chemical analysis", 1896) filled a gap, at their day, in the Hungarian literature of the subject.

After Than's death, in 1908, his Department was divided in two parts, and the building was enlarged by a new wing in 1909.<sup>19</sup> Lajos Winkler was appointed head of the Institute of Chemistry No. I,<sup>20</sup> and Gusztáv Buchböck head of the Institute of Chemistry No. III.<sup>21</sup> Both had been earlier Than's co-workers. Thus, on the whole, 3 successor Departments of Chemistry tried to fill the gap that Than's death caused to chemistry in Hungary.

Winkler's activity in the field of classical chemical analysis was huge. He published over 200 papers in Hungarian and German journals, thus his fame reached far over the borders of the country. His name gained reputation from his method, developed in his doctoral thesis, "Determination of oxygen dissolved in water" (1888), which has since been referred to as Winkler's iodometric method, and is still in use, even today.

Gusztáv Buchböck was among the first to deal, in Hungary, with the relatively young branch of science of physical chemistry. His book "Physikai kémiai mérőmódszerek" (Physicochemical methods of measurement<sup>22</sup> was the first practical handbook in the field in Hungarian language).

## **The Institute of Chemistry of Kolozsvár University**

### ***The new building***

The coming into being of the new chemistry building of Kolozsvár (today Cluj, Romania), considered very modern at the time, is linked to Rudolf Fabinyi, appointed full Professor of Theoretical and Practical Chemistry on June 30, 1878.<sup>23</sup>



**The view of the Chemistry Building of Kolozsvár (Cluj) University**

The whole building, together with its furniture and equipment cost 170,000 Florins, in contrast to the cost of 351,000 Florins for the Budapest Institute, and some of the considerably higher expenditures of the foreign institutions listed in the introduction. The construction of the building started in autumn 1880 and was finished in 1883. The building was 49 m long and 15.5 m wide. In it, there were, besides the lecture hall and its offices on the first floor, 12 laboratories of different sizes for practical training and independent experimentation. There was also a room for the central heating equipment, another one for the machinery, and the store-rooms for materials, tools and instruments. For the storage of volatile, caustic and explosive materials there was a special cellar in the courtyard. Public training started in the building in October 1883.

***Rudolf Fabinyi, the chemist***

The monograph devoted to Fabinyi's life and work lists 94 papers written between 1874 and 1918, which encompass nearly all the branches of chemistry (analytical chemistry, electrochemistry, inorganic and organic chemistry). A considerable number of these papers (nearly a quarter) were published in foreign journals. Fabinyi was among the first in Hungary to recognise the role of electricity in chemical research. He established that the levelling off of high-voltage electricity induced synthetic processes in coal gases. He carried out research for practical purposes, too, for the industry was not remote from Fabinyi's interests as proven by his considerable number of patents.<sup>24</sup>

Fabinyi was a very active member both in University and Hungarian scientific public life, in general.<sup>25</sup> He was the first president of the Hungarian Chemical Society founded in 1907.<sup>26</sup> The Society celebrated its 100-year anniversary in 2007, and on this occasion a Rudolf Fabinyi prize was founded

The tempests of history did not allow Fabinyi's students to continue their work at his institute, and to accomplish the work of the outstanding master. After the peace treaty of Trianon (1920) all the Professors and other members of the staff that denied the oath of allegiance to the Romanian state, fled, together with Fabinyi, to the land remaining as Hungary.

**Chemistry building of József Technical University*****Establishing of the new chemistry building***

Chemist and university Professor Vince Wartha and architect Győző Cziegler were the two persons that suggested the open space in Lágymányos (Budapest, 11<sup>th</sup> district) as the site for the Technical University. The first building to be erected<sup>27</sup> on the campus of Budapest Technical University, was the Chemistry Building (1903), which, at the beginning, housed the Department of Electrochemistry and Chemical Technology.

***Vince Wartha, the chemist***

Vince Wartha's name is being mentioned today mainly in relation with the development of the so-called "eosin glaze" of the Zsolnay factory's ceramics. The founder of Hungarian training in chemical technology, however, achieved significant results in a much wider field. László Móra's monograph lists 662 publications



of Wartha.<sup>28</sup> His book, “The technology of ceramics industry” (1892), and its enlarged, two-volume version, “Pottery. The book of applied arts” (1905) were the first to treat the history of ceramics industry in Hungary.<sup>29</sup> The first volume of Wartha’s, “Chemical technology” appeared in 1906 and was, for decades, the only textbook in Hungarian language on the topic.<sup>30</sup> The completion of the Technical University’s buildings in Lágymányos took place during his second period as Rector.

### **Closing thoughts**

On the previous pages the coming into being of three up-to-date laboratories, citadels of chemistry in Hungary, was presented. It is hoped that it has been successfully demonstrated that to establish these laboratories was a task to test their founder’s courage. They were persons with thorough grounding in their trade and of outstanding character, who carried through these tasks so-to-say, as fanatics, and had to tackle many political and economic problems. The age was however, actually favourable to their initiatives, as those that lived at the epoch, enjoyed, until World War I, decades of quiet development. The ministers in charge, in the first place Baron József Eötvös and Ágoston Trefort, were highly responsible civil servants, who knew their jobs, and the experts they employed were particularly talented representatives of the age, trained at high-standard domestic and foreign universities and imbued with love of their country.

The most ancient institution was Pest (later Budapest) University of Sciences, where the struggle for bringing into being an adequate laboratory, had started as soon as in the early 1860s, Kolozsvár followed in 1883 and József Technical University in 1903.

The three great personalities that took upon themselves to carry through the tasks were all young men, on the right side of thirty. Károly Than was 25, Rudolf Fabinyi 29, and Vince Wartha 26, when, with the appointments as heads of department, they each assumed immense loads. It would be difficult to rank them. They were of very different characters, they started in the then Hungary’s different regions, and they came from different family circumstances. All three fulfilled what they undertook, they created adequate frameworks and good conditions for practising chemistry, and for promoting its domestic expansion.

For chemistry at the University of Sciences, in the 21<sup>st</sup> century a large new palace was constructed, at the other side of the Danube, where even the smallest lecture hall can hold 300 persons.



## Notes and References

<sup>1</sup> Bruno Latour, *Science in action. How to follow scientists and engineers through society*. (Milton Keynes: Open University Press, 1987), 63-100.

<sup>2</sup> Imre Szentpéteri, *A királyi magyar Pázmány Péter Tudományegyetem története*,. (*History of the Royal Hungarian University of Sciences*) (Budapest: Királyi Magyar Egyetemi Nyomda, 1935) vol. IV, 481.

<sup>3</sup> Károly Than was born in Óbecse (today Bečej, Serbia) in 1834. His father was manager of the local royal estate. One of his brothers, Mór Than became a well-known painter. While still a secondary school student, he joined the Hungarian army in the War of independence (1849) as apprentice gunner. According to his own words, it was then that his interest in chemistry was awakened. During his absence from home, his mother died and his father lost his job. Thus Károly had to provide for himself. He took up a job as trainee with the pharmacist Ferenc Kiss in Körösladány, then continued his work at Antal Simonides's pharmacy in Hódmezővásárhely. After the years of traineeship Károly went to Szeged as assistant pharmacist, where his understanding boss made it possible for him to finish his secondary school studies in 1855. He enrolled as a student of medicine in Vienna but after a year he switched to pharmacy as there he managed to obtain a scholarship. He was conferred the title of doctor in 1858, then – owing to a grant – went to Heidelberg for a year, where he was working mainly with the renowned Professor Bunsen. In 1859 he returned to Vienna, where he was first Assistant Professor at the Institute of Chemistry of the University, then obtained the title of *private docent* in pharmaceutical and analytical chemistry. It was then that he applied for the job of Professor at the Department of Chemistry of Pest University which he obtained. In October 1860 he was appointed extraordinary, in 1862 full Professor at the Department of Chemistry. – Ferenc Szabadváry, *A magyar kémia kultúrtörténete (Cultural history of Hungarian Chemistry)*, (Budapest: Mundus Magyar Egyetemi Kiadó, 1998), 181-183.

<sup>4</sup> Rudolf Fabinyi (1868-1941) was born in Jolsva, Upper Hungary (today Slovakia) in a middle-class family. He finished his secondary school studies at the Lutheran grammar school in Rozsnyó, then enrolled at the Faculty of Philosophy of Budapest University of Sciences. In 1871 he became Assistant Professor at the Department of General Chemistry of József Technical University as headed by Károly Nendtvich, then acquired a degree as secondary school teacher from the University of Sciences, where he was conferred the title of doctor in 1875. As a scholarship holder he was working for two years in Germany with the Professors of chemistry Wislicenus, Baeyer and Bunsen, and the Professor of Physics, Jolly. After his return home, he became Assistant Professor at the Technical University, and "*private docent*" in organic chemistry at the University of Sciences. In spring 1878 he travelled to Paris to work with the Professor of organic chemistry of the Sorbonne, Wurtz. In the same year he learnt that the Department of Chemistry of Kolozsvár University was vacant, owing to the death of its head. He applied for the job, and obtained it from among 17 applicants. – László Móra, *Fabinyi Rudolf élete és kora*, (*Rudolf Fabinyi's life and age*), (Budapest: Technika Alapítvány, 1999), 21. – László Móra, "Fabinyi Rudolf emlékezete születésének 150. évfordulóján", ("Commemoration of Rudolf Fabinyi on the occasion of the 150<sup>th</sup> anniversary of his birth"), *Magyar Kémikusok Lapja* 54 (1999): 285-289. – Béla Ruzicska: "Fabinyi Rudolf (Rudolf Fabinyi)" in *Százhuszonhat éve nyílt meg a Kolozsvári Tudományegyetem (Kolozsvár University of Sciences was inaugurated 126 years ago)*, ed. István Gazda (Piliscsaba: Magyar Tudománytörténeti Intézet, 1997) 122-123.

<sup>5</sup> Vince Wartha (1844-1914) was born in Fiume (today Rijeka, Croatia). His father was an officer of the army. After finishing his secondary school studies at the Piarists' Grammar School in

Szeged, he enrolled to Buda Polytechnical School. He continued his studies in Zurich, where he graduated from Eidgenössische Technische Hochschule as engineer in 1864. Then he returned home and became assistant to Károly Nendtvich at József Polytechnical School. Owing to the adverse conditions, he went abroad again, this time first to Bunsen in Heidelberg. After obtaining the title of doctor, he returned to Zurich, where he became first assistant at the university, later „private docent”. In 1867 he was invited by József Polytechnical School to head the Department of Mineralogy as substitute Professor. In 1870 he was appointed head of the recently established Department of Chemical Technology. Ferenc Szabadváry – Béla Szőkefalvi-Nagy, *A kémia története Magyarországon (History of Chemistry in Hungary)*, (Budapest: Akadémiai Kiadó, 1972) 203. József Németh, *A műegyetemtől a világhírig (From Technical University to world fame)*, (Budapest: Műegyetem Kiadó, 2005). – János Holló – Ferenc Szabadváry: *A Budapesti Műszaki Egyetem Vegyészmérnöki Karának centenáriumi emlékkönyve 1871-1971 (Memorial volume of the centenary of Budapest Technical University's Faculty of Chemistry 1871-1971)* (Budapest: Budapesti Műszaki Egyetem, 1972).

<sup>6</sup> József Németh, *A műegyetemtől a világhírig (From Technical University to world fame)*, (Budapest: Műegyetem Kiadó, 2005). – János Holló – Ferenc Szabadváry: *A Budapesti Műszaki Egyetem Vegyészmérnöki Karának centenáriumi emlékkönyve 1871-1971 (Memorial volume of the centenary of Budapest Technical University's Faculty of Chemistry 1871-1971)* (Budapest: Budapesti Műszaki Egyetem, 1972).

<sup>7</sup> Károly Than, “A Magyar Királyi Egyetem Vegytani Intézetének leírása öt táblával” (Description of the Institute of Chemistry of the Royal Hungarian University with 5 Tables) in *A Magyar Tudományos Akadémia évkönyvei (Yearbooks of the Hungarian Academy of Sciences)* (Eggenberger Ferdinánd Magyar Tudományos Akadémia könyvtárusánál, 1871) Reprinted from the book cited, 1-17.

<sup>8</sup> Imre Szentpéteri, *A királyi magyar Pázmány Péter Tudományegyetem története.*, (History of the Royal Hungarian University of Sciences, (Budapest: Királyi Magyar Egyetemi Nyomda, 1935) vol. IV, 481-482.

<sup>9</sup> János Wágner (1813-1904) studied in Vienna and Berlin, and was member of the Pest architects' guild. Some of his buildings that exist till today are: the block of flats in 14, Kossuth street (5<sup>th</sup> district, Budapest), the (today) physiology building of Budapest University of Sciences in Pushkin street without the second floor added later, and the characteristic block of eclectic-romanticising houses surrounding the Turk Gul Baba's tomb in Buda. The implementation of several important buildings in Budapest (e. g. the so-called Redoute) is his work as well, in: *Magyar Életrajzi Lexikon (Hungarian Biographical Encyclopedia)*, ed.-in-chief. Ágnes Kenyeres vol. 2, L-Z. (Budapest: Akadémiai Kiadó, 1969), 1018.

<sup>10</sup> Károly Than, “A Magyar Királyi Egyetem Vegytani Intézetének leírása öt táblával” (Description of the Institute of Chemistry of the Royal Hungarian University with 5 Tables) in *A Magyar Tudományos Akadémia évkönyvei (Yearbooks of the Hungarian Academy of Sciences)* (Eggenberger Ferdinánd Magyar Tudományos Akadémia könyvtárusánál, 1871) Reprinted from the book cited, 1-17.

<sup>11</sup> Éva Katalin Vámos: “Justus von Liebig magyarországi levélkapcsolatai (Justus von Liebig's Hungarian connections by correspondence),” *Technikatörténeti Szemle* 25 (2001-2002): 125. The original German letter is published in the paper, along with its Hungarian translation.

<sup>11</sup> Károly Than, “A Magyar Királyi Egyetem Vegytani Intézetének leírása öt táblával” (Description of the Institute of Chemistry of the Royal Hungarian University with 5 Tables) in *A Magyar Tudományos Akadémia évkönyvei (Yearbooks of the Hungarian Academy of Sciences)* (Eggenberger

Ferdinánd Magyar Tudományos Akadémia könyvtáránál, 1871) Reprinted from the book cited, 1-17.

<sup>12</sup> Károly Than, “A Magyar Királyi Egyetem Vegytani Intézetének leírása öt táblával” (Description of the Institute of Chemistry of the Royal Hungarian University with 5 Tables) in *A Magyar Tudományos Akadémia évkönyvei (Yearbooks of the Hungarian Academy of Sciences)* (Eggenberger Ferdinánd Magyar Tudományos Akadémia könyvtáránál, 1871) Reprinted from the book cited, 1-17.

<sup>13</sup> Károly Than, “A Magyar Királyi Egyetem Vegytani Intézetének leírása öt táblával” (Description of the Institute of Chemistry of the Royal Hungarian University with 5 Tables) in *A Magyar Tudományos Akadémia évkönyvei (Yearbooks of the Hungarian Academy of Sciences)* (Eggenberger Ferdinánd Magyar Tudományos Akadémia könyvtáránál, 1871) Reprinted from the book cited, 1-17.

<sup>14</sup> Ferenc Szabadváry – Béla Szőkefalvi-Nagy, *A kémia története Magyarországon (History of Chemistry in Hungary)*, (Budapest: Akadémiai Kiadó, 1972), 196-197. – Ferenc Szabadváry, *A magyar kémia kultúrtörténete (Cultural history of Hungarian Chemistry)*, (Budapest: Mundus Magyar Egyetemi Kiadó, 1998), 185.

<sup>15</sup> Ferenc Szabadváry, *A magyar kémia kultúrtörténete (Cultural history of Hungarian Chemistry)*, (Budapest: Mundus Magyar Egyetemi Kiadó, 1998), 185.

<sup>16</sup> Béla Lengyel (1844-1913) was born in Körösladány. His father, an estate manager, died early, and the widow moved to Pest with her son. After finishing secondary school, the young man started his studies at the Faculty of Engineering of the Technical University. He was in his fifth year, when he enrolled, for the sake of chemistry, at the University of Sciences as well. Finally he chose chemistry as his vocation. From 1866 to 1868 he was assistant to Than, in 1868 he was appointed head of the Department of Chemistry of the Debrecen School of Agriculture. Before taking up his post there, he went to Bunsen’s Institute in Heidelberg as a fellowship holder. Bunsen offered him a job as assistant, thus he renounced to his post in Debrecen. After obtaining his doctor’s title in Heidelberg, he returned home in 1870, and acquired the qualification of *private docent* at Pest University of Sciences. It was then that his career started. – Szabadváry – Szőkefalvi-Nagy, *A kémia története*, 198-199.

<sup>17</sup> The red brick building was originally built – together with a number of others – for the Technical University. As the latter moved to new premises on the other side of the Danube, the buildings in Trefort Garden were gradually emptied. They served as buildings for the Faculty of Sciences of Budapest University of Sciences even at the beginning of the 21<sup>st</sup> century, in *Budapest Lexikon (Budapest Encyclopedia)*, ed.-in-chief László Berza, vol. 1, A-K (Budapest: Akadémiai Kiadó, 1993), 375-376.

<sup>18</sup> Ferenc Szabadváry, *A magyar kémia kultúrtörténete (Cultural history of Hungarian Chemistry)*, (Budapest: Mundus Magyar Egyetemi Kiadó, 1998), 185.

<sup>19</sup> Lajos Winkler (1863-1939) graduated as pharmacist from Budapest University of Sciences (1885), then was working at Than’s Department as trainee. In 1889 he was conferred the title of doctor of pharmacy, in 1890 he became assistant, in 1893 „*private docent*” and senior lecturer, from 1902 on extraordinary university Professor, and from 1909 on Director of the Institute of Chemistry for nearly 25 years. – László Móra “Winkler Lajos” (Lajos Winkler), in *Magyar Tudóslexikon A-tól Zs-ig (Hungarian Scientists’ Encyclopedia)*, ed.-in-chief. Ferenc Nagy (Budapest: BETTER-MTESZ-OMIKK, 1997), 871-872. – F. Szabadváry, *Winkler Lajos (Lajos Winkler)*, (Budapest: Akadémiai Kiadó, 1975).

<sup>20</sup> Gusztáv Buchböck (1869-1935) had studied at Budapest University of Sciences (1896), then stayed with Than at his Institute, first as trainee, then as assistant, and finally, from 1904 on as „private docent”. Thereafter he went abroad for a longer study journey: in Leipzig he was working with Wilhelm Ostwald, and in Göttingen with Walter Nernst. – Ferenc Szabadváry: *Buchböck Gusztáv (Gusztáv Buchböck)*, in *Magyar Tudóslexikon*, 214-215.

<sup>21</sup> Gusztáv Buchböck, *Physikai kémiai mérő módszerek (Physicochemical methods of measurement)*. Budapest: Pester Lloyd, 1922, 1-382).

<sup>22</sup> Rudolf Fabinyi (1868-1941) was born in Jolsva, Upper Hungary (today Slovakia) in a middle-class family. He finished his secondary school studies at the Lutheran grammar school in Rozsnyó, he then enrolled at the Faculty of Philosophy of Budapest University of Sciences. In 1871 he became Assistant Professor at the Department of General Chemistry of József Technical University as headed by Károly Nendtvich, then acquired a degree as secondary school teacher from the University of Sciences, where he was conferred the title of doctor in 1875. As a scholarship holder he was working for two years in Germany with the Professors of Chemistry Wislicenus, Beyer and Bunsen, and the Professor of Physics, Jolly. After his return home, he became assistant professor at the Technical University, and “private docent” in organic chemistry at the University of Sciences. In spring 1878 he travelled to Paris to work with the Professor of Organic Chemistry of the Sorbonne, Wurtz. In the same year he learnt that the Department of Chemistry of Kolozsvár University was vacant, owing to the death of its head. He applied for the job, and obtained it from among 17 applicants. – Móra, *Fabinyi Rudolf emlékezete*, 285-289.

<sup>23</sup> László Móra, *Fabinyi Rudolf élete és kora, (Rudolf Fabinyi's life and age)*, (Budapest: Technika Alapítvány, 1999), 117-124.

<sup>24</sup> László Móra, *Fabinyi Rudolf élete és kora, (Rudolf Fabinyi's life and age)*, (Budapest: Technika Alapítvány, 1999), 67-107.

<sup>25</sup> Éva Vámos, “Scientific Community of an Emancipating Nation: Chemical Societies in Hungary before 1914”, in *Chemical Societies in Europe before 1914*, eds. Štrbáňová and A. Kieldebaek-Nielsen (London: Royal Society of Chemistry, 2008).

<sup>26</sup> Győző Czigler (1850-1905) was a descendant of an old Swiss family of architects. It was his grandfather that had settled in Hungary. Győző had finished his schools in his native town Arad (today Romania), then studied in Vienna at the Academy of Fine Arts. However, owing to his father's unexpected death, he returned to Hungary and completed his father's unfinished work. Thereafter he went for a longer study trip abroad. – Ferenc Végh: “Czigler Győző” (Győző Czigler) in: *Magyar Tudóslexikon*, 237-238.

<sup>27</sup> László Móra, *Wartha Vince, a hazai kémiai technológia megalapítója (1844-1914) (Vince Wartha, founder of domestic chemical technology, 1844-1914)* (Budapest: Tankönyvkiadó, 1967), 119-145.

<sup>28</sup> László Móra, “Wartha Vince” (Vince Wartha), in *Magyar Tudóslexikon A-tól Zs-ig (Hungarian Scientists' Encyclopedia)*, ed.-in-chief. Ferenc Nagy (Budapest: BETTER-MTESZ-OMIKK, 1997), 860-861.

<sup>29</sup> Mór Korach-László Móra, *Wartha Vince (Vince Wartha)*, (Budapest: Akadémiai Kiadó, 1974) 156-157.

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## The Trial of Lavoisier: A Strategy for Teaching Chemical Revolution in a History of Chemistry Course

*Glaucia Maria da Silva\**

To engage students in the study of history is a challenge for all who teach history of chemistry. Traditional methods for teaching at the higher education level usually employ a lecture format of instruction in which the majority of students listen passively to the instructor and jot down notes. The Sao Paulo History of Chemistry course was, in most ways, identical with courses taught in other Colleges and Universities across the world. Lectures and discussion sections, with a textbook, a few paperbacks, and occasional library readings constituted the bulk of the course. The lecture topics moved chronologically from one period to another, from one aspect of society to the next, while students covered one bloc of readings after the other in quiz sessions, with little correlation between lectures and discussion sections.

Current views of learning and instruction challenge the wisdom of this traditional pedagogic practice by stressing the need for the learner to play an active role in constructing knowledge<sup>1</sup>. In this age of active learning, teachers are looking for alternatives to the traditional lecture format and, as a result, they are discovering that debate offers a powerful tool for enlivening their teaching and energising their students. When students engage in debate, they take an active role in their education, and subjects which once may have seemed dull and abstract come vividly to life. These exercises not only draw students into the lesson in a more active way, they also provide students a critical skill that they do not necessarily learn from listening to a lecture. In preparing to debate a controversial question, students undertake a wide range of learning processes. They have to ask themselves what these questions mean personally to them; they have to research the social, political, ethical and historical contexts in which the issues are situated; and they can learn to see complex problems from widely different perspectives. Because this learning is geared toward a specific purpose, that of performing well in the debate itself, students have added incentive and a clear goal to work toward. The objectives are frequently directed toward both social and cognitive

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goals. The social goals include active listening, taking turns, consideration of others' view, and arguing without becoming angry. The cognitive goals include weighing and evaluating arguments.

It has been found that a mock trial is a useful tool not only for engaging students in active learning during the class hour, but also for enhancing the overall communication within the classroom. In a mock trial, students put a character on trial and then simulate the process with students playing the roles of the accused, the witnesses, court personnel and media. The multiple roles required in the trial make room for various student skill levels and public speaking abilities. While offering a variety of ways for students to participate in and learn from the activity, the mock trial format also ensures that all students grapple with and respond to historical evidence. The use of mock trials is not new concept in teaching, especially for pre-law or paralegal programs and there is an extensive body of research for using trials at all levels of education. College courses in English, sociology, communications, business, and even mathematics have all utilised mock trials. Mock trials have also been used in social studies classes to address historical issues, current events, or as a way to integrate social studies with language arts<sup>2</sup>. However, comparatively little has been done using mock trials in higher education courses. This account describes an idea for an interesting teaching strategy for history of chemistry classrooms and how this strategy was implemented.

## Procedure and Discussion

During a recent semester, an activity class was structured around a fictitious trial of Lavoisier. Earlier, the Chemical Revolution one unit had been found especially effective because, as an event in space and time, it was easily identified, but what was not so easy to determine was the meaning, or the significance, of this event, both for its participants and for subsequent commentators. It occurred towards the end of the eighteenth century and involved European science in an upheaval of considerable scope and consequences<sup>3</sup>. The issues that arose during the chemical revolution centered around experiment and theories concerned primarily with the chemistry of gases ("airs") and the phenomena surrounding burning and other forms of what is now called oxidation. During the 1770s the discovery of "airs" led to the rapid growth of intensive work on combustion, and to widespread interest in it within fashionable circles in Paris and London<sup>4</sup>. Thorough examination of the weight relations led Antoine Lavoisier to question the phlogiston explanation of combustion<sup>5</sup>. During the 1780s, Lavoisier developed a counter theory and undertook supporting experiments to a point where he was able to convert the leading



chemists of France (including most of his colleagues in the Paris Academy of Sciences, one of the world's leading scientific institutions at the time) to it. By the end of the 1790's, most British and European chemists had also been converted as well<sup>6</sup>. Besides this, Lavoisier was perhaps the most controversial historical figures related to the Chemical Revolution and his participation on this event is still shrouded in mystery and alive with controversy. So, the central question of the trial, was Lavoisier the responsible for the Chemical Revolution? was broad enough so that the students had to several issues to consider.

The class was composed of forty first-year students. Three class periods of one hour and forty-minute each (not including out-of-class student preparation time) were organised. In the first one, after the rules were explained, the roles to be played during the trial were assigned and the instructions given for the pre-trial student activities. The class had, therefore, knowledge about the purpose of the activity, an overview of the activity including how their work was to proceed, and a clear understanding of the time frame and of the required end product. To prepare the students for the trial, some reference sources were recommended to provide enough background to use effectively the trial as a learning experience.<sup>7-10</sup> The use of the original documents and sources for the historical research was encouraged and the students also could to do Web researches or to consult other references for background information. Furthermore, watching films about the period they were researching was encouraged, with the caveat, movies do not replace historical research.

The class was divided into three groups, and a role was assigned to each group. The roles were rather loosely defined by positions in the debate which was to follow; i.e., prosecutor, defender, or judge. A student volunteered to play the role of Lavoisier, one group was to act as the prosecutor, one as his defense, and the other as his judge, and one student played the lead counsel (advocate) for each side. The remaining students had to assume the roles of witnesses representing historical figures such as Marie-Anne Lavoisier, Fourcroy, Priestley, Cavendish and Black. They also had to prepare briefs for their lead counsel to question witnesses from the other side. The roles were structured so as to maximise the level of disagreement in the ensuing discussion and to represent a variety of perspectives on the historical situation. Dividing the roles this way immediately established an adversarial relationship among the groups.

Arguing about whether Lavoisier was the responsible for the chemical revolution did not seem very productive until the court-room scenario imposed goals for each group, for the prosecution and the defense, victory of their argument, for the judges, a fair verdict. Group work is problematic, perhaps even more so in a class-



room where most of the students work a minimum of thirty hours per week. These conditions make it difficult for group members to meet outside the class. Group contact was encouraged. Thus, on the second class, each group met and prepared its positions. At the end of this class, all students submitted to the Professor a written statement (300–500 words) in which they articulated the Lavoisier role on Chemical Revolution. Before the trial, these written accounts were reviewed and suggestions made for their improvement.

On the trial day, ten minutes were allocated for each side's opening statement and presentation of their case. The prosecuting and the defense attorneys had twenty minutes to produce their two to three witnesses and to cross examine as many witnesses as they liked. At the end, each side had ten minutes of closing arguments. Track of time was kept using a half-hour glass and each side given some general indication of how much time they had left, e.g. "Half your time has elapsed," "You have one quarter of your time left," etc. Each side could call a break at anytime, to collect their thoughts, stretch, and/or get refreshments. Once the prosecution began their case, the trial moved with surprising ease. The defenses' case was well conceived, planned, and articulated. There were no unexpected snags, few objections, and, apart from some sharp verbal altercations between the two lead counsels, no major problems. The prosecutions were clearly unprepared for the thoroughness and clarity of the defenses' case in part because no one knew what to expect and in part because of student procrastination. The instructor did not participate in the deliberations, but listened to the tribunal's comments, offered historical clarifications when asked, and solicited opinions about student performances. The deliberations were enlightening. After much discussion, but without exception, the tribunal concluded that the defense was the most convincing team. When the judge returned to read the verdict, the excitement and tension in the air was evident. Never had the instructor been in such an emotionally charged room as part of an academic exercise.

Finally, it was necessary to consider how to assess grades. All written work should be done individually. The students playing witnesses wrote two to three-page "briefs" about their respective explanation, while the lead counsels wrote a four-page essay detailing their strategy and how they planned to cope with the strengths and weaknesses of their respective issues. The student who played Lavoisier wrote a five-page essay on Lavoisier's background and his ideas regarding many of the prominent issues likely to be discussed. Students also were graded on participation in class, their preparedness to discuss readings, their performance as witnesses, the depth of understanding of the witness, ability to answer questions as witnesses (to the best of their ability), and demonstration of appreci-

ation for the issues at hand. In addition group members were rated from 1-5, in categories, including (though not limited to) responsibility, cooperation, research, and overall performance. However, only one grade in this assignment is a “group grade”, the presentation. Therefore, meeting as a group is not as burdensome as it can be with other types of group projects.

## Conclusions

Overwhelmingly students enjoyed this project but when they complained it was usually about the research and the groups. Their complaints usually mentioned conflicting schedules, their aversion to group work, and the amount of research required. Most students, however, liked the groups and the research. One student noted that the researching with a group was “very helpful and less time-consuming” and it provided him with “a chance to meet some of my classmates”. Others commented that listening to their classmates opened their eyes to different interpretations. Not all of the students were equally successful, but some were transcendent and memorable. The quality of student work produced as part of the mock trial was consistently of high quality. Most students were able to construct interesting historical arguments based on evidence they have read, heard, and acted out. Perhaps it was this mix of learning styles that contributed to the students’ enjoyment of the experience. The mock trial has generated enthusiasm among students that standard class discussions did not, and the fact that the outcome of the trial depends on student argumentation has created a higher level of student commitment to the curriculum. Equally important, students were very positive about mock trial instruction and believed that they learned more during class exercise than they would have during a traditional lecture.

However, all pedagogical strategy can and should be revised and improved each time they are used. The ambiance could be further enhanced in the future by the use of music, flags, and period art. In this account are given the issues confronted during a fictitious trial of Lavoisier. This kind of pedagogical device allows introducing historiographical debate and promotes meaningful critical analysis of historical sources and issues. Mock trials enhance active learning, foster a cooperative spirit, inspire hard work, and allow students to share their knowledge in a unique setting. The weaknesses can be remedied and the strengths further developed to create a memorable and effective course. The trial format requires a great deal of planning, flexibility, patience, and perseverance, but the rewards are well worth it, for student and Professor. They require a fair amount of work, both for students and for teachers, but if they are structured well, they can be a high-

light not only of one course, but of a student's entire learning experience, and the lessons learned can easily last a lifetime.

Despite any weaknesses, the trial was a success for student and professor alike. Most students agreed that this type of activity is valuable both for them and for future students. They appreciated the novel pedagogical approach to the subject. The value of such a trial is that it allows students to work out for themselves complex historical debates and situations. Sometimes they come up with unusual perspectives on the debates, ones that could not have been predicted. These discoveries are the most rewarding part of the learning and the teaching experience. Such an applied approach not only helps students to acquire and retain substantive material, but also increases their motivation and enjoyment in the class as well as generates higher-order thinking. This activity involved preparation time, outside readings, and written requirements. In conclusion, the mock trial is a stimulating alternative way to teach history of chemistry.

The trial became the starting point for students' historical research, but it also opened students to the new ideas about history of chemistry and theatre. Many students were intrigued by the use of plays and expressed interest in seeing them performed. Someone opened their minds to theatrical productions as more than a source of entertainment. Then, guidelines for student-developed productions were prepared<sup>11-14</sup>. The students suggested a theatre script entitled, "The judge of Lavoisier", with a brief description of each scene, a historical time line of the topic, a list of important historical persons involved, and a bibliography of resources. Finally, they performed a dramatisation for an outside audience in December, 2007. This participatory drama brought enormous fulfillment to these students. For some it was the first time that they had ever read a play, and most expressed their enjoyment at using plays in this "unconventional" way. The effort they put into script writing, rehearsals, and performances required ongoing discussion and analysis of their information until they reached an understanding of the underlying impact of the event on its participants. It is believed that the form of the historical drama is an exciting means to educate and entertain at the same time. Seeing a drama take place before you and knowing that these conflicts really did happen is far more stimulating than a fiction which is entirely made up.

All these activities, in which personal metadidactical strategies were developed, have given the opportunity to act as being more than a dispenser of information to a classroom of passive students. It is concluded that the traditional teacher-centered model in which knowledge is "transmitted" from the trainer to the trainee can be usefully replaced in part by alternative models of student development (constructivist and sociocultural ones).

## References

- <sup>1</sup> Barbara Rogoff, Eugene Matusov, Cynthia White, "Models of teaching and learning: Participation in a community of learners," in *Handbook of education and human development: New models of learning, teaching, and schooling*, ed. David R. Olson, Nancy Torrance (London: Blackwell, 1998), 388-414.
- <sup>2</sup> Douglas J. Futuyma, *Science on trial: the case for evolution*. (Sunderland: Sinauer Associates, 1995).
- <sup>3</sup> John G. McEvoy, "In search of the chemical revolution: interpretative strategies in the history of chemistry," *Foundations of Chemistry* 2 (2000): 47-73.
- <sup>4</sup> Robert Siegfried, "The Chemical Revolution in the History of Chemistry," *Osiris* 4 (1988): 34-50.
- <sup>5</sup> Arthur Donovan, "Lavoisier and the Origins of Modern Chemistry," *Osiris* 4 (1988): 214-231.
- <sup>6</sup> John G. McEvoy, "Continuity and Discontinuity in the Chemical Revolution," *Osiris* 4 (1988): 195-213.
- <sup>7</sup> Ana Maria Alfonso-Goldfarb, "As possíveis origens da Química Moderna," *Química Nova* 16 (1993): 63-68.
- <sup>8</sup> Allen G. Debus, "A Longa Revolução Química," *Ciência Hoje* 13 (1991): 35-43.
- <sup>9</sup> Bernadette Bensaude-Vincent, Isabelle Stengers, *História da Química*. (Lisboa: Instituto Piaget, 1996), 123-134.
- <sup>10</sup> Danielle Fauque, "O papel iniciador de Lavoisier," *Química Nova* 18 (1995): 567-573.
- <sup>11</sup> Robert Cassler, "Creating Historical Drama," *The History Teacher* 23 (1990): 255-262.
- <sup>12</sup> Carl Djerassi, Roald Hoffmann, *Oxigênio: uma peça em dois atos e 20 cenas*. (Rio de Janeiro: Vieira & Lent, 2004).
- <sup>13</sup> Nidia Franca Roque, "Química por meio do teatro," *Química Nova na Escola* 25 (2007): 27-29.
- <sup>14</sup> Nidia Franca Roque, "Uma festa no céu-peça em um ato focalizando o desenvolvimento da Química a partir do século XVIII," *Química Nova na Escola* 25 (2007): 30-33.

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## Teaching Chemistry Through History: The Importance of The Periodic Table

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In early times chemistry was more an art than a science. The first applications of mineralogy and pharmacology were used empirically by ancient man to satisfying their needs. The alchemical period was focused on the search for the Philosopher's Stone and the Elixir of Life. In the sixteenth and seventeenth centuries, chemistry started developing more general and abstract theories, including the study of the key role gases play in chemistry. In the 18th century, Lavoisier and other French chemists revolutionised the view of chemistry in its experimental structure, theoretical principles and nomenclature. After this period chemistry became an autonomous science and, finally,<sup>1</sup> it was introduced in French secondary school level curriculum.

Chemistry is an important part of our daily lives. However, chemistry currently has a negative perception in public opinion. In general, people identify chemistry with the artificial, with environmental and ecological damage, and with disasters by industrial contamination. All of these factors, and others, have had a negative impact on public opinion.<sup>2-3</sup> Chemistry also has a bad image among students. Most students do not appreciate the impact of developments in chemistry on various parts of our lives: technology, health, agriculture, energy, etc. and that the results of progress in chemistry benefits all of society.

High school students learn a variety of topics that include: structure of the atom and characteristics of matter, radioactivity, Lavoisier's law, the periodic table of elements, organic compounds, etc. However, they usually don't know who made or discovered that law, when it happened or how it was found. In spite of chemical processes being in use from long ago, the history of chemistry is not usually taught in Spanish high secondary schools. The present authors consider that this absence is preventing a good social understanding of chemistry.<sup>4</sup>

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### Interest of Spanish/Catalan secondary school students towards chemistry

There are 17 Autonomous Communities in Spain; each develops its own independent education systems. The results herein derive from Catalonia.<sup>5-6</sup> Students from secondary schools (14-16 years-old) and high secondary schools (>17 years-old) were asked to complete a questionnaire about chemistry and their feelings about the subject. 321 questionnaires have been completed since 2003, of which 52% correspond to female students and 48% to male. Table 1 summarises the results from four courses.

Results showed us that over the past 5 years interest in chemistry, and also in general science, has been decreasing<sup>7</sup> in younger students. Only 30% of secondary school students will pursue high studies in science and only 5% will pursue chemistry. There have been similar findings throughout Europe.<sup>8</sup> In addition, chemistry has a negative perception among about 40% of secondary school students and high secondary school students who did not pursue science, with 4 out of 10 students believing that chemistry is bad and 7-9 out of 10 students believing that chemistry is difficult.

**Table 1**

#### Interest of Spanish/Catalan secondary school students towards chemistry

Secondary school, 14-16 years old (n=162)		
Question	YES	NO
Will you pursue high studies in science?	30 %	70 %
Will you pursue a career in chemistry?	5 %	95 %
Do you find learning chemistry is difficult?	75 %	25 %
Do you believe chemistry is dangerous and bad?	44 %	56 %
High Secondary school, >17 years old, no science courses (n=84)		
Question	YES	NO
Will you pursue a career in chemistry?	0 %	100 %
Do you find learning chemistry is difficult?	95 %	5 %
Do you believe chemistry is dangerous and bad?	42 %	58 %
High Secondary school, >17 years old, science courses (n=75)		
Question	YES	NO
Will you pursue a career in chemistry?	8 %	92 %
Do you find learning chemistry is difficult?	39 %	61 %
Do you believe chemistry is dangerous and bad?	12 %	88 %

The answers from high secondary school students who are following science courses is worrying because, though they want to become scientists, 4 out of 10 students believe that chemistry is difficult. Why do so many of our future scientists believe that chemistry is difficult? One of the explanations could be that there is currently no relationship built between spectacular science, used to attract young students, and theoretical chemistry in secondary school's curricula. Additionally, chemistry instruction in high secondary school courses occurs only at the symbolic level (technical and unfamiliar language).<sup>9</sup> Furthermore, Nurrenbern and Pickering and Sanger *et al.*<sup>10-11</sup> have shown that students who can resolve mathematical chemistry problems will often still have trouble in answering conceptual problems about the same subject matter.

When most students think of chemistry, they think of mathematical problems, coloured water, symbols, and the cryptic periodic table. They don't usually think of the history of chemistry, a very rich history which teachers could possibly use to help motivate the students and encourage interest in all aspects of chemistry.

In addition, we correlated these results with the amount of history of chemistry in Spanish and Catalan chemistry textbooks. Most of them neglected this subject, though some books did contain a few illustrations (from Lavoisier, Arrhenius, Mendeleev, Gay-Lussac, etc.) or a few words about a small number of scientists (e. g.: "*Who was Dimitri Mendeleev and what did he do in chemistry?*"). However, there are a small number of chemistry textbooks with short general biographies (usually of about around 150 words) for several prominent chemists, such as Dimitri Mendeleev, Linus Carl Pauling and Niels Bohr.

The publishers of Spanish and Catalan chemistry textbooks can choose to include or omit, at will, part of "The History of Chemistry" because it is not present in current secondary school chemistry curricula.<sup>12</sup> Since Lavoisier's time, and even before, the inclusion of history to chemistry textbooks<sup>13-14</sup> has been examined. Discussions between Lavoisier and Fourcroy in the XVIII<sup>th</sup> century<sup>15-16</sup> are known. Fourcroy supported the addition of references to history in textbooks, while in contrast Lavoisier did not agree with these additions. The leaving out "The History of Chemistry" into chemistry textbooks could well be a reason why our students have lost interest in chemistry.

In Figure 1 are shown illustrations from chemistry books currently used in Spanish/Catalan secondary education. They depict, Lavoisier (without any explanation), Marie Curie and Dimitri Mendeleev with short captions, which are given translated, below.





\* Marie Curie made some important discoveries related to radioactivity and several isotopes.

\*\* Who was Dimitri Mendeleiev and what did he do in chemistry?

Generally, our secondary school students have lower interest in topics in chemistry than in other areas of science, such as biology or physics. This lack of interest is difficult to tackle. These are the problems to be confronted in our attempt to change education in chemical topics. In the following ways it is hoped to open up history and the nature of chemistry and increase interest in all aspects of chemistry.

### **“The History of Chemistry” in the classroom: an educational experience**

In order to bridge the gap between chemistry and its history, some changes are proposed in the chemistry curricula for students in their last year of secondary school, as well as for students in their first year of high secondary school. The aim of the project has been to find historical topics in chemistry which will help students understand some concepts more easily.

A few years ago, beginning in 2003, a number of secondary school pupils were made to study some chemists' biographies<sup>17-20</sup> such the alchemists (Paracelsus), Lavoisier, Döbereiner, Meyer, Mendeleev, Rutherford, Marja Sklodowska, Seaborg, etc. Each student or group that had researched a given scientist then made a 15-minute presentation on his/her designated chemist and answered questions posed by their fellow students. By this means, each had the opportuni-

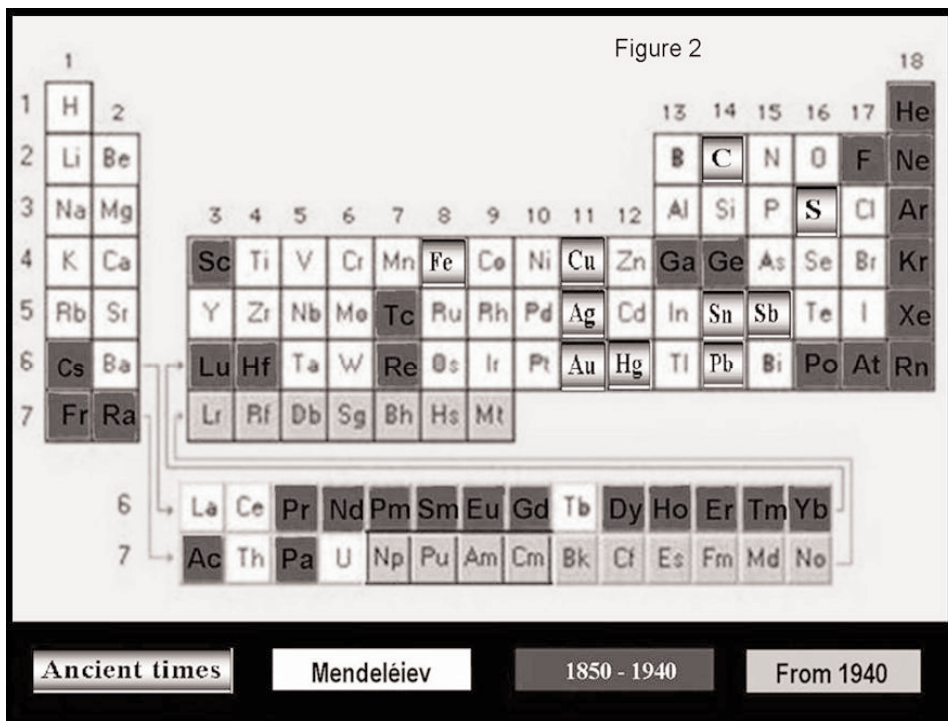
ty to learn about the lives of every chemist who had been researched by the class, either through direct research or through the presentations put on by their fellow students. In this way they became better equipped to compare and understand chemistry research and methods from different time periods in history.

Some of the students who participated in the previous class went on to enrol in high secondary school where they were more involved with chemistry than those who did not make this special study. All of these students, during their last year of secondary school, studied the biographies of Dimitri Mendeléiev and Lothar Meyer gained an understanding of the significant contribution both scientists made to the design and development of the periodic table of elements. Furthermore, by having the students also read the Seaborg biography<sup>21</sup> it helped them understand that new elements can be discovered at any time.

The periodic table of the chemical elements was created in 1869 by Dimitri Mendeleev.<sup>22</sup> The periodic table is a way of presenting the chemical elements. Mendeleev detected a clear relationship between putting chemical elements in atomic mass order and a regular variation of chemical elements properties which take place periodically. He also predicted new elements and placed all of the discovered elements into this new classification. The periodic table has been extended over time as new elements have been discovered. At the moment, it contains 111 confirmed elements.<sup>23</sup>

Our high secondary school students were divided into groups<sup>24</sup> to summarize the historical development of chemical elements and organized them into the periodic table of chemical elements. The elements were divided into four groups, related to the period in which they were discovered; Ancient Times, Mendeleev, 1850-1940, and from 1940 as illustrated in Figure 2. It was created for chemistry high secondary school students during the 2006 to 2007 course.

Figure 2 can be used as a tool to help increase the knowledge and understanding of chemistry among students. First, and most important for our students, was Mendeleev's ability to predict in 1870 the existence and the properties of eka-silicon, eka-aluminium, and eka-boron (names given by Mendeleev) which corresponded to germanium (Ge), gallium (Ga), and scandium (Sc) respectively. Gallium was discovered in 1875, scandium in 1879 and germanium in 1886, these three chemical elements were found to have approximately the same properties that Mendeleev had predicted for his three elements. Unfortunately this information is often missing from chemistry classes despite the fact that this part of history is very motivating to students and builds additional interest in general chemistry.



Secondly, students can clearly see that there were many jumps in the discovery of chemical elements, and our knowledge of chemistry has not been steadily increasing with time. Most of the important jumps in “The History of Chemistry” can be correlated with new laboratory tools and approaches to research, as illustrated in Figure 3.

The concepts and tools listed in Figure 3 have been utilised as an introduction to “The History of Chemistry” classes. The goal of this approach was to identify and create the best possible learning environment in the chemistry classroom. In this way, some chemistry subjects have been taught in parallel with changes in laboratory equipment/apparatus; *Nature of matter* with the clay furnace; *States of matter and gas laws* with glassblowing; *Ions and dissociation of water* with electrolysis; *Atomic structure and bonds* with spectroscopy; *Inert gasses* with distillation of liquid air; and *Radioactivity* with Nuclear reactions.

At the end of the course, feedback was collected from our students via a new questionnaire. The results are summarised in Table 2.

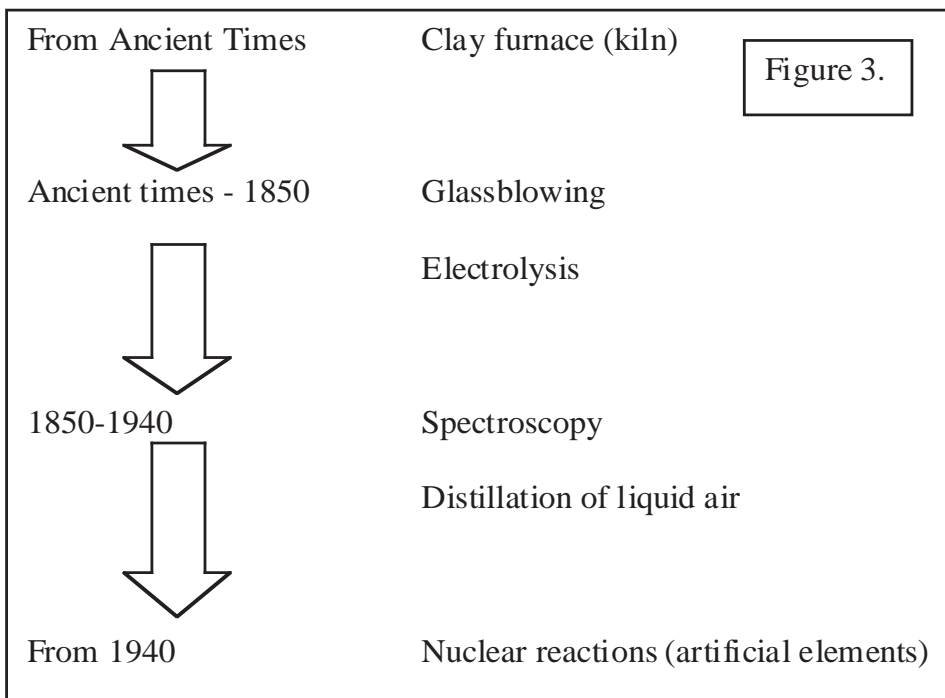


Table 2

**Feedback from Chemistry Students in High Secondary School  
Regarding “The History of Chemistry” Classes**

Question	YES	NO
Do you believe that learning the history of chemistry would improve overall understanding of chemistry?	72%	28%
Do you believe that the history approach required greater attention compared to a regular chemistry class?	20%	80%
Do you believe that you gained in-depth understanding of chemical concepts from “The History of Chemistry” classes?	80%	20%
Was learning chemistry difficult in “The History of Chemistry” classes?	16%	84%
Was learning chemistry exciting in “The History of Chemistry” classes?	88%	12%

As you can see from the data in Table 2, most students considered these classes valuable for increasing their chemistry knowledge (7-8 out of 10) and also stated that this approach increased their interest in chemistry and changed their previous perception of chemistry (2 out of 10 believe that chemistry is difficult, which is the same answer received for question 2 in Table 1). Finally, students indicated that The History of Chemistry classes caused greater interest in continuing to study chemistry.

## Conclusions

High secondary school students involved with chemistry will often intend to become scientists and/or chemists. However, they need a more in depth education of chemistry than is presently provided in secondary school in Spain. The History of Chemistry class aims to provide students with a new approach to learning chemistry and we believe chemistry teachers in general and Spanish/Catalan in particular could employ this new tool to great effect. These chemistry lessons teach secondary school students to appreciate the relationship between chemistry, the lives of chemists' and "The History of Chemistry" as well as providing them with an increased knowledge of general science. In addition, through this method they had an increased understanding of the concept of periodicity related to properties and reactivity of some chemical elements.

Teaching chemistry, by way of its history can again make it attractive to young students. We propose that this aspect of chemistry be present in the Spanish science secondary school curricula.

## Acknowledgements

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## References

<sup>1</sup> J. R. Bertomeu and A. García-Belmar. *La revolución química: entre la historia y la memoria* (Valencia: Universitat de Valencia, 2006), pp. 15-61.

<sup>2</sup> <[http://www.cefic.be/Files/Publications/PES\\_04\\_Ex\\_Summ.doc](http://www.cefic.be/Files/Publications/PES_04_Ex_Summ.doc)>

- <sup>3</sup> Bensaude-Vincent, B. "A genealogy of the increasing gap between science and the public" *Public Understanding of Science* (2001), 10(1), 99-113.
- <sup>4</sup> Zaragoza, C. and Fernández-Novell, J. M. "Bridging the gap between secondary school and The History of Science: an educational experience" in *The Global and the Local: The History of Science and the Cultural Integration of Europe*, ed. Michal Kokowski. (2007) 160-165.
- <sup>5</sup> <[http://www.xtec.es/estudis/eso/curriculum\\_eso.htm](http://www.xtec.es/estudis/eso/curriculum_eso.htm)>
- <sup>6</sup> <[http://www.xtec.es/estudis/batxillerat/curriculum\\_bat.htm](http://www.xtec.es/estudis/batxillerat/curriculum_bat.htm)>
- <sup>7</sup> Fensham P. J. "Issues for schooling in science" in *Science and the Citizen for Educators and the Public. A special issue of the Melbourne Studies in Education*, ed. R. T. Cross and P. J. Fensham (Melbourne: Arena Publications, 2000) 4(2), 73-77.
- <sup>8</sup> Martínez, B.; Gros, T. Romaña, "The problem of training in Higher Education" *Higher Education in Europe*, (1998) XXIII (4), 483-495.
- <sup>9</sup> Gabel, D., "The complexity of Chemistry and Implications for Teaching" in *International Handbook of Science Education*, eds. Fraser, B. J., Tobin, K. G. (The Netherlands, Dordrecht 1998) 233-248.
- <sup>10</sup> Nurrenbern, S. C. and Pickering, M. "Concept Learning versus Problem Solving: Is there a Difference?" *J. Chem. Educ.* (1987) 64, 508-510.
- <sup>11</sup> Sanger, M. J., Campbell, E., Felker, J. and Spencer, C. "Concept Learning versus Problem Solving: Does Particle Motion Have an Effect?" *J. Chem. Educ.* (2007) 84, 875-879.
- <sup>12</sup> <<http://www.mec.es/educa/sistema-educativo>>
- <sup>13</sup> Hedley, J. "The Study of chemical textbooks" in *Communicating Chemistry: Textbooks and Their Audiences 1789-1939* ed. Lundgren, A., and Bensaude-Vincent. B (Canton, Watson P. I., 2000) 1-18.
- <sup>14</sup> García-Belmar, A. and Bertomeu, J.R. "French Chemistry Textbooks, 1802-1852: New books for New Readers and New Teaching Institutions" in *Communicating Chemistry: Textbooks and Their Audiences 1789-1939* ed. Lundgren, A., and Bensaude-Vincent. B (Canton, Watson P. I., 2000) 19-56.
- <sup>15</sup> Grapí, P. "The convenience of history of science in chemistry textbooks at the beginning of the XIXth century. A reflection from Lavoisier and Fourcroy's positions" in *Actes de la I Jornada sobre la història de la ciència i l'ensenyament Antoni Quintana Marí*. ed. Societat Catalana d'Història de la Ciència i de la Tècnica (2005) 69-73.
- <sup>16</sup> Beretta, M. "The Historiography of Chemistry in the Eighteenth Century: a Preliminary survey and Bibliography" *Ambix*, (1992) 39(1) 1-11.
- <sup>17</sup> Román P. *Mendeléiev el profeta del orden químico* (Madrid: Nivola2002).
- <sup>18</sup> Asimov, I. *The search for the elements* (New York: Basic Books, Inc. 1962, Spanish edition 1986)
- <sup>19</sup> Asimov, I. *A Short History of Chemistry – An Introduction to the Ideas and Concepts of Chemistry* (New York. Doubleday & Co., Inc., 1965, Spanish edition 1975).
- <sup>20</sup> Esteban, S, *Introducción a la Historia de la Química* (Madrid UNED ediciones, 2001).
- <sup>21</sup> [http://nobelprize.org/nobel\\_prizes/chemistry/laureates/1951/seaborg-bio.html](http://nobelprize.org/nobel_prizes/chemistry/laureates/1951/seaborg-bio.html)
- <sup>22</sup> Eric Scerri, *The Periodic Table: Its Story and Its Significance* (Oxford, University Press, 2006), 63-158.
- <sup>23</sup> [http://old.iupac.org/reports/periodic\\_table/index.html](http://old.iupac.org/reports/periodic_table/index.html)
- <sup>24</sup> Dennick, R.G. and Exley, K. "Teaching and learning in groups and teams" *Biochemical Education* (1998) 26, 111-115.

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# Theories in the Evolution of Chemical Equilibrium: Implications for its Teaching and Learning

*Juan Quílez\**

This study is divided into two main but related parts. Firstly, a brief historical account is presented of the role of theories of chemical equilibria that evolved around different research programs concerning the attempts at measuring chemical affinities; and secondly, how this may be used as a base for an appropriate sequencing for the teaching and learning of chemical equilibrium. In particular, the background to the four basic chemical equilibrium concepts, namely, 'incomplete reaction', 'reversibility', 'equilibrium constant' and 'molecular dynamics' and suggestions about how teachers may translate such understandings into classroom practice.

## Introduction

Despite the fact that 'affinity' was the key concept in the development of the idea of chemical equilibrium during the last quarter of the 18<sup>th</sup> century and the early 19<sup>th</sup> century, the concept was not at the time given a precise definition (1, 2). This paper traces the evolution of theories of chemical equilibria grounded on the different research programs concerning chemical affinities. How scientists tried to determine the factors affecting affinity and measure this property of chemicals will be discussed. This led eventually to both mathematical reasoning and molecular dynamics as key theoretical tools in the explanation of equilibrium reactions. The starting point of this historical account is the state of the art in the construction of the first affinity tables. Berthollet re-worked this first theory, considering that the amount of the substances involved in a reaction was a key factor accounting for the chemical forces. Guldberg and Waage attempted to measure those forces, formulating the first mathematical affinity equations. Next, the first ideas providing a molecular interpretation of the macroscopic properties of equilibrium reactions will be presented. Lastly, how theoretical chemists integrated the previous findings, into a new field, that of physical chemistry will be outlined (3).

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## Affinity Tables

The earliest affinity table was that published by E.F. Geoffroy in 1718 (4). This table consists of sixteen columns. At the head of each column there is the traditional symbol of a substance (or a group of substances). Below it, the symbols for the substances with which it reacts are arranged in order of their affinity. Therefore, each substance will displace from a combination any of those lower down the column. The affinity table visually represented the relationships between chemical substances determined in the laboratory. The table had two intended uses: to 'discover or explain' what went on in the mixtures of several bodies and to 'predict' what had to result from particular mixtures (5).

In the early years of the 18<sup>th</sup> century, Newton tried to find a theoretical explanation for why some substances reacted with others. In the thirty-first Query of his book *Optics* he considered that in chemistry there would be forces similar to the gravitational ones. Within this theoretical basis, Newton introduced a mechanical view for chemistry. As a consequence, some scientists tried to give account of the measure of these 'elective affinities.' Buffon, Guyton de Morveau, and Bergman were some of the eighteenth-century chemists who supposed that chemical affinity was merely a gravitational attraction (6). The Swedish chemist T.O. Bergman made most extensive studies of displacement reactions. In 1775 he published *De Attractionibus Electivis*. The table in this book, as well as those that followed, up until 1784, was constructed with the aim of studying all the possible reactions. Bergman's theoretical goal was to discover, by experiment, the order of the varying attractions between different particles. This attraction, following very different laws from the gravitational forces, depended on the positions and "figures" of the particles. Thus, this conceptual framework established that chemical combinations were the result of the 'elective affinities,' which solely depended on the nature of the substances involved in the reaction. The determination of the affinities gave a relative order, which explained displacement reactions.

Bergman considered all reactions as being complete and taking place only in one direction. According to his conceptual framework, chemical reversibility was forbidden because it was assumed impossible that a reaction whose direction was determined by the relative order of affinities could be reversed. However, Bergman was aware of other factors that also affected chemical transformations, such as the influence of the mass of the substances over the course of the reaction. He felt that the few remaining anomalous reactions resulted from inadequate data, believing that further and more careful experimentation would enable chemists to fit all reactions into an ordered displacement affinity series without any inconsistencies (7).

*TABLE DES DIFFERENTS RAPPORTS  
observez entre differentes substances*

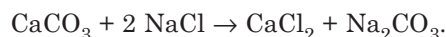
The table is a grid with 12 columns and 12 rows. The substances represented by the symbols in the grid are listed in the legend below the table:

- $\curvearrowright$  Esprit acide
- $\ominus$  Acide du sel marin
- $\oplus$  Acide nitreux
- $\otimes$  Acide vitreux
- $\odot$  Sel acide fixe
- $\odot$  Sel acide volatil
- $\nabla$  Terre absorbante
- SM Substances métalliques
- $\boxplus$  Alcalis
- $\boxminus$  Royale Solubles
- $\circ$  Or
- $\cup$  Argent
- $\text{♀}$  Cuivre
- $\text{♂}$  Fer
- $\text{♂}$  Plomb
- $\text{♂}$  Etain
- $\text{♂}$  Zinc
- $\text{♂}$  Phosphore
- $\Delta$  Soufre mineral
- $\Delta$  Soufre sublimé
- $\boxplus$  Esprit de vin
- $\nabla$  Eau
- $\odot$  Sel
- $\nabla$  Esprit de vin & Esprit acide

Geoffroy's affinity table (1718).

### Berthollet and the Importance of the Amounts of the Reactants

The problem of incomplete and reversible reactions was already known to chemists before the French Revolution. At the beginning of 19<sup>th</sup> century, these unusual reactions were given a new explanation by the French chemist C. L. Berthollet. His ideas developed from experiences with chemical reactions carried out on a large scale. He faced the problem of the variability of affinities when trying to obtain pure  $\text{KNO}_3$  because the process required some recrystallizations and Berthollet noted that as the concentration of nitrate increased, the capacity of the solution for dissolving additional nitrate decreased. He interpreted this anomaly by stating that the affinity responsible for dissolution was not an absolute force; therefore, in this phenomenon there would be equilibrium between antagonistic forces. These ideas found a new frame of implementation thanks to the trip he made in 1798, when he accompanied Napoleon's expedition to Egypt (8). He observed the continuous formation of sodium carbonate on the edge of 'natron lakes.' This reaction can be represented as follows:



This reaction was the reverse of that predicted by the theory of elective affinities. Berthollet accounted for it by means of the great quantities of sodium chloride and calcium carbonate present and the continuous removal of the products. The sodium carbonate formed a crust around the edge of the lake, and the deliquescent calcium chloride seeped into the ground.

Berthollet objected to the perception that elective affinity was an absolute, constant force that always determined the outcome of displacement reactions. He added the effect of quantity on chemical action, for the mass of the reactants could reverse the reaction predicted by the scale of relative affinities. That is, if two substances were competing to combine with a third substance for which they had unequal affinities, a relative large quantity of the substance with weaker affinity might exert a force that could surpass the force of the substance with greater affinity. Eventually, Berthollet considered that any displacement reaction was never complete but that there was an equilibrium state between opposite affinity forces. The strength of these forces, therefore, depended on two factors: the difference in their relative affinities and the quantitative proportion, “All substances which tend to enter in combination act by reason of their affinity and their quantity” (9). The equilibrium state was, in a manner analogous to mechanics, static. Thus, the consideration of the mass of the reactants as a key factor provided a rationalisation for incomplete reactions. It also explained why both the “direct” (forward) reaction (permitted, according to elective affinities) and the reverse one (forbidden by that theory) could occur.

Berthollet’s conception of affinity had an important corollary. Since affinities were a manifestation of universal attraction, all particles exerted an attraction toward all others, tending to unite them in chemical combination. Hence, combinations between particles in variable proportions were likely to occur. This last assertion was inconsistent with Dalton’s new atomic theory, which established the principle of definite proportions.

### **The First Mathematical Formulation of Chemical Equilibria by Guldberg and Waage**

Early investigations of chemical affinity focused primarily on acid/base and metal/acid reactions. In 1862 Berthelot and Saint-Gilles’s laboratory, redefined affinity studies by focusing on organic equilibria (*i.e.*, esterification reactions). Their experimental findings were the starting point for the investigations performed by two Norwegian scientists, C. M. Guldberg and P. Waage. They tried to

formulate a general mathematical equation to account for the experimental data, with the aim of devising a theory that could reconcile the earlier ideas of Bergman and of Berthollet. In their first publication of 1864 (10), they took into account mechanics as a paradigm, and focused on the measurement of what was responsible for what they called “chemical forces”. Convinced that chemistry should become, like mechanics, a science of forces and their effects, Waage and Guldberg aspired to develop a mathematical theory of chemical affinity. For a process they called simple (such as  $A = B + C$ ), they stated (11):

...two forces assert themselves, either a composing or a decomposing, or acting and reacting, and we view it as unavoidably necessary to regard these forces together if one is to find any quantitative expression of these forces.

Unlike Berthollet, Guldberg and Waage assumed that chemical forces were not proportional to the amounts of the substances involved in the reaction, but to the “active masses” (concentrations). For each substance, its active mass had a power they determined by experiment. Thus, for the following:  $P + Q = P' + Q'$ , they argued as follows (12):

If one begins with the general system which contains the four active substances in a variable relationship and designates the amounts of these substances, reduced to the same volume by  $p$ ,  $q$ ,  $p'$ , and  $q'$ , then, when the equilibrium state has occurred, a certain amount  $x$  of the two first substances will be transformed. The amounts of  $P$ ,  $Q$ ,  $P'$ , and  $Q'$  which keep each other in equilibrium will be consequently  $p - x$ ,  $q - x$ ,  $p' + x$ , and  $q' + x$  respectively. According to the law of mass action, the force for the first two substances is  $\alpha(p - x)^a(q - x)^b$  and the action force for the last two is  $\alpha'(p' + x)^{a'}(q' + x)^{b'}$  [where  $\alpha$  and  $\alpha'$  were proportion constants and  $a$  and  $b$  exponents, all to be determined by experiment]. Since there is equilibrium

$$\alpha(p - x)^a(q - x)^b = \alpha'(p' + x)^{a'}(q' + x)^{b'}$$

For the equilibrium: acetic acid + ethanol = ethyl acetate + water, they obtained the following results:  $a = 1$ ;  $b = 0.786$ ;  $a' = 0.846$ ;  $b' = 0.807$ ;  $\alpha/\alpha' = 0.502$ .

The above formula received confirmation from previously published data of Berthelot and Saint-Gilles. Moreover, the accuracy of that equation was tested by Thomsen in 1869 and later by Ostwald in 1876 (13). It was indeed the first equation that approximates the mathematical expression of what we call nowadays an “equilibrium constant”.

The consideration of the concentrations of the substances involved in the equilibrium system, instead of their amounts (*i.e.* masses), was a key factor that account-

ed for the understanding of the evolution of chemical equilibrium. Moreover, the vital step neglected by Berthelot and St. Giles, that of the reverse reaction, was taken into account by Guldberg and Waage, eventually allowing them to formulate the condition for chemical equilibria, when the net rate of reaction is zero (14).

Finally, it should be stressed that Gulberg's and Waage's new approach led the way in the application of mathematical reasoning to the facts of chemical science. The search for an exact equation between the concentrations of the substances involved in equilibrium represented a promising starting point in the search for the quantitative determination of chemical affinities.

### **Molecular Dynamics in Chemical Equilibrium**

In the preceding section attention was drawn to the first attempt at the derivation of mathematical equations representing systems in chemical equilibrium. The brief account that follows here is intended to convey the early interpretations as to how the equilibrium comes about.

In 1839 Gay-Lussac (15) imagined the equilibrium condition as a dynamic process of continuous interchange of acids and bases, which he described as a "pell-mell." In 1850 Williamson (16), studying incomplete esterification reactions, was the first scientist to propose a submicroscopic model in order to explain the "static" state of chemical equilibrium. He did not consider this equilibrium as a situation in which nothing happens; on the contrary, he assumed that two reactions ran simultaneously, each in opposite directions. Thus, reactants as well as products were constantly forming and decomposing in such a way that the amounts of all the substances involved remain constant. This dynamic balance was achieved by assuming an interchanging of atoms, equal in absolute number in each moment of time, taking place in opposite direction. Consequently, the relative velocity of transfer of analogous atoms in each of the two directions was not the same, but it was greater for the substances of lower quantity.

A later attempt to explain the molecular changes taking place in an equilibrium state was due to Pfaundler. In 1867 he described a chemical reaction in terms of the kinetic theory developed by Clausius and Maxwell. Pfaundler's approach was the first attempt to apply the mechanical theory of heat to chemical reactions by using Clausius's kinetic theory of evaporation in the development of a qualitative theory of chemical dissociation (17, 18).

Pfaundler was the first scientist who gave a correct account of partial dissociation. He hypothesised that dissociation varies in different molecules: a fraction of them is completely dissociated, and another fraction is unchanged. In the case of partial decomposition of a gas, Pfaundler assumed that at constant temperature and pressure equal amounts of molecules decompose and unite by collision. That explanation required that not all molecules were in the same state of motion at a given temperature. That is, it was assumed that some of the molecules regularly diverged more or less widely from the average state, for only a small number of collisions were effective to produce chemical reaction both in the sense of decomposition and formation. Eventually, a balanced molecular chemical equilibrium between decomposition and recombination was achieved.

In their last paper, Guldberg and Waage (19) took into account molecular kinetics and energy considerations. This was an attempt to explain the molecular changes taking place in an equilibrium state in the terms previously stated by Pfaundler in 1867.

### **Kinetics and Thermodynamics in the Study of Chemical Equilibrium Reactions: van't Hoff**

The first systematic idea about time in chemical reactions was formulated by C. F. Wenzel (20), whose aim was to search for a method of measurement of chemical affinities. By analogy to mechanics, he chose to measure chemical forces by the velocities with which they affected analogous processes.

In the summer 1864 Guldberg and Waage presented a paper in which they argued in terms of the velocities of reactions in forward and reverse directions. They derived the following rate equation for the forward reaction (21):

$$v = \frac{dx}{dt} = k(p-x)^n (q-x)^p$$

where  $v$  is the velocity of reaction,  $x$  is the quantity transformed in the time  $t$ , and  $k$  a constant depending on the nature of the system, including the temperature. Similarly, they also considered the rate equation for the reverse reaction. The rate of the net reaction was considered to be the difference of the two velocities,

$v_{\text{net}} = v_{\text{forward}} - v_{\text{reverse}}$ . They thus defined the equilibrium condition as  $v_{\text{net}} = 0$ . Although Guldberg and Waage later argued in terms of the “rates” of reactions in forward and reverse directions, initially they did it in terms of “forces”. Guldberg’s

and Waage's theoretical framework was criticised by Ostwald, Guggenheim and Laidler (22).

Eventually, a theoretical explanation of Guldberg's and Waage's equations came mainly from the work of van't Hoff (23) after he turned his attention to the question of how the equilibrium state was reached. Thus, his interest was no longer on the static analysis of forces, but on the dynamics of rates of reversible reactions. The starting point of his deduction was that the equilibrium is to be regarded as a result of two processes taking place with the same velocity in opposite directions. This kinetic approach allowed him the deduction of the mathematical equation representing the equilibrium constant.

In 1885, van't Hoff published one of his most remarkable contributions to the development of physical chemistry, *i.e.* the theory of dilute solutions (24). One of his aims was to derive the law for the equilibrium at constant temperature in a dilute state (*viz.* the equilibrium constant,  $K$ ), based on thermodynamics. van't Hoff expressed the variation of the equilibrium constant with temperature as follows;

$$\frac{d \log K}{dT} = \frac{q}{RT^2}$$

stating his mobile equilibrium principle (25): 'Every system between two conditions of matter (systems) is displaced by lowering the temperature, at constant volume, toward that system the formation of which evolves heat'.

### **The Introduction of Chemical Equilibrium in the Chemistry Classroom**

In introductory chemistry lessons, chemical reactions are normally presented as proceeding to completion, taking place in one direction only. However, in the case of chemical equilibrium, four basic ideas have to be considered (26):

- incomplete reaction;
- reversibility;
- dynamics;
- equilibrium constant.

Chemical equilibrium reactions are incomplete and can take place in both directions. The state of chemical equilibrium is characterised by a quantity, the equilibrium constant. These concepts are explained at a molecular level with the help of the dynamics concept. These four ideas are difficult for students to grasp. As a consequence, there are many misconceptions and difficulties about chemical equilibrium (27). For example students:



- often do not discriminate between reactions that go to completion and reversible reactions,
- may believe that the forward reaction goes to completion before the reverse reaction commences,
- may fail to distinguish between rate (how fast) and extent (how far),
- frequently think that after the reaction has started, the rate of the forward reaction increases with time and that of the reverse reaction decreases, until the equilibrium is reached,
- tend to misrepresent the changes that occur in chemical equilibrium reactions due to a variation in the temperature,
- often lack of mathematical tools and reasoning, which often leads to a poor understanding of the equilibrium law,
- may believe that mass and concentration mean the same thing for substances in equilibrium systems,
- often find difficulties in defining chemical equilibrium. Therefore, they frequently fail to understand the dynamic nature of a system in a state of chemical equilibrium. Instead, many students hold a static conception, for they believe that ‘nothing happens’ in that state,
- may find difficult to understand the proper meaning of words and concepts such as stress, balance, displacement, shift, equilibrium, etc.

Some of those misunderstandings and difficulties are regrettably reinforced in the chemistry classroom by (28):

### ***Methodology:***

- students are exposed to the answers before the problems have been stated (*e.g.* dynamics is usually presented as a fact rather than as an explanation),
- the popular kinetic introduction of chemical equilibrium, in which unsound kinetic assumptions are presented.

These teaching methods may explain some of the student misunderstandings related with the dynamic nature of equilibrium.

### ***Algorithmic chemical equilibrium problems***

The mastery of chemical equilibrium seems to be related with the solving of algorithmic chemical equilibrium problems, which neglects the promotion of conceptual reasoning on students.

**Teachers** who also hold misconceptions that are transmitted to their students.

To avoid those problems a novel curriculum has been developed that facilitates student understanding, based on appropriate uses of the history of chemical equilibrium, as explained in the following section.

### **The Nature of Scientific Knowledge and the Implementation of the HPS Approach: Suggestions for Teaching Chemical Equilibrium**

Among science education researchers there is an emerging consensus about the incorporation of a sound treatment of the history and philosophy of science (HPS) into the curriculum (29). Many authors have eloquently argued about the advantages of using the history and philosophy of science in science classes. The discussion of using the history of chemistry in the chemistry classroom can be focussed on four main points:

- A historical research on the development of chemistry should give explanatory clues about the processes involving the evolution of chemical concepts (*i.e.* to focus on the problems which arose and how they were tackled). That is, to give clues about the important epistemological question ‘how we know’.
- The historical knowledge of chemistry may help in understanding of the difficulties and alternative concepts held by students.
- The history of chemistry teaches students about the nature of chemistry.
- All of the above enables teachers to address the issues of instruction, which means:

a) to formulate general chemical problems; b) to design effective learning sequences.

Many science textbooks present an unsound image of science and many teachers lack a proper background of the HPS issue. Thus, the implementation of programs for teacher education that produce teachers that have a sound knowledge of the content of chemistry, focusing not only on its pedagogical content, but also on its history and philosophy would seem to be essential. In this context, the historical-philosophical reconstruction of key chemical concepts may allow teachers to exemplify many of the key aspects of the nature of scientific knowledge such as:

- the kind of problems that are addressed;
- the theoretical frames from which they are formulated;
- the hypothetical, tentative character of the process of inquiry;
- the interpretation of experimental facts from different concepts, as well as its treatment in textbooks;

- the role played by imagination and creativity;
- the limitations of models and theories;
- the evolution in the meaning of concepts and the process associated with that change;
- the unsolved problems;
- the role of the scientific community;
- the controversial issues;
- the importance of social and economic factors in the construction of scientific concepts.

Listed below are (1) some advantages that accrue to students from the translation of HPS understanding into classroom practice, in particular insight into teaching chemical equilibrium within a HPS framework, (2) key points to be considered and (3) some advantages that are of benefit to teachers.

1. Students may develop a better understanding of the nature of science:
  - The importance of classification in chemistry (*e.g.* affinity tables).
  - Scientists' reluctance to abandon a theory on the basis of anomalous reactions alone (*e.g.* Bergman's affinity).
  - Rival theories coexist many years (*e.g.* Bergman's and Berthollet's theories).
  - The construction of chemistry may be viewed as a human endeavour (*e.g.* the outcome of chemical equilibrium theory was due to the contribution of several individuals, most of them contemporaries of each other).
  - Scientific argumentation (*e.g.* to consider the evidence for and against each model).
  - Language (*e.g.* different meanings of affinity).
  - Controversial issues (*e.g.* Berthollet's theory/Proust law and Dalton's theory).
2. The historical reconstruction sequence as a basis for teaching the introduction and development of the main chemical equilibrium concepts:
  - The kinetic introduction of chemical equilibrium is questionable from an epistemological point of view (*i.e.* students are exposed to the answers before having given the question).
  - Challenging student's previous ideas about chemical reactions in order to develop the concepts of incompleteness [*e.g.*  $\text{Fe}^{3+}(\text{aq}) + \text{SCN}^{-}(\text{aq}) \rightleftharpoons \text{FeSCN}^{2+}(\text{aq})$ ] and reversibility [*e.g.*  $\text{Cr}_2\text{O}_7^{2-}(\text{aq}) + \text{H}_2\text{O}(\ell) \rightleftharpoons 2 \text{CrO}_4^{2-}(\text{aq}) + 2 \text{H}^{+}(\text{aq})$ ]
  - Explaining chemical equilibrium reactions (why is it that the reaction 'stops' when there are still reactants in the vessel of reaction?). Then, dynamism comes as an explanatory concept.

- The traditional textbook kinetic deduction of the equilibrium law has many flaws. Then, it seems appropriate the empirical derivation of the equilibrium constant, leaving to advanced levels the rigorous deduction of  $K$ , based on thermodynamic grounds.
- 3. Teachers' understanding of students' ideas and their resistance to change: teaching implications:
  - To note that some of the misunderstandings that students hold when dealing with chemical equilibrium parallels some of the ideas of 19th century scientists.
  - Teachers may use the history of chemical equilibrium to encourage students' conceptual change. It means that students may be helped by the teacher in order to challenge early models of chemical reaction and, ultimately, their own conceptions.
  - The historical dimension can be used to achieve determined affective objectives because the episodes of resistance to conceptual change in science clearly show that even well-known scientists can be mistaken. Thus, it is comforting to perceive that others have thought in a similar manner—that to hold such thoughts are not guilty of mere stupidity.
  - The laboratory can be used to replicate some 19th century chemical equilibrium experiments.
  - The historical dimension may add fundamental insights when planning alternative teaching approaches to questioned/controversial educational issues (e.g. Le Chatelier's principle may be reshaped on the grounds of the equilibrium law).

## References

1. M. Goupil, *Du Flou au Clair? Histoire de l'Affinité Chimique* (Paris: CTHS, 1991).
2. M. G. Kim, "The Layers of Chemical Language, I: Constitution of Bodies v. Structure of Matter," *Hist. Sci.*, 30 (1992): 69-96.
3. R.S. Root-Bernstein, *The Ionists: Founding Physical Chemistry, 1872-1890*. PhD Thesis (Princeton University, 1980).
4. E. F. Geoffroy, "Table des Differents Rapports Observés en Chimie entre Differentes Substances," *Memoires de l'Académie Royale des Sciences* (1718), 202-212. Two English translations are available: a) "Concerning the different affinities observed in chemistry between different substances, in H. M. Leicester and H. S. Klickstein, *A Source Book in Chemistry, 1400-1900*, (Cambridge: Harvard University, 1963) 67-75; b) "Table of the different relations observed in chemistry between different substances," *Science in Context*, 9 (1996): 313-319.
5. M. G. Kim, *Affinity, That Elusive Dream* (Cambridge: The MIT Press, 2003).

6. A. M. Duncan, *Laws and Order in Eighteenth-Century Chemistry* (Oxford: Clarendon Press, 1996).
7. F. L. Holmes, "From Elective Affinities to Chemical Equilibria: Berthollet's Law of Mass Action," *Chymia*, 8 (1962): 105-145.
8. S. W. Weller, "Napoleon Bonaparte, French Scientists, Chemical Equilibrium, and Mass Action," *Bull. Hist. Chem.*, 23 (1999): 61-65.
9. H. M. Leicester and H. S. Klickstein, *A Source Book in Chemistry, 1400-1900* (Cambridge: Harvard University, 1963) 199.
10. P. Waage and C. M. Guldberg, (translated by H. I. Abrash), "Studies Concerning Affinity," *J. Chem. Educ.*, 63 (1962): 1044-1047.
11. Waage and Guldberg, "Studies Concerning Affinity," 1045.
12. Waage and Guldberg, "Studies Concerning Affinity," 1046.
13. W. Ostwald, *Outlines of General Chemistry* (London: MacMillan, 1890).
14. C. M. Guldberg and P. Waage, *Études sur les Affinités Chimiques* (Oslo: Christiania University, 1867).
15. J. L. Gay-Lussac, "Considerations sur les Forces Chimiques," *Ann. Chim. Phys.*, 70 (1839): 407-434.
16. A. W. Williamson, "Suggestions for the Dynamics of Chemistry Derived from the Theory of Etherification," *Proc. R. Inst.*, 1 (1851-1854): 90-94.
17. L. Pfaundler, "Beiträge zur Chemischen Statik," *Ann. Phys.*, 131 (1867): 55-85.
18. a) E. W. Lund, "Activated Complex-a Centenarian?," *J. Chem. Educ.*, 45 (1968): 125-128;  
b) J. Berger, "Chemische Mechanik und Kinetik: Die Bedeutung der Mechanischen Wärmetheorie für Die Theorie Chemischer Reaktionen," *Ann. Sci.*, 54 (1997): 567-584.
19. C. M. Guldberg and P. Waage, "Über Die Chemische Affinität," *Journal Prakt. Chem.*, 127 (1879): 69-114.
20. M. M. P. Muir, *A History of Chemical Theories and Laws* (New York: Arno Press, 1907-1975).
21. a) E. W. Lund and O. Hassel, "Guldberg and Waage and the Law of Mass Action," in O. Bastiansen, Ed., *The Law of Mass Action. A Centenary Volume, 1864-1964* (Oslo: Det Norske Videnskaps-Akademi Oslo, 1964) 37-46;  
b) E. W. Lund, "Guldberg and Waage and the Law of Mass Action," *J. Chem. Educ.*, 42 (1965): 548-549.
22. a) M. G. Kim, *Practice and Representation: Investigative Programs of Chemical Affinity in the Nineteenth Century*. Ph.D. Thesis (Los Angeles: University of California, 1990);  
b) E. A. Guggenheim, "More About the Laws of Reaction Rates and of Equilibrium," *J. Chem. Educ.*, 33 (1956) 554-555;  
c) K. J. Laidler, "Chemical Kinetics and the Origin of Physical Chemistry," *Archive for History of Exact Sciences*, 32 (1985): 43-75.
23. a) J. H. van't Hoff, "Die Grenzebene ein Beitrag zur Kenntniss Esterbildung," *Ber. Dtsch. Chem. Ges.*, 10, (1877) 669-678; b) J. H. van't Hoff, *Studies in Chemical Dynamics* (London: Williams & Norgate, 1884-1896).
24. J.H. Van't Hoff, "L'Équilibre Chimique dans les Systèmes Gazeux ou Dissous à l'État Dilué," *Archives Néerlandaises des Sciences Exactes et Naturelles*, 20 (1885): 239-302.
25. Van't Hoff, *Studies in Chemical Dynamics* 217.
26. J.H. van Driel; W. de Vos; N. Verloop and H. Dekkers, "Developing Secondary Student's Conceptions of Chemical Reactions: the Introduction of Chemical Equilibrium," *International Journal of Science Education*, 20 (1998): 379-392.

27. J. van Driel and W. Gräber, 'The Teaching and Learning Chemical Equilibrium'. In J.K. Gilbert, et al. (eds.), *Chemical Education: Towards Research-Based Practice* (Dordrecht: Kluwer, 2002) 271-292.
28. J. Quílez, "Changes in Concentration and in Partial Pressure in Chemical Equilibria: Students' and Teachers' Misunderstandings", *Chemistry Education: Research and Practice*, 5 (2004): 281-300.
29. M. Matthews, *Science Teaching. The Role of History and Philosophy of Science* (London: Routledge, 1994).

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## Charles Friedel (1832-1899) and the Laboratory of Practical Chemistry in the Rue Michelet in Paris

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In France, the 1890s were marked by a number of important events in academic, economic, and –especially between 1894 and 1898, in the turbulent years of the Dreyfus affair– political life. The decade began with a fundamental reform of the educational system, especially at the secondary level. At the time, secondary education was divided between two parallel tracks: the ‘classical’ (*enseignement classique*), and the ‘special’ (*enseignement spécial*). The ‘special’ programme was intended to prepare for employment in agriculture, trade, and manufacturing. It embraced a substantial element of instruction in chemistry, including practical classes culminating in the techniques of analysis in the final year. But the programme failed to meet the needs of industry and was seen as lacking the status of its classical counterpart. In an attempt to achieve parity of esteem, the *enseignement spécial* was transformed into a new ‘modern’ programme (*enseignement moderne*) with a structure similar to that of the ‘classical’ programme in 1891. The *enseignement moderne* had its own ‘modern’ *baccalauréat* in what was intended as a move towards the unity of secondary education.<sup>1</sup> In addition to the provision in secondary education, chemistry was also taught in a number of vocational schools, though at a level that offered an inadequate coverage of the techniques used in the chemical industry.<sup>2</sup>

Higher education had been reformed in the previous decade, but the reforms had left unresolved the fundamental question of institutional titles. Should higher education be dispensed by individual faculties (as had been the case since the founding of the National University by Napoleon in 1808) or by Universities? The budget legislation of 1893 created groups of faculties (*corps de facultés*) in the major cities. These were hybrid bodies, somewhere between loose federations of individual faculties and true Universities. The reform of 1893 also introduced the PCN, a one-year programme of preparatory study in physics, chemistry and natural history, required for entry into a Faculty of Medicine: as part of this change, teaching of chemistry that had previously been conducted in Medical Faculties

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was transferred to the Faculties of Science. In 1896, the *corps de facultés* were redesignated as Universities,<sup>3</sup> and in a symbolic act signalling the change the new buildings of the Sorbonne were completed.

On the economic front, a visit to the international exhibition in Chicago in 1893 led Albin Haller, Professor at the Faculty of Science in Nancy, to sound the alarm with regard to chemistry. His report, which he submitted to the Minister of Commerce and Industry in the following year, reads as a solemn warning against the threats to the French chemical industry.<sup>4</sup> Haller insisted, for the industry to become competitive, in particular with its counterpart in Germany, it was essential to improve the teaching of chemistry, most notably in relation to industrial practice. The report, reinforced by other contributions by Haller, made a great impact in the press. When extensive extracts from the report were published in the *Revue générale des sciences* in 1894,<sup>5</sup> the journal's editor Louis Olivier described the enthusiastic response of his readers, both in the administration and among teachers.<sup>6</sup> In response to readers' demands for further discussion of the issues, Haller's articles were reissued as an independent volume in 1895.<sup>7</sup> The volume began with a discussion of the relations between higher education and industry in different countries, followed by a statistical comparison between output in the chemical industries of Britain, Germany, France, and the rest of the industrial world. Only one conclusion could be drawn. This was that France lagged far behind Britain and even more so Germany. The evidence did much to ensure renewed interest in the request for the creation of a school of practical<sup>8</sup> chemistry in Paris that the chemical manufacturer and Senator Alcide Poirrier had submitted to the Senate in 1893 (see below).

### **Practical chemistry in Paris during the 1890s**

In Paris, there were few institutions that prepared chemists for high-level industrial employment. The most prestigious of those that did so was the *École municipale de physique et de chimie industrielles* of the city of Paris (EMPCI). Opened in 1882, the school trained a limited number of pupils in accordance with a programme that gave prominence to laboratory classes (*travaux pratiques*). Candidates, all Parisian and aged between 14 and 18, were required to pass an entrance examination, after which three years of study led on to the diploma for the ablest pupils and for the rest, a leaving certificate. In this way, the school trained both chemists and foremen, i.e. in present French terminology 'ingénieurs' and 'techniciens'.<sup>9</sup>

Until 1891, another very popular school had been the one that Edme Fremy (1814-1894) ran in his practical chemistry laboratory at the *Muséum d'histoire naturelle*. A pupil of Gay-Lussac and Pelouze, Fremy was appointed to Chairs first at the *Muséum* and then at the *École polytechnique*. He was well connected on many fronts, including the Academy of science of the Institute of France (where he was elected in 1857) and Parisian high society. He also maintained strong links with manufacturers and was an administrator of the Saint-Gobain factory. Entry to Fremy's school, which he founded in 1863, was free of charge, and there were no formal entry requirements and no diploma. The programme of study lasted three years, but many students left after six months to work in a factory or laboratory. At the *Muséum*, they learned practical work '*à la paille*', acquiring skills that employers greatly appreciated. Fremy was particularly well known for his dexterity as an experimenter, and he offered no theoretical instruction. Between 1863 and 1891, more than 1400 students were trained, including Henri Moissan. But on 13 December 1891, the laboratory closed, provoking strong reactions in the press and from the *Société chimique de Paris*, which asked for the reopening of the laboratory, without success.<sup>10</sup>

### A debate in the Senate

Industrialists themselves had raised the question of practical training in the *Société chimique de Paris*. Early in 1891, the chemicals manufacturer Louis-Alphonse Adrian, a member of the *Chambre syndicale des produits chimiques*, had presented the council of this Society with three different proposals for a school of practical and industrial chemistry.<sup>11</sup> The second of the proposals recommended that laboratories for applied chemistry, supported by grants from manufacturers, should be attached to certain Faculties of Science or to Higher Scientific Schools.<sup>12</sup> The resulting discussion was both fierce and confused.<sup>13</sup> Several members of the council were already committed to strengthening the relations between academic chemistry and the industrial world, and a number of attempts had been made to create an industrial section. Among the Presidents of the Society, who were particularly active in this respect were Charles Lauth (1883), Paul Schützenberger (1885), Philippe de Clermont (1886), Achille Le Bel (1892), Charles Friedel (1888) and Auguste Scheurer-Kestner (1894).<sup>14</sup> Among these, Charles Friedel (1832-1899), an Alsatian, crystallographer and chemist, was especially important. He had been a student of Wurtz, was a champion of the atomic theory, and succeeded Wurtz in the Chair of Organic Chemistry at the Sorbonne (1884). He was in regular correspondence with Scheurer-Kestner, whom he invit-

ed to accept the presidency in 1892: "I hope that in this way you will be willing to give a helping hand [...] in our efforts to establish an ever closer bond between industry and chemical science".<sup>15</sup> Together, between 1894 and 1898, Friedel and Scheurer-Kestner worked to establish a Section of Industrial Chemistry in the Society, but with little success. Scientifically and economically, however, the general climate was favourable. This was a good moment, in fact, at which to ask the government to found a programme of advanced training in practical and industrial chemistry.

It was from this context that the Laboratory of Practical Chemistry in the *rue Michelet* (1896) emerged. The founding benefited from two other institutional opportunities: first, the closing of Fremy's laboratory at the *Muséum* (13 December 1891) and, secondly, the installation of the Faculty of Science in the New Sorbonne, in the southern part of Henri-Paul Nénot's building in the *rue Cujas*.

On 24 March 1893, Senator Alcide Poirrier said to the Minister of Public Instruction, in the debate about the *Muséum's* budget for 1894: "There was a distinctive style of chemistry teaching in Mr. Fremy's laboratory; it was the only laboratory where young men could acquire the knowledge necessary for work in the chemical industry. Now, the laboratory has been closed, and this has not improved the situation, far from it".<sup>16</sup> At the time, Fremy's name was familiar to everyone. His wife's death had recently been announced in the press, and Fremy's health was failing. Hence it was understandable that Poirrier should choose to cite Fremy in the debate, barely a year after the laboratory at the *Muséum* was closed.

Trained in business, Alcide Poirrier (1832-1917) became one of the heads of the *Société des matières colorantes de Saint-Denis*, in 1858. In 1870, he was a member of the *Chambre syndicale des produits chimiques* (subsequently its President), and in 1879 he became a member of the *Chambre de commerce de Paris* (later Secretary, Vice-president, then President for three years). In 1889, he was elected Senator (for 28 years), sitting on the political left, and Vice-president of the Senate from 1902 to 1905. As a close friend of Lauth, Scheurer-Kestner, and Friedel, and an active member of the *Société chimique de Paris*, he spoke about Friedel's plan for a school of practical chemistry in the Faculty of Science.

The Minister, Charles Dupuy, replied: "[Our] laboratories prepare for the examinations and degrees of the University, but they do not train for specific employment as a laboratory or production chemist ... I believe that in the budget for 1894... I shall be able to satisfy Mr. Poirrier's request".<sup>17</sup> The discussions contin-

ued, and the plan came to fruition two years later. In 1894, Friedel and his colleagues moved to the New Sorbonne, so releasing the buildings in the *rue Michelet* for use by the future laboratory.

### The Laboratory of Practical Chemistry (1896)

On 1 November 1896,<sup>18</sup> the Laboratory of Industrial and Practical Chemistry opened, with Friedel as director and Camille Chabrié as his assistant. The programme lasted three years, with respectively 50, 40 and 30 students. Each student had to pay 500F per year, as a contribution to chemicals and other materials for experiments, and the budget for the three years was 118 800F.<sup>19</sup> There were no formal requirements for entry to the laboratory apart from a solid secondary education, but as there were many applicants it was necessary to be selective.<sup>20</sup> In this respect, the context was similar to that of Fremy's laboratory, the instruction was entirely practical, and it was open to anybody with the necessary intelligence and skill. The *Comité de patronage* included a number of representatives of the industrial or political world such as Scheurer-Kestner, Poirrier, and Lauth, but initially industry gave no financial support.<sup>21</sup>

The Laboratory's original mission was to train chemists for work in the chemical industry and in faculty laboratories or other institutional laboratories such as the *Laboratoire municipal*. The standing of students at the end of the course was similar to that of superior technicians in our own day. Each year, a student was expected to take one certificate, and after the three years, the most successful students received a diploma as a chemist, awarded by the Faculty.<sup>22</sup> During the first two years, students learnt qualitative and quantitative analysis and performed laboratory exercises in inorganic and organic chemistry. The third year was devoted exclusively to the chemical analysis of manufactured products, both organic and inorganic. Along the way, students were introduced to manufacturing processes and to the handling of apparatus they might be expected to use later in industrial laboratories. Theoretical teaching was not neglected: in addition to lectures in the faculty, students attended lectures on industrial chemistry and applied physics, as well as ones by the laboratory staff.<sup>23</sup> The additional training in theory means that the similarity to Fremy's laboratory breaks down. The practical instruction consisted of a large number of laboratory classes intended to inculcate not only a high level of manual dexterity but also (because of a large measure of freedom in the teaching, here as in Fremy's laboratory<sup>24</sup>) a spirit of initiative. As for the syllabus, Friedel's laboratory of practical chemistry had much in common with the *École municipale de physique et de chimie industrielles*.

This is hardly surprising, given the shared aspirations of the chemists who founded the two institutions.

### **Friedel's succession (1899)**

Friedel had been very active in Dreyfus's cause. His engagement in support of justice drained much of his energy, and in April 1899 he died. He left two posts to fill in the Faculty of Science: the Chair of Organic Chemistry, in the *rue Cujas*, and the position of Director of the Laboratory of Practical Chemistry, in the *rue Michelet*. In the Council of the Professors of the Faculty, there was discussion of whether the two Chairs should be transformed into a single Chair of Applied Chemistry, or whether the distinction between the two posts should be maintained. Haller was invited to accept both posts. But he accepted only the Chair of Organic Chemistry,<sup>25</sup> while the Laboratory of Practical Chemistry was entrusted to Henri Moissan.<sup>26</sup> One year later (1900), at the Faculty of science in Paris, the Chair of Chemistry (Troost's) fell vacant, and Moissan was appointed to the post, which he held in addition to the directorship of the Laboratory.<sup>27</sup>

### **Moissan's long-term task**

Moissan was faced with two urgent tasks. The first was to resolve the question of the dilapidation of the building, which was in danger of collapse. On 26 June 1900, for example, students had their feet in water because of leaks, rust was falling from the chimneys into the crucibles, gas-mains were leaking, the work-surfaces were burnt and marked, the roof was beyond repair and letting in rain, etc.<sup>28</sup>

The second task was to raise the standard of the students, improving their theoretical instruction and their general command of the subject. The view that Moissan expressed to the Council of the Faculty on 10 November 1900 was unequivocal: "Some of them [students] are impeded in their analytical work because they don't know the rule of three! An examination for entry to the laboratory should be introduced".<sup>29</sup> He asked too that the students should be required to follow the lectures for the licence.

The name *Institut de chimie appliquée* (given to the Laboratory in 1901) was changed to *École nationale supérieure de chimie de Paris* (ENSCP) in 1948. Now, the entrance examination is difficult, and the chemical engineers who graduate from the ENSPC are generalists with excellent career prospects. As successor to Friedel, whose ideas he followed, Moissan (Nobel Prize, 1906) brought great fame

to the Institute. In particular, he was sensitive to the evolving nature of instrumentation in chemical laboratories, which increasingly drew certain techniques from physics. He also recognised that as electrochemistry and physical chemistry had emerged as major areas of chemical enquiry, and that the teaching offered to students had to constantly evolve.

## Conclusion

This case study has brought out the complexity of the structures and procedures for teaching and training in practical chemistry in Paris at the end of the XIX<sup>th</sup> century. The shifting requirements of manufacturers, the limited number of posts, and the cost of the training all conditioned what could be done. But, in conclusion, let me insist on the remarks that Chabrié (who succeeded Moissan as director in 1906) made at the banquet of the *Association des élèves de M. Fremy* in 1908: “[my] Institute would never have been founded if the *Muséum*’s laboratory had continued to exist... Friedel had often expressed to [me] the indignation he had felt at the closure”.<sup>30</sup> In that speech Chabrié was expressing an essential and important truth.

## Notes

<sup>1</sup> Danielle Fauque, “L’enseignement de la chimie et les exercices pratiques dans le cadre de la réforme de 1902”, in *Études sur l’enseignement des sciences physiques et naturelles*, ed. Nicole Hulin (Lyon: ENS Éditions, SFHST, 2001), 154-80, on 157-58.

<sup>2</sup> For more information, see Charles R. Day, “Education for the industrial world: technical and modern instruction in France under the Third Republic, 1870-1914” in eds Robert Fox and George Weisz, *The Organization of science and technology in France 1808-1914* (Cambridge: Cambridge University Press, 1980), 127-153.

<sup>3</sup> Alain Prost, *L’enseignement en France, 1800-1967* (Paris: Armand Colin, 1968), 252-254. See also Robert Fox, “Science, the University, and the State in Nineteenth-Century France”, in *Science, Industry, and the Social Order in Post-Revolutionary France*, ed. Robert Fox (Aldershot UK, Brookfield Vermont USA: Variorum, Ashgate, 1995), VIII, 66-145, on 110-118.

<sup>4</sup> Albin Haller, *L’Industrie chimique, les Produits chimiques et pharmaceutiques, à l’Exposition de Chicago. Rapport à M. le Ministre du Commerce et de l’Industrie, et à M. le Ministre de l’Instruction publique* (Paris: Imprimerie nationale, 1894). Review by Henri Moissan in *Revue générale des Sciences pures et appliquées (RGS)*, V (1894): n° 21, 829-32 (15 novembre).

<sup>5</sup> Albin Haller, “La lutte des nations sur le terrain de l’industrie chimique”, *RGS*, V (1894): 473-87 (15 juillet); “Sur quelques récents perfectionnements de la grande industrie chimique”, *RGS*, V (1894): 563-78 (15 août); “L’enseignement chimique à l’étranger. Laboratoires nouveaux”, *RGS*, VI (1895): 201-204 (15 mars).

- <sup>6</sup> Haller, "L'enseignement chimique", *RGS VI* (1895): 201, n. 2. Danielle Fauque & Georges Bram, "La chimie française à l'orée du XX<sup>e</sup> siècle", *Pour La Science* 189 (1993): 44-50, on 48.
- <sup>7</sup> Albin Haller, *L'Industrie chimique* (Paris: Librairie J.-B. Baillière et fils, 1895).
- <sup>8</sup> 'Practical': This word means that the pupil was trained in laboratory practices relevant to both pure and industrial chemistry. The time devoted to laboratory instruction was greater than that allocated to theoretical lectures.
- <sup>9</sup> Hippolyte Copaux (dir.), *École municipale de physique et chimie industrielles. Cinquante années de science appliquée à l'industrie* (Paris: EMPCI, 1932).
- <sup>10</sup> Danielle Fauque, "Organisation des laboratoires de chimie à Paris sous le ministère Duruy (1863-1869): Cas des laboratoires de Fremy et de Wurtz", *Annals of Science*, 62 (2005): n°4, 501-31.
- <sup>11</sup> I am grateful to Alan Rocke for drawing my attention to his point in correspondence in the spring 2007. Ulrike Fell and Alan Rocke, "The Chemical Society of France in its Formative Years, 1857-1914: Disciplinary Identity and the Struggle for Unity", in *Creating Networks in Chemistry: The Founding and Early History of Chemical Societies in Europe*, eds A. Kildeboek Nielsen and S. ěrbánová (London: Royal Society of Chemistry, 2008), 92-113. Marika Blondel-Mégrelis, "Esquisse pour une histoire de la Société chimique, 1857-2007", *L'Actualité chimique*, 310 (juillet 2007): I-XIX, on XII. Archives of the Société française de chimie (SFC), Procès-verbaux de la Société chimique de Paris (SCP), 20 juin 1891, 91-100.
- <sup>12</sup> SFC, procès-verbaux des séances du conseil de la SCP, 20 juin 1891, 91-93.
- <sup>13</sup> SFC, procès-verbaux des séances du conseil de la SCP, 20 juin 1891, 91-93; 7 décembre 1891, 95-96; 25 janvier 1892, 99-100.
- <sup>14</sup> Laurence Lestel (ed.), *Itinéraires de chimistes, 1857-2007. 150 ans de chimie en France avec les présidents de la SFC* (Les Ullis, France: EDP Sciences, SFC, 2008).
- <sup>15</sup> Charles Friedel to A. Scheurer Kestner, 29 novembre 1892, Bibliothèque nationale universitaire de Strasbourg, Mss 5982-83, p. 245 V°.
- <sup>16</sup> *Journal officiel de la République française (J.O.)*, Débats parlementaires, Sénat. Séance ordinaire du vendredi 24 mars 1893, 394.
- <sup>17</sup> *J.O.* (1893): 394. Also, Fauque, "Organisation des laboratoires", 530-31.
- <sup>18</sup> Archives nationales de France, Centre des archives contemporaines (CAC), Fontainebleau, ENSCP, 20010498, 131/1, Arrêté du 29 avril 1896.
- <sup>19</sup> CAC, 20010498, 131/1, Delpeuch's report, extract, 1895.
- <sup>20</sup> CAC, 20010498, 131/2, the Doyen G. Darboux to the Recteur, 29 novembre 1897.
- <sup>21</sup> CAC, 20010498, 131/1 V°, handwritten extract of the projected budget for 1896-1899.
- <sup>22</sup> CAC, 20010498, 131/1. It is clearly announced in the school rules that the diploma is a diploma of "ingénieur-chimiste" in 1896.
- <sup>23</sup> Paul Freundler, "Charles Friedel et l'enseignement pratique de la chimie appliquée à la Faculté des sciences de Paris", *Revue générale de chimie pure et appliquée I* (1899): 209-10.
- <sup>24</sup> Fauque, "Organisation des laboratoires", 503.
- <sup>25</sup> Archives nationales de France, Centre d'accueil et de recherche des Archives nationales (CARAN), Paris, AJ/16/5122, Registre des procès-verbaux des séances du Conseil de la Faculté des sciences, 4 juillet 1899, 249.
- <sup>26</sup> CARAN, AJ/16/5122, 28 octobre 1899, 256.
- <sup>27</sup> CARAN, AJ/16/5122, 18 juillet 1900, 283; 5 novembre 1900, 286.
- <sup>28</sup> CAC, 20010498, 131/2, Moissan to the Recteur, 26 juin 1900.
- <sup>29</sup> CARAN, AJ/16/5122, 10 novembre 1900, Moissan at the Faculty Council.
- <sup>30</sup> *Bulletin de l'Association des Anciens Élèves de M. Fremy* (1908): nouvelle série, n° 4, 173.



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## The Société Française de Chimie (1857-2007) as a Place for Thinking Chemistry in France

Laurence Lestel\*

This contribution is related to the adventure of editing a biographic book on 88 French chemists to celebrate the occasion of the 150<sup>th</sup> anniversary of the French Society of Chemistry (Société Française de Chimie, SFC) in 2007. For its 50<sup>th</sup> and its 100<sup>th</sup> anniversaries, the Société published books about its history, including the presentation of the scientific contributions of its first Presidents [*Cinquantenaire*, *Centenaire*]. More than two years ago, the idea emerged to publish a reference book on its presidents, which finally came out last January, under the name: *Itinéraires de chimistes. 1857-2007*. Some members of the *Club d'histoire de la chimie*, a thematic group of the Société, developed the project and the SFC accepted to edit the book. After a short history of the beginning of the Société, the content of the book and the difficulties met will be presented.

### The Société Française de Chimie

In 1857, three young chemists, Arnaudon, Collinet and Ubaldini, began to meet once a week to discuss their work and about the chemical papers and books published in France and abroad. The meeting place was a Parisian cafe, located Cour du Commerce, a passage which started at 130 boulevard Saint-Germain. They were soon joined by young chemists, often foreigners, who worked with Chevreul, Dumas, Sainte-Claire Deville, Berthelot or Balard. They founded a chemical group whose initial statutes were drafted in June 1857 [see Fournier, Fell and Roche]. The first President, elected in June 30, 1857, was Jacques Arnaudon, a young Italian from Turin, 28 years old, who had been in France since 1855 for training in the laboratory of Chevreul. The President used to be elected for a month, the secretary and treasurer for six months. The Société took its name, the Chemical Society of Paris (Société Chimique de Paris).

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The Société thus started its activities only a few years after the Chemical Society of England, founded in 1841, but before the Deutsche chemische Gesellschaft (founded in 1867), the Russian Chemical Society (1868), the American Chemical Society (1876) and the Chemical Society of Japan (1878).

After the Presidency of Arnaudon, the second President was Anton Rosing, a young Norwegian student of Dumas who then left France for London soon after his period as President of the Société. He then went to Edinburgh and returned to Norway in 1861. The third President was Aimé Girard, who at that time worked in Dumas's laboratory at the Sorbonne and at the Ecole Polytechnique, where he was appointed Curator of collections of chemistry and mineralogy in March 1858. Under the chairmanship of Girard, the Société which was already very structured planned the publication of a chemical revue, and sought a new location, one which best suited to the growing Société. Girard prepared to do what Dr. Quesneville called "the chemical 18 brumaire", a reference to the 18 Brumaire (November 9, 1799), where Napoleon Bonaparte had set up the Consulate. On December 29, 1858, the Société admitted as new members the well known chemists Henri Sainte-Claire Deville, Louis Pasteur, Auguste Cahours, and Paul Dehérain. It was then stated that the Société wished to "change the spirit which was its own since its founding, by enlarging the circle of its scientific activity". On the same day, Jean-Baptiste Dumas was elected a member and President by acclamation, contrary to the statute which provided for an election by secret ballot. The Vice-Presidents were the new members Pasteur and Cahours. At that time, Dumas was already recognised as the most influential chemist of France. The initial small circle of friends was thus dispersed.

However, Dumas did not involve himself in the new Société. In 1859 and 1860, the regular meetings of the Société were directed by Pasteur, who succeeded Dumas at the Head of the Society in 1861. Until 1922, Presidents were elected for one year. This rapid rotation led some to be presidents several times: Pasteur in 1861, 1865 and 1869; Wurtz in 1864, 1874 and 1878; Berthelot in 1866, 1875, 1882 and 1889 before being appointed Honorary President in 1900 to represent the Société at the ceremonies of the *Exposition Universelle*. Between 1923 and the Second World War, Presidents were elected for three years. The mandates then varied from one to three years. Since the adoption of the latest statutes of the Société in 2006, the President is elected by a directorial Board for a three-year term, renewable once. A total of 75 presidents have held the reins of the Société between 1857 and 2007.

After the beginning centred on Paris, the Société gradually developed some activities outside of Paris, at the initiative of Albin Haller creating in 1895 a section in Nancy. Then sections were created in Lyon in 1898, Lille and Toulouse in 1902,

Marseille and Montpellier in 1905. The Société then took the name of Société Chimique de France in 1906.

Scheurer-Kestner introduced a thematic section of industrial chemistry in 1894, but without any success. Thematic sections were created under the Presidency of Champetier. The first was the Division of Analytical Chemistry created in 1958, followed in 1964 by the Divisions of organic chemistry and of physical and mineral chemistry. Following the numerous attempts to strengthen ties between industry and the Société, the last created division was that of Industrial Chemistry in 2006. In 1984, the Société merged with the Society of Physical Chemistry and became the French Society of Chemistry. Finally, it took back the name of Société Chimique de France in 2008.

### ***Itinéraires de chimistes, 1857-2007: the Société Française de Chimie and its Presidents***

The editorial choice was to write the biography, scientific contribution and role at the French Chemical Society of its 75 Presidents in around six pages for each, which is quite short, including a short abstract in English. An Index of names is also included. For the first Presidents, we benefited of the insight that is given by elapse of time. But the book also includes biographies of the recent Presidents, as Armand Lattes who has been President of the Society until the end of the year 2007.

The biographies of the Presidents of Honour of the French Chemical Society were included. Eight of them never assumed the Presidency of the Society. As this book was to be devoted to well-known French chemists, the eight French chemists who received the Nobel Prize of chemistry were included. Only one of these was President of the Société, Henri Moissan. Two of them were Presidents of Honour, Victor Grignard and Paul Sabatier. The others, Marie Curie, Irène Joliot-Curie and Frédéric Joliot, Jean-Marie Lehn and Yves Chauvin, never had any responsibilities in the Society. We have, however, refrained from taking into account the Presidents of the Society of Physical Chemistry, which had also a long history, as this would have considerably increased the size of the book and the workload, with the risk of not completing the work in time for the anniversary of the Société.

Many of the Presidents of the Society have the expected profile of personalities known for their expertise in chemistry, often holding other institutional positions. Thus, among the 75 presidents, 44 are Academicians. Most of these were elected in the section of chemistry, but some have been elected in mineralogy (Sainte-Claire Deville, Pasteur), rural economy (Girard, Maquenne, Lindet or André), sci-

ence applied to industry (Portevin, Velluz) or as free academician (Bel, Hackspill, Tréfouël, Henri Moureu, Henri Normant). Berthelot, in 1873, was elected in the section of general physics.

Some of the Presidents of the Société have studied medicine or pharmacy. Some came from engineering schools, others have been trained at University, but not all of them took their baccalaureate, i.e. Scheurer Kestner who regretted it later. Besides those who have accumulated many institutional posts (Dumas), some such as Clermont and Le Bel apparently did not seek to make a career, thanks to their personal fortunes. They however cultivated strong networks of friendship with chemists of their time.

Throughout its history, the Société has sought to reinforce its links with industry [Blondel-Mégrelis]. Some industrialists were elected to its head: Scheurer-Kestner, Lauth, Laire, Thesmar, Poulenc, Paul. Many of these manufacturers made recognised scientific works. But Pascalis, from the Ecole polytechnique, was proud to have “no scientific background”, he was elected because of his position at the Chambre syndicale des produits chimiques. This work also led us to better understand the growth of different sectors of French chemistry, organic chemistry with the professor Auguste Béhal and its students Moureu, Blaise ou Tiffeneau, or inorganic chemistry around Georges Chaudron and its students Michel and Bénard.

Some affiliations appeared: Alphonse Combes was the nephew of Charles Friedel; Leon Lindet was the nephew of Aimé Girard; Henri Moureu was the son of Charles Moureu. The history of the Société is especially marked by the friendships among the first small group of chemists at the origin of the Société, and by the great friendship and solidarity that has existed between the Alsatian Wurtz, Friedel, and their students Clermont, Le Bel, Combes, Scheurer-Kestner. They dominated the Société during the years 1870 to 1890. They were very influential on the *Bulletin* editorial board, which became the place where atomism was defended by Wurtz. These Alsatian chemists were also among the founders of the French Association for the Advancement of Science (AFAS) in 1872 (Lauth, Wurtz, Friedel, Clermont, along with Girard and Grimaux) and the founders of the Ecole Alsacienne, a famous high school in Paris, in 1874 (Friedel, Clermont, Wurtz, and also Gautier). Grimaux, Lauth and Friedel signed the request for the review of the trial of Alfred Dreyfus in 1898, for which Scheurer-Kestner was heavily involved. In response to this engagement, Grimaux was dismissed from his post as Professor at the Ecole Polytechnique in 1898.

These chemists also played a role at the international level. Friedel was at the head of the Committee for the Nomenclature of Organic Chemistry, whose works

were presented in Geneva in 1892. The commission included Béhal, Bouveault (secretary), Combes, Gautier and Grimaux. Many of them participated in the actions of the International Union of Pure and Applied Chemistry (IUPAC) and Charles Moureu was its first President. Thanks to the involvement of Delaby, IUPAC was able to reorganise its activities after the Second World War.

The activities of these chemists was greater than the strict domain of science. Girard was a scientific journalist in the newspaper *La Patrie*. Girard and Balard were involved in the development of photography (at a time when taking a photo and develop it required a number of chemical operations). Le Bel published a dozen works in the *Bulletin* of the prehistoric French Society and gave to the Society the prehistoric cave of Eyzies, along with his house at, 250 rue Saint-Jacques, in Paris, where the Society is still resident. Many of these chemists have shown interest in the history of their discipline. Thanks to Berthelot, the late nineteenth century, the history of alchemy and early chemistry was rediscovered. Willm helped Dumas to publish the works of Lavoisier. Grimaux was one of the editors of Gerhardt's book. Tiffeneau was also interested in Gerhardt and his relations with Liebig. Delepine published numerous biographies of French and foreign chemists.

### **The difficulties encountered**

The book has 49 authors, from among almost 600 people, i.e. 15% of the members of the Society, who are registered as interested by the history of chemistry. Many of the writers are thus chemists. Many exchanges between authors and five reviewers contributed to make information more reliable, a rather difficult task with such a large number of records covering 150 years of French chemistry.

To support their work, the authors have plunged with delight into archives, classic archives such as those of the Academy of Sciences, Ecole polytechnique, Ecole supérieure de physique et de chimie industrielle de la Ville de Paris (ESPCI), the National Archives for the files of careers of the teaching personnel; or more unexpected ones, such as those of the Bank of France (Pascalis) or the Prefecture of Police of Paris (for the chemists who were inspectors of industrial firms). The *Bulletin* of the Chemical Society of Paris, which became the *Bulletin de la Société chimique de France* in 1907, has been much used source for both the obituaries and the minutes of the directorial board of the Société [Bram and Golfier]. The more recent Presidents have helped by oral testimony or by writing themselves.

However, such work did not go smoothly. I would like to emphasise some problems that were met. Should more space be allocated to famous chemists, such as

Berthelot, Sainte-Claire Deville or Moissan, for whom books already exist, or keep the balance between all of the Presidents, in order to give information on the lesser-known chemists, such as Joseph Riban, Rodolphe Engel or Georges Pascalis? Should the older chemical terms be translated into modern terms, or keep their original form? Should the accounts be harmonised, which might seem necessary for a reference book, or should more freedom be allowed in style? The discussions to resolve such conflicts meant that the book was not published in July, 2007, as expected, but in January, 2008 [*Itinéraires de chimistes*].

This work had originally a single goal: the publication in 2007 of the biographical dictionary of Presidents of the Société. It has, however, allowed correcting erroneous information, such as the holder of the Presidency in 1901, who, contrary to what had been said by Armand Gautier in 1907, was not Berthelot, but Engel [Gautier]. It has produced a better understanding of the growth of certain sectors of French chemistry. It highlights the contribution of the Société to the internationalisation of research, through the IUPAC organisation and by the editing of scientific publications on the European scale. It actually has opened the door to many avenues of reflection on the French chemistry of the nineteenth and twentieth century, for example, what were the links of the Société with the Société de Chimie Physique, what was the role of the Society during World Wars I and II? How to take into account the involvement of these chemists in the Conseil d'hygiène of Paris? Many other questions appear when reading the book! It should be of interest to compare this work with biographies concerning members of other chemical societies.

## References

- Marika Blondel-Mégrelis, “Esquisse pour une histoire de la Société chimique, 1857-2007”, *L'Actualité chimique*, 310 (juillet 2007) : I-XX.
- Georges Bram, Michel Golfier, “Le Bulletin de la Société chimique et quelques-uns de ses premiers rédacteurs”, *Bulletin de la Société. Chimique de France*, 134 (1997): 841-851.
- *Centenaire de la Société chimique de France (1857-1957)* (Paris, Masson, 1957), 249 p.
- *Cinquantenaire de la Société chimique de France* (Paris, Gauthier-Villars, 1908).
- Ulrike Fell, Alan Rocke, “The Chemical Society of France in its Formative Years, 1857-1914”, in *Creating Networks in Chemistry: The Foundation and Early History of Chemical Societies in Europe*, eds. Kildebaek A. and Štrbáňová S. (London, Royal Society of Chemistry, 2008): 91-112.
- Josette Fournier, “Un élève de Chevreul : Jacques Arnaudon (1829-1893) co-fondateur et premier président de notre Société”, *L'Actualité chimique*, (juillet 2003) : 47-52.
- Armand Gautier, “Le cinquantenaire de la Société chimique de France”, *Revue scientifique*, 5<sup>ème</sup> série, 7 (1907) : 641-689.
- *Itinéraires de chimistes. 1857-2007. 150 ans de chimie en France avec les présidents de la SFC*. L. Lestel coord. (Paris, EDP Sciences and SFC, 2008), 582 p.

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## Changing Identity and Public Image. A Sociosemiotic Analysis of Famous Chemical Laboratory Pictures

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Francesca Turco*

In the last decades the analysis of scientific iconography has achieved a certain importance in the historiography of science.<sup>1</sup> This is understandable, because images play a major role in the scientific communication. The sociosemiotic analysis of scientific iconography<sup>2</sup> is a powerful means of historical research, which may be added to the linguistic,<sup>3</sup> epistemological<sup>4</sup> and operational<sup>5</sup> instruments in the historians' tool-bag. The laboratory is the place in which chemistry has build up and continuously rebuilds its identity as a specific experimental discipline. On the whole, the experimental practices greatly changed in time, however some visible aspects of the chemical laboratory have proved to be permanent and easily recognizable; among these aspects, the most relevant ones are those connected with the simple operations as to grind or to distil. The use of laboratory images as a socially recognizable representation of a specific research activity preceded the birth of chemistry as an autonomous discipline, and went with the development of the discipline until the last decades of the XIX Century. In our paper we analyse the sociosemiotic meaning of famous laboratory pictures by Jan van der Straet (1523-1605), Louis-Jacques Goussier (1722-1799) and Wilhelm Trautschold (1815-1876). The images were published in very different editorial contexts and the same languages of the accompanying texts are revealing the shifting of the international language of science: Latin,<sup>6</sup> French,<sup>7</sup> and German.<sup>8</sup> Anyone of the considered images reflects the complex relationships between the contextual culture, the artist's style, the publishing (production) context and layout.

We have applied to the three images of laboratory the simplest machinery of the sociosemiotic analysis. For every image we looked at the most important features of its actual production: the personality of the artist, the media used and its public, and obviously the social and institutional context of production. When we go along the research axis from sociosemiotic analysis to historiography, we see

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that the analysis can give us interesting results on classical issues of historiography of science. Not surprisingly we get first hand information on the image of chemistry and chemists at the time of production of the image, but also on the hierarchies which regulate the work inside any laboratory. Moreover, our studied cases span more than two centuries, so we can obtain a good result on the most typical problem of historiography: the issue of continuity and discontinuity.

Before the analysis of the images, we add just a couple of hints to the origin and development of the kind of analysis we will use in our paper. Roland Barthes was the first to apply to a still image the usual linguistic distinction between the *denotative* aspects (what is represented in the image) and the *connotative* aspects which refer the viewer to a different and more intricate set of meanings.<sup>9</sup> The connotative aspects are the most important, because they refer to a cultural repertoire which is reckoned as common to the sender and the receiver of the iconographic message. At the same time, our decoding of the connotative aspects is difficult, because we (the historians) may know/use only a small part of the cultural repertoire that was brought into play by an image in an other past culture. From the point of view of the technique of analysis, on the whole, we followed the proposals of Kress and van Leeuwen, who remarked that, beyond the piecemeal analysis of the single details, a fundamental concern lies with “the composition of the whole, the way in which the representational and interactive elements are made to relate to each other, the way they are integrated into a meaningful whole”.<sup>10</sup>

### Early modern era: Jan Van der Straet

The first artist we meet is Giovanni Stradano or Jan Van der Straet (1523-1605). He was a Flanders-born mannerist artist active mainly in 16th century Florence. After an apprenticeship in Flanders he reached Florence in 1550, where he entered in the service of the Medici Dukes and Giorgio Vasari. This move from North to South was somewhat usual for many northern intellectuals, at that time. We recall two aspects of Stradano’s activity in Italy: the participation to the cycle of paintings in the Studiolo of Francesco I, Grand Duke of Tuscany, and the collaboration with Luigi Alamanni that brought to the publication of the famous *Nova Reperta*.

Francesco I ordered Vincenzo Borghini to design a particular private room and Vasari was appointed to the realization. Borghini described the Studiolo with these words: “Lo stanzino ha da servire per una guardaroba di cose rare et pretiose, et per valuta et per arte, come sarebbe a dire gioie, medaglie, pietre intagliate, cristalli lavorati et vasi, ingegni et simil cose, non di troppa grandezza, ripos-

te nei propri armadi, ciascuna nel suo genere”.<sup>11</sup> The Studiolo was a glorious *Wunderkammer*, in which the picture by Stradano was flanked by other paintings on productive activities and useful inventions, from glass works and the invention of gunpowder to wool mills and smelting works. To be sure, originally the paintings simply covered the doors of the cabinets of the Studiolo, and the title of Stradano’s work was *Il laboratorio dell’Alchimista* (1570).

In Stradano’s picture (Figure 1) we see a grandiose perspective, which gives a scope without limits to the activity in the foreground. In the semiotic of images it is important to follow the iconographic episodes from left to right. On the left side of the image it is put what it is known, and on the right one what is new. In figure 1, at left, we see a man reading a book (the known) and at the far right a man working with an attentive face. The ‘model’ was Francesco I; here the Grand Duke is cautiously experimenting under the guidance of Giuseppe Benincasa, who was a Flemish botanist and whose Dutch name was Josef Goodenhuysen. With a gesture of the hand, Benincasa points to the *product* of distillation, the laboratory operation which is at the centre of the scene. A small crowd of eleven persons is at work in the picture, but at the centre of the scene there is the final product of this work. We underline the characteristic stare of Giuseppe Benincasa since through it he can gaze upon the observer. Behind the glasses, symbol of perspicacity, the man looks directly the observer’s eyes. The structural analysis calls this typology of relationship ‘demanding action’, because the scene is not only offered information, but involves the consulting observer.

The theme of *Laboratorio*, the Florentine picture, is resumed in a plate belonging to a famous engravings collection, published with the revealing title of *Nova Reperta*. From 1587 on, Luigi Alamanni regularly commissioned designs from Stradanus for engravings representing a variety of his favourite literary and scientific subjects. He supplied detailed iconographic instructions, often with references to literary sources, and composed Latin captions for the engravings. The cover of the collection celebrates the most important discoveries (or innovations) of the time: discovery of America, compass, gunpowder, printing, but also distillation and the use of guaiac against syphilis.

*Nova Reperta* consists of a cover plus 19 engravings; *Distillatio* (Figure 2) is the eighth of the collection, and its caption is: “In igne succus omnium, arte, corporum Vigena fit unda, limpida et potissima”.<sup>12</sup> While many engravings were the result of “what Dutch engravers called a *compilatiewerk*, a scene brought together from various sources”,<sup>13</sup> in the case of *Distillatio* Alamanni and Stradanus had a complete reference model in *Laboratorio*. However Stradanus was mindful of the very different audience of the new image, and worked again on the theme.<sup>14</sup>



Figure 1. *Il laboratorio dell'Alchimista*, oil painting by Jan Van der Straet, 1570.

Alice McGinty<sup>15</sup> considers *Nova Reperta* as a whole system of images on four themes: America discovery, matter and movement, rationalization of agriculture, visual arts and printing mechanization. The context in which we find the image of laboratory is extremely interesting because it contributes to the specific theme of the conquest of matter and movement. The other engravings on this theme represent gunpowder, iron clocks, the use of stirrups, and a watermill. Following Anne Marie Thibault-Laulan we think that: “Pareilles situations offrent un terrain privilégié à l’analyse par l’étendue du corpus qui facilite le repérage des lois d’assemblage, élément capital du système”.<sup>16</sup> It is just the aspect of being an extensive corpus which in a recent and impor-

tant publication made *Nova Reperta* a kind of icon of the “early modern era”.<sup>17</sup> For Park and Daston “the aggregate effect of the *Nova reperta* engravings, which depict sixteenth-century landscapes, workshops, ships, and domestic spaces, is to portray the period as one of extraordinary fertility, creative ambition, and innovation”.<sup>18</sup>

In the image of figure 2 we note a more explicit narrative than in figure 1: the known at the left, a group of workers and operations at the centre, and at the right other people preparing new substances and carrying out a new experiment. It is interesting the fact that now the perspective is closed by a flight of arcs, and at the centre we see a big water bath (or *balneum Mariae*); the process of distillation



Figure 2. *Distillatio*, engraving by Jan Van der Straet, in *Nova Reperta*, 1638.

is the protagonist of the scene. At the left, it is noteworthy the communication by gestures, from the spot of the actual distillation to the standing gentleman who is pointing to a special point on the book. The meaning of this communication is ambiguous, because it may mean that the operation has to follow the instructions, but also that the experiment clashes with the theory found in the book. At the farthest corner of the laboratory image we find a very unusual and impressive display of curved and straight lines which seem to close the access to that last, secret room. Anyway it was the time of secret procedures.

Now we can compare the overall change of message/meaning from *Laboratorio* to *Distillatio*. We have to remember that the audience of the two images was different. *Laboratorio* was a private image, while *Distillatio* was a public one, and the shift of meaning is twofold: from the product (as a private property) to the process



(as a public opportunity), from a private teaching to a public learning (from the experiment). *Veritas filia temporis*,<sup>19</sup> but also, we could asset, *filia spectantis*: the observer and his context.

### ***Siècle des Lumières : Louis-Jacques Goussier***

Louis-Jacques Goussier (1722-1799) is the most interesting artist of our trio, essentially because he was also a learned man and a researcher. He was born in Paris, in a modest family; self-taught, in 1746 he came in contact with the French scientific establishment, with the modest task of executing calculations for Charles-Marie La Condamine. In 1747, through d'Alembert's recruiting, he becomes a regular collaborator of the *Encyclopédie*. Among his many and important activities we mention three other works. Since 1768 he was engaged in the preparation of a *Physique du Monde* with Baron Etienne-Claude de Marivetz; since 1789 collaborated to the *Encyclopédie méthodique des manufactures, arts et métiers*, with Roland de la Platière, and in 1794 published a *Recueil de Dessins de Machines et Instruments des Arts*, for the Conservatoire des Arts et Métiers. The long collaboration of Goussier with Baron de Marivetz underlines his scientific stature, because the *Physique du Monde* was a very ambitious work,<sup>20</sup> and Goussier was formally working au pair with Marivetz.<sup>21</sup>

The crucial step in Goussier's professional life happened in 1747, when he became a collaborator of the great *Encyclopédie*. It is noteworthy that this is the same date of the editorial appointment to Diderot and d'Alembert for the *Encyclopédie*. The first volume of articles of the *Encyclopédie* was published in 1751, the first volume of plates was published only in 1762 and the last one ten years later, in 1772. It is evident a long period of research and preparatory work, in particular by Goussier. We remark three features of his collaboration with the *Encyclopédie*. He wrote 61 articles; that on forges runs for 45 folio pages. He drew more than 900 plates and supervised all the plates of the *Encyclopédie*, as d'Alembert wrote: "highly-skilled & very intelligent in all parts of Mathematics & Physics", "très-versé & très-intelligent dans toutes les parties des Mathématiques & de la Physique", "joint la pratique du Dessin à beaucoup de connoissances de la Mécanique [...] il s'est particulièrement occupé des figures de l'*Encyclopédie* qu'il a toutes revûes & presque toutes dessinées".<sup>22</sup> Goussier followed all the vicissitudes of the great enterprise of Diderot and d'Alembert, and in a sense is the third author of the grandiose project.<sup>23</sup>

Another fundamental aspect of the image is the strong emphasis on horizontal lines: the long bench and, really impending, the shelf at the base of the great hood. In the semiotic analysis, the horizontal lines suggest ‘phisicality’, and in both cases, on the table and on the shelf, we see a lot of vessels and apparatus.



We obtain another important result of the semiotic analysis when we look at the whole plate. The table of affinities was the most abstract result of the chemical inquiry, and in Goussier's plate the table appears just below the laboratory image. So we see that theory supports the laboratory work, and the 'support' is strengthened by a peculiar iconographic aspect. Blank separators are present along three sides, but absent at the border with the laboratory image. The connection between the physical place (above) and the theoretical place (below) is very strict.

The plate we are studying is strictly connected with the article *Laboratoire* of the *Encyclopédie*. The entry *Laboratoire* was written by Gabriel François Venel, the author of many other entries. It is very interesting the fact that the article *Laboratoire* concerns only the operations of a single science, indeed chemistry: "lieu clos & couvert, [...] qui renferme tous les ustensiles chimiques qui sont compris sous les noms de *fourneaux*, de *vaisseaux*, & d'*instrumens* [...] & dans lequel s'exécutent commodément les opérations chimiques. Voyez nos *Pl. de Chimie*, *Pl. I.*".<sup>24</sup> We remark that in the *Encyclopédie* chemistry is presented as a discipline autonomous from physics since the "Discours préliminaire des editeurs", written by Jean le Rond d'Alembert. In the "Discours préliminaire" physics is presented with these words: "Physique ou Etude de la Nature", "le principal mérite du Physicien seroit, à proprement parler, d'avoir l'esprit de Système, & de n'en faire jamais. A l'égard de l'usage des Systèmes dans les autres Sciences, mille expériences prouvent combien il est dangereux. La Physique est donc uniquement bornée aux observations & aux calculs".<sup>25</sup> Later in the text, chemistry is presented in a much more active attitude: "La *Chimie* est imitatrice & rivale de la Nature: son objet est presque aussi étendu que celui de la Nature même: ou'elle *décompose* les Etres; ou elle les *révivifie*; ou elle les *transforme*, &c. La *Chimie* a donné naissance à l'*Alchimie*, & à la *Magie naturelle*".<sup>26</sup>

The words *physique* and *chimie* have a similar, almost equivalent, lexical presence inside the text of the *Encyclopédie*: 1806 occurrences for *physique* and 1592 occurrences for *chimie*. This somewhat surprising situation of an almost lexical equality may be interpreted as the sign of a tendential process towards a social-epistemological equality between chemistry and physics. In fact, if we look at the upper particular of figure 4, and read the captions of the plate, we discover that the principal characters of the whole scene are a *physicien* and a *chimist*, who are discussing on the problems of dissolution: "Physicien conférant avec un Chimiste sur la dissolution".<sup>27</sup> Goussier's plate testify that the distinction between chemistry, with its practitioners and *ad hoc* spaces, and physics was a cultural, possible choice - choice that, as it is well known, the great Lavoisier always defined in a unique direction, presenting himself as a *physicien*.





Figure 4. Details of the plate *Laboratoire et table des Rapports*, by Louis-Jacques Goussier, 1763.

We add a last remark on the details of figure 4. The support for funnels, the same funnels, and –in particular– the stem glasses testify that chemistry is living a phase of transition from the use of cumbersome apparatus to devices of more modest dimensions. On the whole, the *Laboratoire* is presented in the *Encyclopédie* as the other *ateliers* and *officines*, with a plate which explains the running of the particular place and many other ones (in this case 23) which illustrate instruments, apparatus etc. used in that particular place. The most important difference is in the plate of Figure 3, where it is put in evidence the importance of theory.

### ***Biedermeierzeit*: Wilhelm Trautschold**

Wilhelm Trautschold was born in Berlin in 1815; until 1832 he studies in his native town, and until 1838 he went on with his apprenticeship in Düsseldorf. In 1840 he began to work at Giessen, where he was a drawing teacher for the University in the years 1843-1846; by then he painted the official and now famous portrait of Justus Liebig. In the period 1846-1860 Trautschold worked in several European towns (Liverpool, Berlin, Rome, Florence) and eventually in the period 1860-1873 he lived in London. He died in 1877 in Munich.

Trautschold is not an important figure in the history of German art; however his works (and life) are really representative of the *Biedermeierzeit*. In the German speaking Europe the label *Biedermeier* refers to work in the fields of literature, music, arts and interior design in the period since the Vienna Congress in 1815 until 1848, the year of the European revolutions, and the label contrasts the works of the *Biedermeierzeit* with the Romantic era which preceded it. In reality, the appellation *Biedermeier* was derogatory, but in our case this kind of art work

may be singularly useful, because as wrote Koetschau: “Die bürgerlich-patriarchalisch Gesellschaft der Universitätsstadt Giessen [...] hat Trautschold in einer so ungekünstelt vornehmen Weise festgehalten, wie sie damals [...] nur recht vereinzelt vorkam”.<sup>28</sup> In this quotation the key word is *ungekünstelt*, because for us it is significant that Trautschold has described the bourgeois society of the Giessen University in an *unposed* way of representation. He tried to represent the middle class of his time, a middle class calm, unworried, working and far from the political troubles of the preceding Napoleonic period. Trautschold has been a figure of a certain importance in the history of the Giessen University; in 1907, in occasion of the third centenary of the University it was published a periodical, titled *Ludoviciana. Festzeitung zur dritten Jahrhundertfeier der Universität Giessen*. The journal published the reproductions of three works by Trautschold: the inevitable portrait of Liebig, the image of Liebig’s chemical laboratory (Figure 5), and the picture of the Corps of the Giessen Students during their annual festival. This last image illustrates a short, but precious, biography of Trautschold.<sup>29</sup>

Liebig’s famous Laboratory of Figure 5 was built in 1839, after he had received and turned down an offer of chair from St. Petersburg. The plate is one of the two not technical plates of the collection of *Acht Tafeln* published by the architect Johann Philipp Hofmann, the *Provincial-Baumeister* of Giessen. Since many years the architect was a friend of Liebig, and he was well aware of the great meaning of the new laboratory, whose construction he had been in charge of. With the publication of the collection Hofmann intended to give chemists and architects a fundamental example of building for chemical research. In the preface of the booklet with the explanation of plates, Hofmann affirmed that Liebig’s Laboratory was “der Anstalt, durch welche Giessen zu einer der bekannteste Universitäten in der Welt geworden ist”, and added: “Von den meisten Staaten ist das chemische Laboratorium in Giessen zum Muster für ähnliche Institute genommen worden”.<sup>30</sup>

In Figure 5 we see a large analytical laboratory with tables along the walls; cupboards and drawers below and shelves above the tables; students/scientists at work back to back. The perspective is closed by the flight of four hoods, but there is a central opening on the adjoining lecture hall; the message is evidently on the strict connection between formal and experimental teaching. It is better to speak of ‘formal’ and not of ‘theoretical’ teaching because an impressive feature of the image is the complete absence of any detail in the upper part of the plate. Semiotics makes a correspondence among the direction from the bottom to the top of an image and the passage from concrete to abstract. Figure 5 shows a neat opposition between a crowded lower part and an empty upper part; the meaning



Figure 5. *Innere Ansicht des Analytischen Laboratoriums zu Giessen*, engraving by Wilhelm Trautschold, in Johann Hofmann, *Das chemische Laboratorium der Ludwigs-Universität zu Giessen*, 1842.

of this opposition is that only the experimental work is important. It is also remarkable that all the faces are portrayed along a delimited strip. It is the hint put by Trautschold in order to read a narrative in the image.

Almost all the 14 characters who are working in the laboratory have been identified,<sup>31</sup> and this let us follow the narrative from left to right in a more interesting way. The first episode portrays Vicente Ortigosa, a Mexican student, in a relaxed, meditative pose; he is holding the famous and symbolic *Fünf-Kugel-Apparat*, what is well known and is in use. We remark that at the far left of the image we see many details of cupboards, drawers and shelves, with a serial repetition which underlines that this laboratory is the place also for serial works. A second episode depicts Wilhelm Keller, a North-American student, and Heinrich Will, Liebig's assistant, who face each other at the opposite sides of a table. Will is explaining something; perhaps he is suggesting Keller an experiment. At the centre of the scene, as a full stop in the narrative, we see Heinrich Aubel, the laboratory's attendant, with the unfailing mortar and pestle. The third episode shows Franz

Wilhelm Wydler, a Swiss student, and Franz Varrentrapp, a German chemist who with Will had introduced in 1841 a new procedure for the determination of organic nitrogen. As in the second episode, in the third one we find a student and an assistant working together, but in this case they are looking at a running experiment, waiting for the result. From the point of view of the connotative traits of the image, the fourth and last episode of the narrative is the most interesting. Here two gentlemen are intent on controlling the outcome of a reaction, and it is easy to suppose that they are discussing the final step of an analysis. Emil Bockmann is heating up a test tube under the attentive look of the last character of the scene, the gentleman with the top hat and a glove in the right hand. This last character is Wilhelm August Hofmann, by then a young student, and later a famous researcher and teacher. Wilhelm Hofmann was a gifted pupil of Liebig and the son of the *Provincial-Baumeister*, so the presence of his portrait in the plate is understandable; however the position at the far right –the place of novelty– seems prophetic.

To condense the iconographic narrative in a few words, in Figure 5 from left to right we may look at the four principal stages of any research, thinking on the known and its limits, projecting an experiment, carrying out it, controlling results and interpretation.

## Conclusions

We can now draw some conclusions from our sociosemiotic analysis<sup>32</sup> of the pictures by Stradano, Goussier and Trautschold. The three pictures offered images of chemistry and chemists whose interpretation is easy. In the Sixteenth and Seventeenth Centuries chemistry is seen as a Baconian fight against nature, among fires and smokes. In the Eighteenth Century chemistry is a calm, ordered inquiry driven by a clever curiosity. In the Nineteenth Century chemistry is a well organized, collective enterprise. The issue of continuity vs. discontinuity is complex, because we remarks, side by side, aspects of continuity and aspects of discontinuity. This is particularly true as far as apparatus and operations are concerned. Distillation and alembics are present in all our images, but on the other side the *physicien* and the *chimist* of Figure 4 are discussing on phenomena observed in small stem glasses, that are not present in the Baconian laboratory, and later, in Liebig's Laboratory, Bockmann is using a test tube, an item which was not in use at the time of the *Encyclopédie*.

An unexpected result of our research was the presence in all the images of iconographic documents on the division of labour inside the laboratory. In any image we see people reading, thinking, teaching or discussing, others who are using the instruments, and finally attendants and other workers. The articulate visual description of laboratory hierarchies testifies that the artists and their clients were well aware that the experimenter is not a recluse.

In the end, we think that the sharpest discontinuity is the institutional discontinuity. At the Medici court the laboratory and its activity was the outcome of a *personal patronage*. During the *siècle des Lumières* the running of a laboratory was a *private endeavour*, and eventually, in the Nineteenth Century the laboratory became a *public, state owned place*.

A last word on the semiotics of absence. It is really noteworthy that in Liebig's Laboratory we do not see Liebig himself. His absence is striking, because in a short *Vorwort* of Hofmann's booklet he wrote: "die Zugabe des äußeren und inneren Ansicht des Haup-Arbeitsraumes macht das Buch zu einem Denkmal der Erinnerungen für alle die, welche hier gearbeitet haben".<sup>33</sup> Probably, the absence of Liebig in the image of his world-famous *analytisches Laboratorium* has several meanings; anyway in our context this absence underlies the fact that now (in the Nineteenth Century) research is actually a collective enterprise.

## Bibliography and Notes

<sup>1</sup> E.g.: Marco Beretta, *Imaging a Career in Science: The Iconography of Antoine Laurent Lavoisier* (Canton, MA: Science History Publications) 2001.

<sup>2</sup> Gunther Kress, Theo van Leeuwen, *Reading Images: The Grammar of Visual Design* (London: Routledge, 1996).

<sup>3</sup> Luigi Cerruti, "Werner's Beitrag: A Linguistic and Epistemological Analysis", in: *Coordination Chemistry. A Century of Progress*, ed. George B. Kauffman (Washington: ACS, 1994), 43-56.

<sup>4</sup> Luigi Cerruti, "Chemicals as Instruments. A Language Game", *Hyle*, 4 (1998): 39-61.

<sup>5</sup> Luigi Cerruti, "The inherent complexity of chemistry", in: *Complexity in the living: a problem-oriented approach*, eds. Romualdo Benigni, Alfredo Colosimo, Alessandro Giuliani, Paolo Sirabella, Joseph P. Zbilut (Roma: ISS, 2005), 1-18.

<sup>6</sup> *Nova reperta. In Speculum diuersarum imaginum speculatiuarum a varijs viris doctis adinuentarum, atq insignibus pictoribus ac sculptoribus delineatarum* (Antwerp: Galle, 1638). We will consider also Straet's oil painting placed in the Studiolo of Francesco I de' Medici (1570).

<sup>7</sup> *Recueil de planches sur les sciences, les arts libéraux et les arts mécaniques, avec leur explication*. Seconde livraison. Seconde partie (Paris: Briasson, David, Le Breton, Durand, 1763).

<sup>8</sup> Johann Hofmann, *Das chemische Laboratorium der Ludwigs-Universität zu Giessen* (Heidelberg: Winter, 1842).

<sup>9</sup> Roland Barthes, "Rhétorique de l'image", *Communications*, 4 (1964): 40-51.

<sup>10</sup> Kress and van Leeuwen, *Reading Images*, 176.

<sup>11</sup> Quoted in: Giulio Lensi Orlandi, *L'arte segreta, Cosimo e Francesco de' Medici*, (Firenze: Convivio, 1991), on 109. Eng. Tr.: "The small office has to serve as cloakroom for unusual and precious objects, for valuable and artistic things, i.e. jewels, medals, cut precious stones and blown glasses and vases, ingenious and similar things, of suitable size and stored in their own cabinets, anyone of its kind".

<sup>12</sup> Eng. Tr.: "In the fire, the juice of all bodies is turned by art into a mighty billow, clear and most potent".

<sup>13</sup> John Daniels, Christian Daniels, "The Origin of the Sugarcane Roller Mill", *Technology and Culture* 29 (1988): 493-535, on 500-501.

<sup>14</sup> Michel N. Benisovich, "The Drawings of Stradanus (Jan van der Straeten) in The Cooper Union Museum for the Arts of Decoration, New York", *The Art Bulletin* 38 (1956): 249-251.

<sup>15</sup> Alice B. McGinty, *Stradanus (Jan van der Straet) his role in the visual communication of Renaissance discoveries, technologies and values* (PhD Thesis, Tuft University, 1974), quoted in: Alessandra Baroni, *Jan van der Straet detto Giovanni Stradano: flandrus pictor et inventor* (Roma: Jandi Sapi, 1997), 78-79.

<sup>16</sup> Anne Marie Thibault-Laulan, "Image et langage", *Dictionnaire Le langage* (Paris: C.E.P.L, 1973), *ad vocem*. Eng. Tr.: "Such situations offer a prime area for analysis by the scope of the corpus which facilitates the tracking of assemblage laws, an essential element of the system".

<sup>17</sup> Katharine Park, Lorraine Daston, *The Cambridge history of science*, vol. 3, *Early Modern Science*, (Cambridge: Cambridge UP, 2006), on 1-2. In a footnote Park e Daston remark that different European historiographic traditions give different time limits to this period; here we follow the anglophone historiography, so the term "early modern era" covers the period between roughly 1500 and 1750.

<sup>18</sup> Park and Daston, *Early Modern Science*, 2.

<sup>19</sup> The dictum *Veritas filia temporis* is attributed to Aulus Gellius, but the original text is: "Alius quidam veterum poetarum, cuius nomen mihi nunc memoriae non est, veritatem temporis filiam esse dixit.", Aulus Gellius, *Noctes Atticae*, Liber XII - 11,7.

<sup>20</sup> The *Monde* of the title means 'Universe'.

<sup>21</sup> Kenneth L. Taylor, "Marivetz, Goussier, and Planet Earth: A Late Enlightenment Geo-Physical Project", *Centaurus* 48 (4) (2006): 258-283.

<sup>22</sup> Jean le Rond d'Alembert, "Discours préliminaire des éditeurs", *Encyclopédie ou Dictionnaire raisonné des sciences, des arts et des métiers* (Paris: Briasson, David, Le Breton, Durand, 1751), vol. 1, i-lij, on xlij. "highly-skilled & very intelligent in all parts of Mathematics & Physics", "combines the practice of Design and a great knowledge of Mechanics [...] he was particularly busy with the plates of the *Encyclopédie* which he has all revised & almost all drawn".

<sup>23</sup> G. Dulac, "Louis-Jacques Goussier, encyclopédiste et ...'original sans principes'", in: *Recherches nouvelles sur quelques écrivains des lumières*, eds. Jacques Proust, Martha Fletcher (Genève: Droz, 1972), 63-110; on the collaboration between Diderot and Goussier see: John R. Pannabecker, "Representing Mechanical Arts in Diderot's *Encyclopédie*", *Technology and Culture* 39 (1998): 33-73.

<sup>24</sup> Gabriel François Venel, "Laboratoire", *Encyclopédie ou Dictionnaire raisonné des sciences, des arts et des métiers* (Paris: Briasson, David, Le Breton, Durand, 1753), vol. 9, 145. Venel's article is an important contemporary corroboration of chemists' *primazia* in the use of research laboratories; see: Maurice Crosland, "Early Laboratories c.1600-c.1800 and the Location of Experimental Science", *Annals of Science*, 62 (2005): 233-253. Eng. Tr.: "enclosed & covered place



[...] which contains all chemical utensils that are included under the names of *stoves, vessels, & instrumens* [...] & in which the chemical operations are conveniently run. See our *Pl. of Chemistry, Pl. I*.

<sup>25</sup> d'Alembert, "Discours préliminaire", on iv and xxxi. Eng. Tr.: "Physiscs or Study of Nature", "the chief merit of the Physicist should be, strictly speaking, to have the spirit of System, & never to do so. With respect to the use of Systems in other sciences, a thousand experiences show how it is dangerous. Physics is therefore only confined to comments & calculations".

<sup>26</sup> d'Alembert, "Discours préliminaire", Eng. Tr.: "*Chemistry* is imitator & rival of Nature: its purpose is almost as large as that of Nature itself: where it *decomposes* the Beings, or it *revives* them, or it transforms them, etc. *Chemistry* gave birth to *Alchemy*, & the *natural Magic*".

<sup>27</sup> Eng. Tr.: "Physicist discussing with a Chemist on dissolution".

<sup>28</sup> Karl Koetschau, Walter Cohen, Bernd Lasch, *Rheinische Malerei in der Biedermeierzeit* (Düsseldorf: Verlag des Kunstvereins für die Rheinlande und Westfalen, 1926). Eng. Tr.: "Trautschold has captured the bourgeois-patriarchal society in the university town of Giessen [...] in such an unposed and elegant way, as by then [...] it only sporadically occurred".

<sup>29</sup> Christian Rauch, "Wilhelm Trautschold", *Ludoviciana. Festzeitung zur dritten Jahrhundertfeier der Universität Giessen* (1907): 91.

<sup>30</sup> Hofmann, *Das chemische Laboratorium*, xi. Eng. Tr.: "the institution, which made Giessen one of the most famous universities in the world", "For most countries, the chemical laboratory in Giessen has become the model for similar institutes".

<sup>31</sup> Georg Schewedt, *Liebig und seine Schüler. Die neue Schule der Chemie* (Heidelberg: Springer Verlag, 2002), 126-129.

<sup>32</sup> In the title of this paper and in the text we should have used the form *(socio)semiotic*, because we said almost nothing on the social aspects of the iconographic communication. Anyway, the form *(socio)semiotic* is ambitious and cumbersome, and we trust that the reader will fill our silence (mandatory for sake of brevity) with his/her knowledge of usual cultural categories as 'early modern era', *siècle des Lumières* or *Biedermeierzeit*; see also Park and Daston, *Early Modern Science*.

<sup>33</sup> Justus Liebig, "Vorwort", in: Hofmann, *Das chemische Laboratorium*, vi-vii. Eng. Tr.: "The addition of exterior and internal views of the main work spaces makes the book a monument to the memories for all those who worked here".



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## Institutionalisation of Chemistry in Mexico during the Twentieth Century (1934-1970)

*Mina Kleiche Dray\**

### Introduction

Historically, Mexico's rich natural resources have provided a platform for its economic development. New-Spain had been the provider of gold and silver for the Metropolis, generating the extractive and mining industry expansion for the service of the Spanish crown in the XVIII century. Later, independent Mexico has also based its economic development on the exploitation of its main natural resource, oil, from the 1940's onwards. However, today Mexico doesn't own an autonomous economy based on exploitation of oil. The oil industry, major national economic industry, has not overcome its technological dependence on other countries. The industries derived from oil chemistry, pharmaceutical chemistry, and in general the natural resources transformation industries participate only very little in the economic growth of the country. However, world development of these industries, as in Europe and the USA, is based mainly on scientific and technical knowledge, generated, conceptualised, codified and applied by chemists (both engineers and researchers) working in their national academic and productive sectors.

This situation of dependency is the same today in other industrial sectors and in all economic sectors in Mexico and provides much for discussion by politicians responsible for science policy. All of them conclude that the main cause of this circumstance is the lack of links between academic and productive sectors. Since the North American Free Trade Agreement (NAFTA), which has opened trade exchanges with USA and Canada after 1994, the Mexican government has been applying strategies in favour of collaboration between academic and the productive sectors in view of creating a national system for innovation based on permanent partnerships. The objective herein is not to assess this new innovation poli-

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cy in Mexico but to give some of the historical elements which could explain how scientific professionalisation is a key factor to the understanding of the emphasis that national leaders now place on the need for increasing the interactions between the academic and industrial sectors in Mexico.

The process of chemistry institutionalisation in Mexico has been analysed through a review of the interactions between state, academy and productive sectors since the 1940's, a period when Mexico decided to modernise its industry, until the 1970's. The main hypothesis is that a socio-historical analysis of the interactions between political, economic and academic fields will allow the understanding of the present relationship with each other and how the scientific field was structured within a specific context.<sup>1</sup>

## Methodology

The process of institutionalisation in chemistry is discussed starting at a certain historical point of two principal institutions: The National School of Chemical Industries, created in 1916 and historically the main training centre for Mexican chemists, which in 1965 became the Faculty of Chemistry; and the Chemistry Institute created in 1941. Today these two bodies are located at UNAM (*Universidad Nacional Autónoma de México*), the largest and oldest university in Mexico.

The origin and the setting up of the National School of Chemical Industries are not given here because of the limitations of time and space. Instead, it will show how different events favoured the emergence of chemistry as a discipline with an infrastructure. Specific issues will be highlighted, including the a research group devoted to its practice with a legitimacy that was rewarded by several indicators such as diploma, title, journals, training, etc.

It has been explained previously how, until the end of the 1930's, chemistry practised in Mexico did not appear as an institutionalised discipline but as a group of three types of technical knowledge based on areas of professional activity;<sup>2</sup> these were:

1. Chemical-pharmacists devoted to the production of basic substances for medicine manufacturing and pharmaceutical industries;
2. Industrial chemical technicians occupied with quality control and rules inside public organisations;

3. Chemical engineers devoted to the maintenance and control of the production chain in the new national industry.

All of them graduated from the Tacuba School, also called the National School of Chemical Industries, the only school in Mexico devoted to training professionals with a specific education in chemistry.

However, according to their testimonies, by the end of the 1930's graduates from the Tacuba School could not find any good positions in industry. Most of them became either part-time teachers at the school or they went to work full time in the public sector, for example in the *Consejo Superior de Salubridad* (Higher Health Council), *Secretaría de Industria* (Ministry for Industry), *Secretaría de Hacienda* (Ministry for Treasury), *Secretaría de Guerra y Marina* (Naval and Army Ministry), where they performed in analysis and normalisation (meaning "quality control"), oil production control as well similar duties in sugar and alcohol production laboratories. If they found a position in any of the industries such as in soap, refineries and paper factories, they were placed in subordinate positions under the supervision of foreign technical staff (mostly English in the oil industry and Cubans in sugar refineries).

It will now be shown how in this context, a political shift and the initiative by some academics and industrial tycoons created a framework that allowed the emergence of chemistry as a scientific discipline with its professional practitioners.

## **I. Chemistry as a profession (1941-1965/1970)**

### ***I.1. The place of chemistry within political discourses and decisions***

The first question to be discussed delves into the *position of chemistry within the political discourse and decisions when it appeared for the first time as a premise for a science policy in the 1940's*.

In 1942, Manuel Avila Camacho's government created the *Comisión Impulsora y Coordinadora de la Investigación Científica (CICIC)*, supported by the *Secretaría de Educación Pública* (Ministry of Education), in order to encourage a basic science education for the first time which would favour industrial development. The recent (1938) re-appropriation of the oil industry invited a preference for achieving technological independence. Since science appeared as an instrument for progress, defined as economic growth, this was translated into the following actions:

1. Provision of postgraduate scholarships to favour the education of human resources.

Between 1943 and 1950, the CICIC gave 107 scholarships of which nearly half were destined to students traveling to foreign countries. A special attention was given to biology with 48.6% of the scholarships, followed by mathematics and physics with 29%. However, only 9.3% of scholarships were destined to chemistry and the rest were given to the Human Social Sciences.

2. Creation of new infrastructure.

Some institutes were devoted to research with specific objectives for solving specific problems of the country; other institutes were devoted to basic research in order to establish a national science base for supporting the technological development of the country. Thus, on the one hand, research institutes for tropical diseases and public health were created and on the other hand the *Instituto de Geología*, *Instituto de astronomía*, *Instituto de física*, *Instituto de matemáticas* and *Instituto de biología* appeared, but no institute for chemistry.

### ***I.2. “Los Químicos como Académicos” or how the academy becomes autonomous***

a) The Chemistry Institute, the first institute of education for chemistry researchers.

Finally in 1941, the Chemistry Institute was created. Fernando Orozco, director of the School of Tacuba, and Antonio Madinaveitia, a Republican Spanish scientist, initiated the creation of the Institute.<sup>3</sup> F. Orozco was one of the students sent in 1921 with a scholarship to the University of Marburg on the program established by Vasconcelos, the first Minister of Education in Mexico<sup>4</sup> after the Mexican Revolution begun in the 1910's. In Marburg, Orozco carried out his Ph.D. in analytical chemistry. Returning to Mexico, he was given a position as Professor in the Tacuba School, he set up a quality control laboratory for the government and finally in 1935 in the context of the reform of the School he took over its direction.<sup>5</sup>

Antonio Madinaveitia, doctor in pharmacy and chemistry, having graduated from the University of Madrid then taught chemistry at the University of Granada and Madrid, was one of the republican Spanish scientists invited by the Mexican government to continue his work in Mexico.<sup>6</sup>

Thanks to the financing by *Casa de España*, an institute especially created for the Spanish scientists,<sup>7</sup> and with the aid of some chemistry students from the Tacuba School by F. Orozco, Antonio Madinaveita opened a research laboratory at the School in 1939.<sup>8</sup> It was named *Instituto de Química* (IQ), with Orozco as director and Madinaveita as Head of Research.<sup>9</sup> However *Casa de España* quickly became *el Colegio de México* keeping only the human and social sciences departments, the IQ was integrated into UNAM in 1941 but without receiving any financial support. The relationship of Madinaveita with the Rockefeller Foundation made possible a financial grant to the IQ<sup>10</sup> which allowed the creation of two laboratories, plus a small library and the purchase of materials. Following this, *El Banco de México* also gave financial help in order to assess the natural resources of the country.

#### b) The creation of a PhD in chemistry

After vigorous debates in the technical council of the Science Faculty, the IQ was granted permission to start a doctoral postgraduate program specialising in chemistry. However the possibility of becoming full-time researcher or Professor in chemistry did not exist at that time in the UNAM and for this reason training at postgraduate level attracted only very few students. The creation of an institution devoted to research and training in chemistry was not enough for setting up chemistry as a profession. In fact other events linked to the process of autonomy in the UNAM motivated the growth of human resources for chemistry.

#### c) “El personal de carrera”: the status of full-time researcher (1944-1947-1953).

Indeed the attempts by President Lazaro Cardenas for reforming higher education after 1934 and for reinforcing qualified human resources in the new national industries caused resistance in the UNAM teaching corps, which had been fighting to preserve their autonomy. The following two outcomes were delivered:

##### 1. Creation of positions for Professors and researchers.

The 1944 UNAM law<sup>11</sup> defined the different categories of teaching and research staff at the University and established a set of rules forbidding them to practice other activities or to occupy other public positions.<sup>12</sup> In 1947, a new category of professors was created: full-time Professor whose activity was devoted to the teaching or any other activity related to this. As *profesores de carrera*, they were

not allowed to teach in other places. They were recruited from the *profesores ordinarios* after 5 years of service, being nominated by the directors of the faculties.

## 2. A boom in the recruitment for researchers during the 1950's.

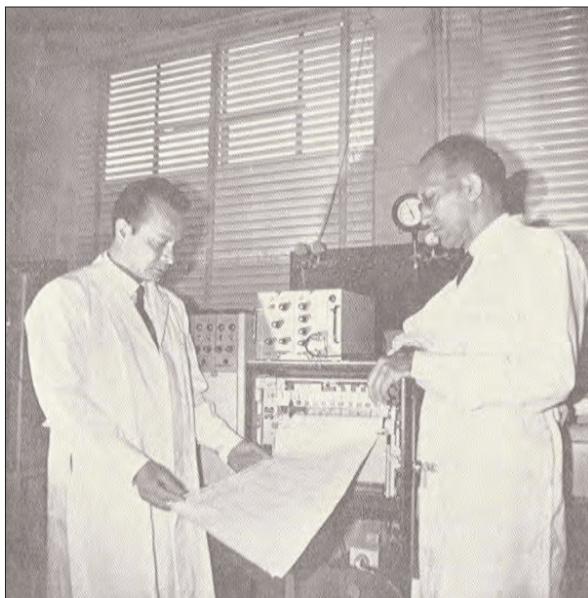
Nabor Carillo, rector of UNAM (1953-1957), created in 1954 a position for full-time researchers and provided a budget for recruiting Professors and researchers. These measures reinforced human resources at the IQ. Until 1951, the IQ recruited an average of 2 to 4 professors per year; in 1954 it recruited 20 professors and researchers, after this date the institute recruited an average of 10 professors per year until 1965.<sup>13</sup>

The events just described show how on the one hand the nationalistic policy of the 1940's allowed the creation of an infrastructure dedicated to research. On the other hand they show how the resistance against the same policy by the UNAM scholars allowed the autonomy process of the academy positions due to the creation of full-time research positions and a corresponding budget.

However if the interactions between political, economic and social fields allow us to understand, as showed previously, how a discipline appeared and acquired a certain social legitimacy, they do not inform us about the content of Mexican chemists' practices. In particular, what did they work on? How did they work? What were their scientific issues? Were they working on specific subjects related to Mexico, or at an international level on scientific universal issues? To answer these questions, the dynamics of chemistry research since the 1940's are reconstructed in the following paragraphs.

### ***1.3. Steroid chemistry: a scientific niche for the internal structuring of chemistry in Mexico***

Initially, the researches undertaken by the IQ were related to economic issues of the country. Then in the Second World War context, supported by the CICIC and Banco de México, the researchers focused their study on the country's natural resources in order to find substitutes for imported products. However, since 1948, when Syntex proposed to the head of IQ that they carry out research on steroids, a proper organisation for investigation was developed. Created in 1944, Syntex was the first company in the world to produce steroids on an industrial scale.<sup>14</sup> In 1945 the founders of Syntex, Mexican, North American, and European chemists, succeeded in producing progesterone, a steroid hormone extracted from the *dioscorera* plant and examined its therapeutic effects. Since 1934, progesterone had been produced in Europe



**Figure 1. Organic Chemistry Department of Syntex<sup>17</sup>**

from animal cholesterol (spinal chord from cattle) but in very low quantities and at a very high price. An American chemist, Russel Marker, discovered in 1940 the *dioscorera* in Veracruz State when researching certain plant steroids, the sapogenins, which molecular structure is very similar to cholesterol.<sup>15</sup> The *dioscorera* plant of Mexico, an endemic climber is very rich in sapogenins, which motivated Russel Marker and his Mexican partners to co-found Syntex in 1944 in Mexico City in order to produce progesterone cheaply and in large quantities.

<sup>16</sup> Seeking to synthesize other steroid hormones extracted from plants and new molecules with the same characteristics as human steroid hormones, and to produce them industrially, the head of Syntex sought and obtained the necessary human resources and the scientific infrastructure to enable these activities in IQ.

This collaboration attracted both researchers from the IQ and students preparing for the PhD in organic chemistry, specifically in steroid chemistry, because it offered them opportunities, to devote themselves to full-time research with added salary, to work with new substances and with new equipment such as microanalysis, as well as to enter a new research field that of the chemistry of natural products. In this way, IQ participated in the manufacture of steroid hormones that were naturally produced by the human body, such as cortisone (the miracle medicine that suppresses rheumatic arthritis, heals allergies and other inflammatory diseases<sup>18</sup>) and most importantly in one of the most significant discoveries of the XX century, the contraceptive pill.<sup>19</sup>

Thus the collaboration between Syntex and IQ built the scientific niche for chemistry as a scientific discipline in Mexico during the 1950's. However once again, the involvement of the government was a key factor which allowed a real consolidation of this field.



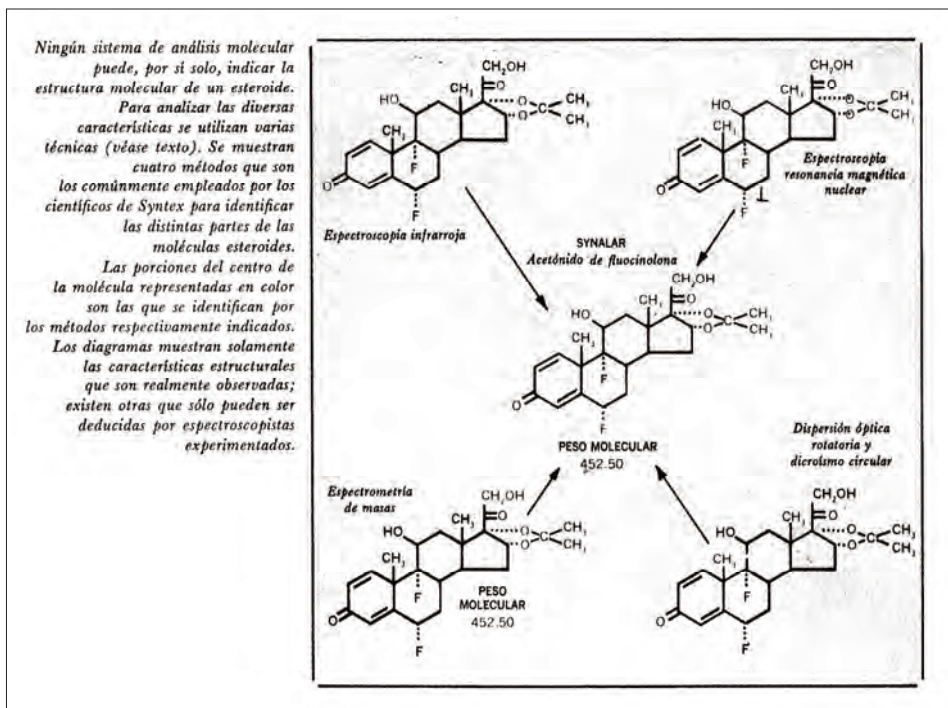


Figure 2. Examples of the processes used in Syntex in order to identify steroids molecules.<sup>20</sup>

## II. Consolidation of the autonomy of chemistry

### II.1. The Institutional decentralisation and diversification of subjects

Consolidation of the autonomy of chemistry came first of all from the creation of other institutions devoted to chemistry research. In 1961, the UNAM Rector Chavez, who wished to open the University to the intellectual and scientific elite of the country,<sup>21</sup> eliminated the position of researcher at the faculty, raised the salaries and provided budgets for the purchase of laboratory equipments.<sup>22</sup> In this way the *Division de Estudios Superiores* (later the *Division de Post-Grados*) was created in 1965, transforming the school of Chemical Sciences into a faculty, *La Facultad de Química* and reestablished the PhD program once again. This event brought the opening of new departments on areas other than organic chemistry, notably in inorganic chemistry, biochemistry, chemical engineering, theoretical and experimental chemistry and quantum chemistry.

The creation of other research institutes for chemistry in different places initiated the diversification of research subjects as well from the 1960's onwards: the department of chemistry in the CINVESTAV in 1961, the creation of new research lines in the School of Biological Sciences at the Polytechnical National Institute, The Chemical Sciences School in Puebla<sup>23</sup> in 1963 an applied research in the *Instituto Mexicano del Petroleo* (Oil Mexican Institute) in the oil chemistry field (catalysis, polymers and fertilisers) from 1966 onwards.<sup>24</sup>

## ***II. 2. Institutionalisation of a science policy (The 1970's)***

The second key movement for consolidation of chemistry was the setting of a science policy in Mexico. Since 1970, Mexican politics were characterised by the renewal of a nationalist debate which considered reducing the economic dependence of the country on external influences. A link between the state and the national private capital was suggested as a means for overcoming the imbalances and inefficiencies of the productive sector and also to protect the country from private and foreign interests.

The government created the *Consejo Nacional de Ciencia y Tecnología* (CONACYT) in 1970 as an organisation in charge of planning, coordinating and assessing science and technology government policies and linking these with national development. In the field of chemistry, an important decision of CONACYT during the seventies was to create the *Centro de Investigación en Química Aplicada* (CIQA, Centre for Applied Chemistry Research) in Saltillo in 1976 in the state of Coahuila, as part of the decentralisation of science that began during this period. The 1970's science policy supported chemistry in a permanent way, which allowed reinforcement of the infrastructure and training of human resources.

## **Conclusión**

One can realise that historically the extractive industry and the industrial modernisation based on oil chemistry spurred the economic development rather than scientific activity in itself which should have played a main role to support it.

Research in the field of steroid chemistry promoted by the industrial sector, represented a strategy for industrial development as an investment factor, and allowed the building of a participating niche for chemistry in XX century Mexico. Research in this case was not an academic response to the traditional demand by the state.

Indeed, these are the efforts of the state from the 1920's onwards who offered an infrastructure and human resources in favour of industrial development. Following this, the state played an important role in creating science policy allowing the support of research for itself which in turn allowed a diversification of research subjects, the creation of new institutions and, as a consequence of university autonomy as well as consolidation of a new scientific activity: chemistry.

As the position of scientific and technological knowledge was defined in the political projects after 1940, we have analyze the interactions of these representations with the academy and the productive sector. This in turn led to the institutionalisation of chemistry as a field of knowledge by the end the 1960's. Thus, having in mind local and international scientific issues, interactions between academy, productive sector and state, it has been shown that the requirements for the development of a research field are not only the links with social, economic and political issues of a country, but also the presence of an participating scientific niche.

This analysis demonstrates the importance of a specific historical context in the scientific institutionalisation is a key element for understanding the current political discourses of the country leaders expressing the need to stimulate interactions between the academic and productive sectors.

## Notes

<sup>1</sup> This paper is extracted from a larger study to be published in 2008: Mina Kleiche-Dray and Rosalba Casas-Guerero, "Institucionalización de un campo científico: el caso de la química en el siglo xx", *Revista REDES (Revista de Estudios Sociales de la Ciencia)*, 40 pages, forthcoming.

<sup>2</sup> M. Kleiche-Dray y R. Casas-Guerero, "Universidad/Laboratorio/sectores productivos: La construcción de la comunidad de Químicos en México. Perspectiva sociológica e histórica", in *I Congreso Iberoamericano de Ciencia, Tecnología, Sociedad e Innovación, CTS+I, Sección 13. Universidad, Empresa e Innovación, México D.F. de 19 al 23 de junio de 2006*.

<sup>3</sup> P. Padilla Olivares, "Génesis de una Facultad", *Revista de la Sociedad de Química de Mexico* 45 (2001): 106-114.

<sup>4</sup> Vasconcelos arrived in power with an intensive educational project reaching all over the country. He gave an impulse to higher education by granting scholarships that allowed several Mexican students to train in Europe. Between 1920 and 1925, 20 students benefited from these funds, studying chemistry in Germany, France, Switzerland and Great Britain.

<sup>5</sup> Fernando Orozco played an important role as a consultant to Pemex in the production of petrol during oil expropriation, see Andoni Garritz & Juan Antonio Chamizo, *del Tesquite al ADN* (México : Fondo de la Cultura Económica, 1995), p. 32.

<sup>6</sup> He was also the founder of *Sosa Texcoco* Company in 1942, which manufactured caustic soda and sodium carbonate from water in the Texcoco lake, see *del Tesquite al AND*, p. 33.

<sup>7</sup> "...a refuge for those who could contribute to the cultural progress of the country...", see "Homenaje a Madinaveitia", *Revista Mexicana de Química*, Vol.24, 4, p. 157.

<sup>8</sup> "Cinco lustros de existencia", *Boletín del Instituto de Química*, XVII(diciembre de 1965), p. 83.

<sup>9</sup> "Cinco lustros de existencia", p. 84

<sup>10</sup> This help would continue until 1963.

<sup>11</sup> See M. Landesmann-Miklos, *Identités académiques et générations. Le cas des enseignants de biochimie de l'Université Nationale Autonome du Mexique(UNAM)*, Doctorat es Lettres et Sciences Humaines, option Sciences de l'Education, (Nanterre: Université de Paris X, 1997), Vol.1, chapter 2. Cadre Historique, disciplinaire et institutionnel, p.110-155.

<sup>12</sup> In fact in the foundation project of The National University in 1910, and in its autonomy law of 1929, the scholars sought a way to introduce research in the centre of university life and keep it apart from political influence. An internal standard for nominating Professors was suggested, which became the Academy of Professors. This Academy could not escape political or personal interests which helped to create gradual controversies. For this reason the rector Alfonso Casas promulgated a new constitution: *The organic law of the UNAM* which shifted power from the Academy to the University Council in 1944.

<sup>13</sup> See the paper, "Homenaje a Madinaveitia", p. 159.

<sup>14</sup> J.F.H. Arellano, *Anuario de la Comisión Impulsora y Coordinadora de la Investigación Científica* (México, 1994); C.CE. Lida y J.A. Matesanz, "El colegio de México: una hazaña cultural, 1940-1962", in *Jornadas 117* (1990), W.B. Murphy, *Science Serendipity. A half century of innovation at Syntex* (New-York: The Benjamín Company INC, 1994); F.G. Pereira, *Productos químicos y farmacéuticos* (México: Atlanta S.A., 1956).

<sup>15</sup> We have to know that diosgenin has an advantage related to cholesterol from which progesterone was manufactured. Cholesterol produced two intermediate substances which could turn into commercial products while diosgenin produced only one intermediary substance, 16-D from which chemists could access to almost all of the other interesting pharmaceutical steroids. This made Syntex the main provider of hormones to several pharmaceutical companies in the US and Europe who manufactured drugs used for human therapy, see Syntex, *Una corporación y una molécula. Historia de la investigación en Syntex* (Cuernavaca : Litoarte, 1967).

<sup>16</sup> Syntex, *Una corporación y una molécula*

<sup>17</sup> Syntex, *Una corporación y una molécula*, p. 100.

<sup>18</sup> It was quickly shown however that cortisone altered the metabolism producing oedema, hypertension and other secondary effects.

<sup>19</sup> In 1951 Rosenkrauz and his group produced the 19-nor-progesterone, and with Djerassi they provided the derivatives of this molecule which most important steroid was the German estiron or 19-nor-estiron, first synthetic pharmaceutical substance manufactured by Syntex and patented on 15/10/1951. Later, Luis Miramontes produced the 19-nor-17alfa-ethynilesteron and Syntex began producing for business the norentindone since 1951 This substance was sent to the USA for clinical studies and showed its efficiency in ovulation inhibition in 1956, opening a new research field for Syntex. See *Una corporación y una molécula. Historia de la Investigación en Syntex*, p.56, O.M.Lagos, *Estudios económicos de la industria químico-hormonal. Escuela Nacional de economía*. Tesis de licenciatura (Mexico : UNAM, 1962) and C. Lomnitz-Adler, *Las salidas del Laberinto* (México : Loaquim Mortiz-Planeta, 1995).

<sup>20</sup> *Una corporación y una molécula. Historia de la Investigación en Syntex*, p.102.

<sup>21</sup> "In the arrangement given by the University Council in April 10, 1962, we said that academic activity of university researchers is developed within the Institutes and their work must be focu-

*sed on the conditions and national problems. In this way research received a different structure from the faculties and schools : from this date on, university activity should be carried out within independent and specialized structures”, I. Carillo Prieto, El personal académico en la legislación universitaria (México: UNAM, 1996).*

<sup>22</sup> M. Landesmann-Miklos, *Identités Académiques et générations. Le cas des enseignants de biochimie de l’Université Nationale Autonome du Mexique(UNAM)*, Doctorat es Lettres et Sciences Humaines, option Sciences de l’Education, (Nanterre : Université de ParisX, 1997)

<sup>23</sup> In province, the state of Puebla, there had been a tradition for chemical research since the 19th century. In 1939, at Puebla State University, a degree for chemical pharmaceutical biologists was created and transformed in 1944 into the Faculty of Chemical Sciences. This Faculty would open postgraduate positions in 1970.

<sup>24</sup> M. Snoeck, *La industria de refinación en México. 1970-1985.*

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