

# Tarski's Intuitive Notion of Set

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**Abstract.** Tarski did research on set theory and also used set theory in many of his emblematic writings. Yet his notion of set from the philosophical viewpoint was almost unknown. By studying mostly the posthumously published evidence, his still unpublished materials, and the testimonies of some of his collaborators, I try to offer here a first, global picture of that intuitive notion, together with a philosophical interpretation of it. This is made by using several notions of universal languages as framework, and by taking into consideration the evolution of Tarski's thoughts about set theory and its relationship with logic and mathematics. As a result, his difficulties to reconcile nominalism and methodological Platonism are precisely located, described and much better understood.

“I represent this very rude kind of anti-Platonism, one thing which I could describe as materialism, or nominalism with some materialistic taint, and it is very difficult for a man to live his whole life with this philosophical attitude, especially if he is a mathematician, especially if for some reasons he has a hobby which is called set theory, and worse –very difficult”  
(Tarski, Chicago, 1965)

Tarski made important contributions to set theory, especially in the first years of his long and highly productive career. Also, it is usually accepted that set theory was the main instrument used by Tarski in his most significant contributions which had philosophical implications and presuppositions. In this connection we may mention the four definitions which are usually cited as supplying some sort of “conceptual analysis”, both methodologically (the first one) and from the point of view of the results obtained (the rest): (i) definable sets of real numbers; (ii) truth; (iii) logical consequence and (iv) logical notions. So we could reasonably conclude that for Tarski set theory was reliable as a working instrument, then presumably as a conceptual ground. As we shall see, there are some signs that the reason for this preference might have been its simple ontological structure as a theory, at least excluding the upper levels, the highest infinite.

Nevertheless, very little was known about Tarski's conception of set theory from the philosophical viewpoint, apart from some comments he made to his closest friends and collaborators, and the conjectures which could perhaps be

made from a few scattered remarks here and there in his publications. Fortunately, the situation is very different when we look at the unpublished evidence. Tarski left unpublished quite a few pieces of his work over the years, especially expository work, with no new technical results, including lectures actually given and transcriptions of contributions to meetings. Some of those lectures, as well as a few letters, have been recently published, containing materials useful for knowing more about his notion of set. Yet there are still more materials which have remained unpublished so far, which are highly relevant for the issues involved. Among them, there are two contributions to meetings in 1965, and the records of Tarski's conversations with Carnap and Quine in the early forties.

In the following, the most relevant passages of all of those materials will be quoted and analyzed, to try to throw some light on Tarski's ideas about the notion of set, especially from the philosophical point of view, in an attempt to present a global, coherent position in the philosophy of mathematics. Like Russell and Gödel, Tarski was much more sincere about his philosophical tendencies when no publication was involved, so his deepest beliefs are much easier to discern from those materials. Thus, in this case the archival work has been once again the only effective way to try to understand Tarski's actual ideas. The resulting picture is a fascinating struggle between his nominalistic tendencies and his professional need to behave *as if* the mathematical entities with which he was working actually existed.

## 1 Universal languages

It is usually believed, by following van Heijenoort, that after the semantic, or model-theoretic, revolution logic could no longer be seen as a universal language, especially in the Hintikka sense of a language regarded as a universal medium, and so that the old universality implicit in Frege and Russell was no longer possible. However, a few traces of universality can be discovered in Tarski's writings, and some of them are associated to set theory (ST), so we first need to discuss those traces. As we shall see, several senses of "universality" do appear in his work.

The expression "universal language" is used by Tarski in different senses. In the first sense, ordinary language is universal because it is unable to contain the fundamental difference between the object-language and the metalanguage; that is why ordinary language produces antinomies. In *Wahrheitsbegriff* (WB) Tarski called this type of universality *semantic universality* ([64], p. 262), and in this sense the introduction of the truth predicate in the metalanguage does not transform it into a universal language. In this connection, Tarski's distinction between object-language and metalanguage, needed to develop important semantic concepts, is not compatible with a truly universal language, in the semantic sense. But there are other senses of the expression.

A second sense appears in a footnote to the same celebrated monograph on truth ([64], p. 210, ft. 2):

In order to give the following exposition a completely precise, concrete, and also sufficiently general form, it would suffice if we choose, as the object of investigation, the language of some one complete system of mathematical logic. Such a language can be regarded as a universal language, in the sense that all other formalized languages -apart from 'calligraphical' differences- are either fragments of it, or can be obtained from it or from its fragments by adding certain constants... (...) As such a language we could choose the language of the general theory of sets which will be discussed in §5...

The same idea appears later again, where it is said that a complete system of logic should contain all semantical categories occurring in the languages of deductive sciences: "this fact gives to the language mentioned a certain 'universal' character, and it is one of the factors to which logic owes its fundamental importance for the whole of deductive knowledge" ([64], p. 220). Since this sort of universality makes possible for a language to embrace all other formalized languages of mathematical interest, I will call it *mathematical universality*. In this sense, a language which is mathematically universal should be capable to express the whole of mathematics.

In the last sentence of the quotation Tarski went so far as to consider the language of "the general theory of sets" as one of those universal languages in the mathematical sense, so we need to determine the relation between this general theory, as developed in §5 of WB, and what we know as standard ST today. A first difficulty arises from the fact that the last part of the quotation was added for the English translation of WB, appeared in 1956 (see de Rouilhan [7]), and that this can be related to the changes introduced in the original WB monograph by the famous infinitistic 1935 Postscript. In particular, this postscript has been pointed out by de Rouilhan as containing the abandonment of logic, regarded as a universal language, as opposed to logic as mere calculus. But as we shall see mathematical universality can survive that postscript.

A second difficulty is that the general theory of sets appearing in §5 of WB is a simplified form of the formal apparatus of *Principia Mathematica* (PM), but still containing types, and so it cannot be identified with standard ST. However, a few years later Tarski made clear that everything you do with type theory you can do with no types ST. Thus, in his lecture in Harvard from 1939-40 (Tarski [68]) Tarski said that when ST is correctly used, we can dispense with the complicated apparatus of type theory, and its variables of many kinds, and replace it by a single universe of discourse for ST, with just one type of individual variables. By individual variables he meant variables which range over individuals, not over properties, and other "abstract" entities, a requirement which could not be met by type theory, which uses variables of any type, so the PM type theory involves higher order logic. And this matched very well Tarski's well-known preference for nominalism for, although he was an as-if-realist (see below), he always chose, when possible, the simplest theory from the viewpoint of the ontological implications. In this way the theory might, presumably again, be somehow "reduced" to the material, actual world. (Yet see the final section of this paper.)

This 1939-40 idea deserves to be transcribed in its entirety in Tarski's own words, not only because it shows his viewpoint about the expressive power of the language of ST, but also because it will allow us to introduce a further sense of universality ([68], pp.154-65):

It is not at all obvious at first glance that every mathematical discipline can be reduced to a formalized theory of the standard type. The crucial point here consists in carrying out such a reduction for the general theory of sets, since as we know from the work of Frege and his followers, and in particular from Whitehead and Russell's *Principia Mathematica*, the whole of mathematics can be formalized within set theory. Only a few years ago most logicians, under the influence of *Principia Mathematica*, were inclined to believe that the formalization of set theory required a much more complex apparatus; they thought, namely, that a so-called "theory of types" was required in one form or another, that one could not deal with a single universe of discourse, and that one had to use variables of infinitely many kinds. We know today that we can dispense with such a complicated apparatus, and that we can formalize the theory of sets within the standard type, like any other mathematical discipline. In order to carry this out, we need only one non-logical constant, namely the sign " $\in$ " for the two-termed relation which holds between an element and a set to which this element belongs. Then the only concern lies in a careful selection of the axioms. They must be weak enough to escape the antinomies, but at the same time they must be strong enough to ensure, within our universe of discourse, the existence of sets which correspond to as large a class of sentential functions as possible...

Thus, since ST can be formalized with no use of type theory, and so it can be used to reduce the whole of mathematics, the language of ST fully deserves to be regarded as a *mathematically universal language*, our second sense of universality. For Tarski this universality should give ST a very special status in mathematics and metamathematics, as the ultimate tool for the foundational task, at least by the time when the lecture in Harvard was given. In this second sense our intuitive notion of set should be clear enough to serve as a true foundation for the whole of mathematics, as the whole of mathematics can be expressed in the language of ST.<sup>1</sup>

Besides, the quotation makes very clear that one of the main advantages of ST, once we dispense with types, is that we can work with just a single universe of discourse for the variables, so I think mathematical universality should be related to the problem of the fixed vs. variable universe of discourse, and also to Russell's old claim about the need for a totally unrestricted domain of the variable (for details, see my [48] and [51]). I believe for philosophical reasons Tarski should be regarded as a universalist, so defending a fixed, unique universe of discourse when an important concept was in discussion, as it happens with his famous

<sup>1</sup>Accordingly, de Rouilhan's thesis that the 1935 Postscript to WB supposed the abandonment of logic as a universal language seems to me to be inexact, at least in the broad sense in which Tarski regarded mathematical universality. Besides, due to the distinction between object-language and metalanguage, which is made in order to define truth without generating antinomies, WB was *already* lacking universality, at least semantic universality, before the postscript was added in 1935. Also, as we shall see below, mathematical universality was later to be linked to the universality of the universe of discourse, already from that postscript onwards.

definitions of truth, logical consequence, and logical notions. As we have seen he is very clear in the Harvard lecture about this, at least for his semantic program in the 30's. A little reflection shows this to be very likely the case.

In the WB a universal domain is required, in this sense of universality, as what is defined is not truth in a particular sense, or in a particular model, but in a unique, universal sense, where the only model involved is the real, actual world. Thus, no relativization of truth is involved in the famous definition.<sup>2</sup> Such a goal is not always achieved in practice, since a simplified form of type theory is used across the monograph, and for Tarski type theory involved a renounce to a single universe of discourse. However, the 1935 Postscript adds the important modification to dispense with the theory of semantic categories. That theory was rather obscure, but it was somehow related to the simplified type theory used by Tarski ([64], p. 215, fn.). In this connection, the main change from the 1933 text to the 1935 postscript is usually described as the replacement of a theory of finite types by one of transfinite types.<sup>3</sup> Let us see what this actually involves.

For once, the role of the 1935 type theory seems to me not very clear. Thus, although in the languages considered variables for individuals and for classes and relations<sup>4</sup> are used ([64], p. 268), and orders are kept (p. 269), Tarski introduces "variables of indefinite order which, so to speak, 'run through' all possible orders" (p. 271). He does so in a rather tentative way, by "reason of trials and other considerations" (*ibid.*), but he says to be "almost certain that we cannot restrict ourselves to the use of variables of definite order if we are to obtain languages which are actually superior to the previous languages in the abundance of the concepts which are expressible by their means" (pp. 270-271). And the latter was required to obtain the maximal level of generality for the definition of truth; thus, the concept of truth can now be defined for any language, no matter its order, as variables of transfinite order can now be used (p. 271). However, in this way the type theory used in former sections of WB, the simplified form of type theory, is transformed into a transfinite form of type theory, so one of the main differences with ST vanished. Thus, the new type theory was more closer to ST than ever, so to a true universal domain.

And this is confirmed by Tarski in a footnote, when he writes that from the languages considered "*it is but a step* [my italics, FRC] to languages of another kind which constitute a much more convenient and actually much more frequently applied apparatus for the development of logic and mathematics" ([64], p. 272, fn.). And these latter languages are described as having a very simple structure, and using just variables of indefinite order, which all "belong to the same semantical category" (*ibid.*). This seems to me equivalent, or very close, to the introduction of a universal domain, especially because Tarski mentions Zermelo

<sup>2</sup>See Simons [55], §6, about this; according to Simons, this approach agrees well with Bolzano's.

<sup>3</sup>See Gómez-Torrente [26], II, especially pp. 161-162.

<sup>4</sup>For the latter Tarski writes "variable sentence-forming functors" ([64], p. 268), and these were formerly explained as referred to classes and relations ([64], p. 213).

and his successors as having shown that “with a suitable choice of axioms it is possible to construct the theory of sets and the whole of classical mathematics on the basis provided by this language” (*ibid.*). Agreed, the single step mentioned was not taken in the actual development of the postscript; this would have perhaps required a complete rewriting of the whole monograph. However, the path is clearly pointed out to the possibility to do so, i.e. to use ST as a more convenient and acceptable tool. Therefore, it seems clear enough that mathematical universality is now linked to a new formalization of ST where a single universe of discourse, a fixed domain, is used, exactly in the way described in the quotation from the Harvard lecture above.

The same universality takes place, I think, in the definition of logical consequence. It is true that in this connection some apparent counter-intuitive consequences may emerge, especially related to arguments concluding in the cardinality of certain existing things in the world, but none of them should be relevant for Tarski, as he usually accepted the axiom of infinity as a sort of logical axiom.<sup>5</sup> A single, global universe of discourse would make impossible that “different” definitions of logical consequence might result. (In a similar way, different concepts of number should result from the PM type theory without the clause of the unrestricted, universal domain of the variable, through the axiom of reducibility.) Or worse, without that single universe of discourse, the concept of logical consequence could not actually be defined at all: “...it is impossible to define the proper concept of consequence adequately whilst using exclusively the means admissible in the classical theory of types” ([64], p. 413, fn. 2).<sup>6</sup> Thus, the renounce to type theory was required to reach the full universality needed to develop the semantic program Tarski had planned.<sup>7</sup>

Finally, in the 1966 logical notions lecture, it seems obvious that the universal, fixed domain is, again, the whole actual world, which is explicitly mentioned in the official definition (see the last section below).

Therefore, it should be noted that the fixed domain, the unique universe of discourse, could be considered as a truly philosophical requirement for Tarski’s

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<sup>5</sup>See Gómez-Torrente [25] and Bays [1] for opposite views about this issue, all of them raised by the seminal Etchemendy [10]. Mancosu [41] is a recent defense of the fixed domain interpretation, based on unpublished evidence.

<sup>6</sup>It could perhaps be objected that, since what Tarski is mentioning here is just *classical* type theory, he still might believe that the same definition of logical consequence was possible in the *simplified* form of type theory actually used by him in former publications of the 30’s. But this is simply not the case. Tarski made totally clear that he could not give the definition in former writings precisely because he “wished to avoid any means of construction which went beyond the theory of logical types in any of its classical forms” ([64], p. 413, fn. 2). And the “classical” form used in those writings was precisely his well-known, simplified form of type theory, the one dispensing with ramification and the axiom of reducibility; see papers VI, VIII (WB) and IX of [64]. Therefore, even with the usual simplification type theory made the fundamental definition impossible.

<sup>7</sup>In the 50’s Tarski used a standard, model-theoretic definition of logical consequence, where the universe of discourse is relative to a model, so domain universality is lost. Yet the entities involved would still belong to the set-theoretical universe.

whole project of clarification of deductive sciences. This can be regarded as a third sense of “universality”: the universality of our universe of discourse, so we can call this universality *domain universality*. As it was the case in Russell, for Tarski the universe of discourse should be totally unrestricted, except for working with “partial” theories, where we are trying just to playing with the notion of interpretation of one theory into another.<sup>8</sup>

Yet domain universality was introduced at a price. As Tarski said in the Paris 1935 congress, in his philosophical talk on the establishment of scientific semantics, the semantic concepts should be defined “in terms of the usual concepts of the metalanguage and are thus reduced to purely logical concepts, the concepts of the morphology of language”, and this procedure “proves to be closely connected with the theory of logical types” ([64], p. 406). But as we have seen, the only way to obtain domain universality involved an actual renounce to type theory, so Tarski's original goal to avoid semantics to be “an independent deductive theory” ([64], p. 405) were not to be reached. And this was important for him, since by that time he believed that when semantic concepts cannot be reduced to logical (or physical) concepts we cannot proceed in “harmony with the postulates of the unity of science and of physicalism” ([64], p. 406). Therefore, Tarski could not show the semantic concepts to be ultimately “logical” because he preferred mathematical universality to be linked to domain universality. (See last section below, for the question whether or not the very set theoretical notions are logical.)

However, there is still another sort of universality, a fourth one. In the last letter to Neurath of 1936, Tarski wrote that a universal language is not enough, because it should force us to introduce the usual semantic concepts ([27], p. 27), which I take to mean that this is impossible because it would produce antinomies, in not distinguishing between object-language and metalanguage, so universality here is the usual one: semantic universality. But immediately he goes on to say that if that problem was taken aside in the development of an actual science, some might then think that a single universal language would suffice, but this would be still incorrect for this reason (p. 26):

To pursue actual sciences, something like physics, one must have available an extended mathematical apparatus; now we know however, that for every language (therefore also for the presumed ‘universal language’) one can give entirely elementary number-theoretic concepts, respectively sentences, which in this language cannot be made precise, respectively cannot be proved.

Therefore no powerful scientific language can be universal. For one thing,

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<sup>8</sup>Domain universality is not contradictory with Tarski's well-know *relativism* about what notions should be regarded as logical and as non-logical. Since his 1933 definition of truth (originally earlier), which is equally valid for mathematical and for factual truths, he always defended the impossibility to draw a clear line between logical and factual notions. This line is an arbitrary one trying to split all notions into two kinds, but all of them belong to the same, unique world. For the same reason, his well-known relativism concerning the line of separation between logic and mathematics seems to proceed from his assumption that the important thing is the unique world underlying both of them, so the fundamental property of a language trying to describe this world is its mathematical universality.

thanks to the Löwenheim-Skolem theorem (LS) every consistent theory has a model in arithmetic; but then, by the Gödel incompleteness result, arithmetic has to contain sentences which are undecidable. If this is so, then there is a further sense of universality: to be really universal a language should be complete. We can call this fourth sense of universality *completeness universality*. So it seems that what is involved is that only very elementary languages (e.g., sentential logic) should be regarded as universal languages in this sense. And what about ST? According to Tarski this could be regarded as a mathematically universal language, but ST is also incomplete since Gödel 1931, so perhaps what Tarski had in mind is that if ST is regarded as a universal language, then it fails because it is incomplete: it has to contain undecidable sentences.

Thus, although the language of ST can be mathematically universal, it cannot be completeness universal, but this is no big deal, as no mathematically interesting language can be completeness universal. Also, ST can be domain universal, though not semantically universal, but this again is no big deal, because only ordinary language, or any language incapable of supporting the distinction between object-language and metalanguage, can be semantically universal, and semantic universality is the kind of universality generating antinomies.

Domain universality and mathematical universality fit each other very well from the philosophical viewpoint. If there is just one world, the real, actual world, then the language trying to mathematically describe this world has to have this world, a world of individual objects, as the unique universe of discourse, so this language, either the language of set theory or a similarly powerful one,<sup>9</sup> should be regarded as a universal language.

It has been written by Nihiluoto ([46], p. 18) that Tarski's position concerning the universality of language was an hybrid between the classic conception of language as a universal medium (one language, one world) and the model-theoretic conception of language as calculus (many worlds, many languages), as he thought there is just one world but many languages. In view of the many senses of "universality" that we have found in Tarski, this should be qualified in the sense that, if we accept the universality of ST as a language for all deductive sciences, its mathematical universality, especially when this is linked to domain universality, then there could be just one language as well, and so Tarski should be considered rather on the universalist side.<sup>10</sup>

## 2 Conversations with Carnap

In the academic year 1940-41 Tarski was in Harvard, together with Carnap, Quine, Russell and Hempel. A "Logic Group" was constituted, in order to hold regular meetings, and more private conversations took also place, mostly between Carnap, Tarski and Quine. Carnap took notes of those discussions, which are

<sup>9</sup>See below for algebra as a possible replacement candidate.

<sup>10</sup>See Feferman [14] and [15] for more details about this controversy.



now in the Carnap papers in the Pittsburgh University. Tarski's contributions to those discussions are very interesting from the philosophical viewpoint, so in this section some of the things Tarski said are quoted and discussed.<sup>11</sup>

The main issue of the discussion was the problem of a possible language to serve as a ground for the unity of science, so the logical-factual distinction, the nominalism vs. realism dispute, and the merits of a finitistic language were questions which emerged very often. Tarski systematically defended the ultimate need for a finitistic language, his well-known position against any analytic-synthetic distinction, and his nominalistic tendencies. As he fixed his global position in a very brief, threefold statement we can start by quoting it (Jan. 10, 1941):

I understand at bottom only a language that fulfills the following conditions:

1. *Finite* number of individuals.
2. *Realistic* (Kotarbinski): The individuals are physical things.
3. *Non-Platonic*: Only variables for individuals (things) occur, not for universals (classes etc.).

I think we have to take this very seriously. First, we can wonder what does it mean to say that he understood just finitistic,<sup>12</sup> first order languages. I believe he must be thinking of understanding as something similar to "visualize", or to "imagine", in the same way we imagine or visualize physical entities, situations or processes. But second, we can also wonder, in view of this "practical" limitation in comprehension, how did he manage to "understand" higher languages, i.e., languages of higher order involving very complex variables. Fortunately we have Tarski's reply to this question, just in Tarski's comments following the quotation above.

He says that he understood any other language in the same way he understood classical mathematics: as a calculus. And by that he offers the following explanation: "With whatever higher 'Platonic' statements [unreadable?] in a discussion, I interpret them to myself as statements that a fixed sentence is derivable (or derived) from certain other sentences." Therefore, higher order languages, and variables, were comprehensible to him just in a proof-theoretic way, and by this he meant that no deep comprehension was involved, in the visual, intuitive way, as it happens usually when we are in contact with physical objects. So we can perhaps call "physical intuition" to what was underlying the model of his actual comprehension of mathematical entities and statements. As we shall see below, there are other hints to support this interpretation.

<sup>11</sup>Greg Frost-Arnold is writing a Ph. D. dissertation about these records, entitled "Carnap, Tarski, and Quine's Year Together: Logic, Mathematics, and Science", and also planning a book to make Carnap's full notes public. He was so kind as to sending me his English translation of the notes, which I am using here. Also, Mancosu [42] is a first attempt of exposing the main lines of discussion between Tarski, Carnap and Quine in those conversations.

<sup>12</sup>Tarski later relaxed the first requirement, about the finiteness of the domain, to "we make no assumptions about the cardinality of the domain." Also, some acceptance of number theory *as applied* to physical objects seems to emerge here and there.

As for the fact that Tarski mentioned Kotarbinski, it should be remembered that he dedicated WB to him, and very often he mentioned him as his favorite philosopher, and his philosophical position, sometimes referred as “reism”, as the closest to his own. Also the fact that Tarski allowed the editors of his *Collected Papers* ([65]) to include a paper by Kotarbinski, which was partially translated by Tarski himself, has been mentioned by Corcoran as a sign that he considered Kotarbinski’s paper as a sort of summary of his own position in philosophy (see Corcoran [5]).

From this starting-point Tarski was consistently defending his nominalistic position, but there are a few more remarks specifically relevant to his notion of set. Let us have a detailed look at them.

First, there is a short remark where Skolem is explicitly mentioned. There Tarski was trying to state his main philosophical requirements for a true, finitistic viewpoint, avoiding Platonism. Then he adds that number theory has to be excluded from a language of science because by Skolem we know that all mathematics have a model in number theory (Jan. 10, 1941):

Why is even elementary arithmetic, with countable domain, excluded? Because, following Skolem, all of classical mathematics can be represented through a countable model, so it can be expressed in elementary arithmetic, e.g. when one takes  $\in$  as a certain relation between natural numbers.

Let us first suppose this referred to Skolem’s celebrated 1933 result that elementary arithmetic can be shown, in an effective way, to have non-standard models. So if by LS every mathematical theory can be “reduced” (in no effective a way though) to number theory, and number theory have non-standard models, i.e. exotic, freaky models which have very little to do with numbers, then our intuitive notions of mathematical “objects” are totally jeopardized, in the sense that we might not know what are we talking about when talking about them.<sup>13</sup>

Also, what has this to do with ST? Well, number theory is reducible to ST as a mathematically universal language (see above), and ST is also vulnerable to LS. Thus, if ST is consistent, as we believe it is, then it has a model, then it has a countable model (a model in number theory, in the domain of natural numbers), so by Skolem it has non-standard models as well, then ST might not be reliable enough from the foundational viewpoint. The axioms of ST would then have models very far from our intuitive notion of set, the model we think we have in mind when we talk about sets, and when we use and apply ST, so ST would not be safer than arithmetic, and might not be a good basis for foundational tasks. Thus, the assumption that Tarski was referring to Skolem 1933 allows us to somehow understand Tarski’s assertion that elementary arithmetic should be “excluded”.

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<sup>13</sup>In the context of Tarski’s physicalist attitude and rejection of classes, talk about mathematical objects should be always understood in the reductive sense; see below on Tarski’s Platonistic reductionism.

Now let us suppose that Tarski was referring just to LS, so not thinking of Skolem's non-standard models of arithmetic. Then all consistent mathematical theories will have a countable model in arithmetic. This includes ST, in whose case membership is interpreted as a relation among natural numbers. Then the only reason to "exclude" arithmetic might be that in this way number theory would allow the whole of mathematics, so including lots of Platonic entities, to be accepted in the language of science which Tarski, Carnap and Quine were looking for at that time. Well, both interpretations seem to be possible. I like the first one best, mostly because of its philosophical interest, although I admit the second to be much simpler, and perhaps the right one.

At any rate, something is clear: Skolem's well-known "relativism" of set theoretic notions is independent of his construction of non-standard models for arithmetic, as it was formulated long before. As Skolem wrote already in 1923 (I quote after Hao Wang [70], p. 127):

The most important result above is that set-theoretic notions are relative. I had already communicated it to F. Bernstein in Göttingen in the winter of 1915-16. (...) I believed that it was so clear that axiomatization in terms of sets was not a satisfactory ultimate foundation of mathematics that mathematicians would, for the most part, not be very much concerned with it. But in recent times I have seen to my surprise that so many mathematicians think that these axioms of set theory provide the ideal foundation for mathematics; therefore it seemed to me that the time had come to publish a critique.

This was of course applied to the continuum hypothesis (CH) and similar problems (*ibid.*):

Since Zermelo's axioms do not determine the domain B [the model for them], it is very improbable that all cardinality problems are decidable by means of these axioms. For example, it is quite probable that what is called the continuum problem is not solvable at all on this basis; nothing need be decided about it.

So Skolem was probably convinced that when we talk in general of "set" or "set theory" we do not know what we really mean, or even that by those expressions nothing clear could actually be meant at all.<sup>14</sup>

It is then likely that those "limitative" results (LS, Skolem 1933 and Gödel 1931) helped Tarski to leave his work in ST (see below) after finishing with his "semantic program" in the 30's. This can be now illustrated by some evidence in Tarski's conversations with Carnap, where we find a very surprising paragraph.

<sup>14</sup>For an excellent presentation and analysis of Skolem's set theoretic relativism, see Jané [31]. By the way, this paper mentions, as reported by Skolem, that in 1958 Tarski made an objection to Skolem that his insistence on countable models could be reversed by using the upward LS, so presenting the reductionistic flavor towards uncountable ones instead. Jané shows how Skolem dispensed very quickly with this criticism, but for us it requires of some discussion. After all, if Tarski was somehow vulnerable to Skolem's relativism, why he did not accept it and was rather apparently objecting to it? I think that Tarski's argument could be seen not as an attack to Skolem's "partial" relativism, but rather as an endorsement that this relativism can be constructed both ways, so presented as a still deepest form of relativism. A different take on this criticism will be considered at the end of the paper.

They were looking for a global, unified language for science, in the spirit of the old Vienna Circle, so Carnap asked Tarski if that language should be constructed with or without types. Then Tarski replied (March 6, 1940):

Perhaps something completely different will develop. It would be a wish and perhaps a guess, that the whole general set theory [die ganze allgemeine Mengenlehre], as beautiful as it is, will disappear in the future. Platonism begins with the higher levels. The tendencies of Chwistek and others (“Nominalism”) to talk only about designatable things [Bezeichenbarem] are healthy. The only problem is finding a good execution. Perhaps roughly of this kind: in the first language, natural numbers as individuals, as in Language I, but perhaps with unrestricted operators; in the second language, individuals that are identical with or correspond to propositional functions in the first language, thus to the properties of natural numbers expressible in the first language; in the third language, the properties expressible in the second language are taken as individuals, and so forth. So in each language one has only variables for individuals, but nevertheless they cover entities of different levels.

First, it is surprising that for Tarski the whole general ST might disappear in the future! The expression he uses is the same we already discussed in the first section above, so he presumably was referring to the same, the general theory of sets, with types, used in §5 of WB. The further reference to “higher levels” would match this interpretation, as those levels were used in type theory. However, it is hard to see why Tarski might have thought in 1940 that type theory could “disappear” in the future, since the typical theory of types from PM was hardly used by that time, not even in the simplified form of §5 of WB. Besides, as we also saw above, for Tarski type theory could be fully dispensed with and replaced by no type ST, so obtaining the same mathematical universality in a simplest way, plus domain universality.<sup>15</sup> Therefore, it is likely he was thinking just of some sort of ST, which would explain that he found it “beautiful”, an adjective he probably would not use to describe type theory. If so, this would be compatible with his increasing doubts about ST, as we shall see in the following.<sup>16</sup>

As for the possible replacement for general ST, at first I thought he was perhaps thinking of something much more general, as category theory, where membership is defined, then eliminated, so ST could be reduced to category theory. But category theory developed from 1945 onwards, and it seems that Tarski did not like category theory anyway, probably for not allowing the sort of individual variables he was looking for. Thus perhaps he was thinking of some sort of a very general algebra and foresaw some ways to use it as a main tool for the foundations of mathematics. If this was so, that could help understanding the

<sup>15</sup>As I pointed out in the first section above, although Tarski originally tried to use type theory in his semantic program, he was somehow forced to dispense with it in favor of ST, so as long as his own writings were concerned, the “disappearance” of type theory had already taken place. He did not like ST either, but ST was more convenient for certain purposes. See the last section below for more details about this issue.

<sup>16</sup>See Mancosu [42], especially §2, for more details about Tarski’s ideas to dispense with type theory in his conversations with Carnap.

1940's "algebraic shift" which has been mentioned as a turning point in Tarski's development.

Tarski's algebraic shift of 1937-1940 is well documented in Givant [21] and [22]. He probably felt that some sort of algebra could serve as a new foundation for mathematics, returning so to the point of view of Löwenheim and Schröder. In particular, Tarski showed that his algebra of binary relations was able to provide with "a suitable framework for formalizing all of mathematics" ([21], p. 24; see [21], pp. 53 ff.). This might have been seen by Tarski as a solution of all the problems associated to PM type theory, and also to the ultimate indeterminacy of ST. From the ontological viewpoint, algebra was certainly better suited for a nominalist: "Even algebraic symbols were taken to represent particulars, particular numbers or particular quantities" (Hintikka [29], p. 363). Algebra, like ST, is not completeness universal nor semantically universal, but after Tarski's elaboration may be mathematically universal and domain universal. The main difference from the ontological viewpoint may be that the individuals constituting the algebraic domain might be seen as more akin to what Tarski had in mind when requiring genuine individuals as the raw material for the individual variables to range over. If this interpretation is correct, we would be before a very important, technical option, which may have been inspired on clear philosophical reasons.<sup>17</sup> At any rate, Tarski continued to use ST when it was convenient to him.

Also, in the quotation he talks about general ST as a beautiful thing, and this seems much more easy to explain, as he always loved classical mathematics, and considered mathematics not only as a science but also as an art. As Corcoran has recently written (in correspondence with me):

Tarski loved classical mathematics including Euclidean geometry, number theory, algebra, and analysis -he never held back on anything classical out of philosophical scruples. He hated people saying half-baked skeptical things about classical mathematics -this included Wittgenstein (at the top of the list, cf. Feferman account), the intuitionists, the logicians and the formalists. Tarski was a Platonic reductionist -he used whatever Platonic entities that were needed for classical mathematics and its metatheory- but he always imagined that these apparently Platonic entities were really somehow reducible to physical or mental things.

Next, he praises nominalism from his teachers, which shows that his philosophical tendencies were a very strong influence on him. This could go even so far as to somehow have compromised some of his technical options, as it can be clearly seen in the quotation. This does not seem to be totally compatible with Corcoran's opinion above, but we must not forget that we are talking of unpublished evidence, taken from records of private conversations. As we shall see below, in similar contexts Tarski felt much more free to express his deepest philosophical beliefs.

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<sup>17</sup>Tarski's big output after the algebraic shift is well represented by his work on cylindric algebras, which may be seen as the algebraic way to do first order logic, and on ST without variables, which might perhaps be interpreted as a way to dispense with the problematic range of the ontologically unrestricted variables of ST.

Finally, in the quotation Tarski seems to presuppose that having only individual variables dealing with entities of different levels would be a good accomplishment. This shows that his preference for the least ontologically exuberant system was a very early one. Also, this can be clearly connected with the above mentioned Harvard lecture's preference for ST over type theory on philosophical grounds. (Yet things may not be so simple, as we shall see in the last section below.)

As for Corcoran's suggestion to call Tarski's position "Platonistic reductionism", it is a very interesting one, so I shall use it in the following. As a working logician and mathematician Tarski behaved as if the entities which he used actually existed, though ultimately he admitted just individual, physical objects, so he could be called an as-if-realist also. Similar positions have received similar names: methodological realism, working realism, or internal Platonism,<sup>18</sup> but I think Platonistic reductionism is more informative, for it adds the reductive element. Besides, it matches very well the physicalist tendencies that can be found in Tarski's work, and physicalism is clearly a reductivist position.<sup>19</sup>

After all, it was in the spirit of the old logicism, and also of the old logical empiricism, to present most of the entities under consideration as a sort of "logical constructions" in terms of more solid, reliable entities (see my [49] for details about Russell's constructive definitions). So Platonistic reductionism could be seen as Tarski's own version of those old tendencies. Yet the clear empiricist tendency always present in Tarski's philosophical approach should not be interpreted as if Tarski was somehow returning to the old logicist masters: his sort of reductionism should even be regarded as a pioneering work towards an empiricist philosophy of mathematics, or even towards a set theoretic naturalistic position as it is well-known today (see for instance Maddy [40]). Finally, reducibility to physical or mental entities should be understood, I think, in the sense that all mental entities are ultimately also reducible to physical ones, as seems to be clear in Tarski's quotation above concerning Kotarbinski.

### 3 Is set theory a wasting of time?

Let us return to ST. As we have seen, by the end of the 30's ST was already infected by several limitations which clearly affected its conceptual basis. Because of the Gödel limitations, ST could not really be regarded as a universal language in the sense of completeness universality, and due to LS and to Skolem 1933 there could not possibly be a clear model for it. But the situation was to become still

<sup>18</sup>See Jané [32], and the references cited there to Shapiro and Ferreirós.

<sup>19</sup>See Frost-Arnold [20] though. He has showed quite convincingly that physicalism was not Tarski's main motivation in WB, but solving the semantic antinomies. Frost-Arnold's criticism of the classical Field [19] paper is also convincing because that paper was based mostly on a unique mention by Tarski of the word "physicalist". Yet there is another mention of the same tendency in Tarski's contribution to the 1965 Chicago meeting, although it seems it was made in a rather casual way.

worse: in 1939 Gödel showed the axiom of choice (AC) and CH to be consistent with the standard axioms of ST. A first, initial implication of this was some talk about non-Cantorian ST, in case it could also be proved that the denial of CH was also consistent with the Zermelo-Fraenkel theory with the axiom of choice (ZFC) (which had to wait until Cohen 1963), so the Gödel result could be seen as partially confirming the Skolem suspicious attitude towards the indeterminacy of ZFC.<sup>20</sup>

All in all, this could help understanding what Tarski said to Carnap in 1941 that “general” ST might disappear in the future. As we saw above, this and similar results may have caused Tarski to abandon his technical work on ST, and also help to explain the algebraic shift mentioned. This is confirmed by Givant's recent letter to me:

Concerning the many of the open questions in set theory, his attitude was rather negative. Most of these statements had been proved to be independent of the usual axioms of set theory, and seemed to be unresolvable by reasonable new axioms that would be accepted as “self-evident” by the majority of the mathematical community. He believed that much of the current research into the foundations of set theory -the results concerning the truth or falsity of this or that statement in various set-theoretical models- would eventually hold little interest for most mathematicians and would even be viewed by future generations in much the way that current philosophers view certain religious disputations of medieval scholastics. He thought that people would gradually stop working in set theory because there were too many unanswerable questions.

Yet Tarski continued to work on ST when interesting problems appeared. And this can be easily interpreted as one more evidence of the tension between his philosophical attitude and his research work.<sup>21</sup> As Givant adds in the mentioned letter:

I would like to distinguish between Tarski's personal philosophical point of view and his actual practice in his mathematical work. He was not inhibited in his use of transfinite, or infinitistic, methods, and this applied to all aspects of his work, not only to his work in set theory. In fact, I believe he found certain more finistic aspects of logic, such as recursion theory, distasteful. Most importantly, he was very much concerned with problems he thought would be of interest to a broad spectrum of mathematicians -not just to logicians. (...) Tarski did regret that he had spent so much of his life working in set theory. I am not sure when this regret was first felt, but already by 1930 he had begun to move away from set theory. Still, when interesting set-theoretical problems occurred to him, even late in life, he would follow through on them. His papers with Erdős and with Keisler are just two examples. So it seems that disillusionment with set theory did not carry with it, as a corollary, total rejection of this domain. His intellectual interests were always the dominant force, not his philosophical beliefs.

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<sup>20</sup>On the other hand, Gödel's 1939 result can also be seen as increasing the reliability of ST, as compared with type theory. Thus, those who formerly adopted type theory for fear that further contradictions appeared in ST, could then regard ST as a more reliable tool: “These consistency results undermined any remaining cautionary reason to adopt TT [type-theory]” (Ferreirós [18], p. 1938). However, Tarski had other reasons, not only technical but also philosophical, to dislike ST.

<sup>21</sup>That unresolved tension was already pointed out by Mostowski [44].

Thus, it seems that the possible impact of CH independence results on Tarski is a matter to be somehow investigated. However, there was no apparent reaction to these results in the published work by Tarski known to me. But if we study the unpublished evidence things are very different.

## 4 The continuum hypothesis

The first reference known to me is the one appearing in a letter to Gödel from 1946 ([72], pp. 271-2; I quote after Sinaceur [56], p. 13). In the letter Tarski is announcing a part of what he was planning to say in the Princeton conference (Dec. 1946), and we read:

As regards the question in which you are interested [absolute provability, definability, etc.], I don't think that I can do anything else [in the Princeton Bicentennial talk] but to emphasize the fundamental difference between all the undecidable statements known at present in elementary number theory on the one hand and some undecidable statements (like continuum hypothesis) in analysis and set theory; the statements of the first kind being clearly undecidable in a relative sense while those of the second seem to be undecidable in some absolute sense.

The first thing to notice is that he thinks CH to be undecidable, then independent from ZFC, so he somehow foresaw Cohen's result in 1963. Also, he thought CH to be absolutely undecidable, not just undecidable in relation to a certain axiomatization of ST. Yet in the actual talk Tarski said very little on CH, and after just mentioning the results of Gödel on CH, he added ([56], p. 26):

In discussing the relations between set theory itself and logic I shall begin by saying that I believe that the set-theorist may expect much from the formal logician. I believe that certain problems of set theory may actually be independent of the axioms of set theory and may be shown to be so independent by formal logical means.

So there is no explicit mention of his actual belief that CH might be absolutely undecidable according to the letter to Gödel. Also, he does not mention CH explicitly, and the example he offers after the quote is not referred to CH but to another technical problem (the Suslin one). Finally, it seems to me he was thinking of CH as well, in writing about the independence of ZFC. So we seem to be once again before one more example of the "conviction and caution" attitude, so dear to Gödel, but also very well-known to Tarski.

All in all, Tarski seemed to believe that CH might result to be undecidable, yet at the same time, in not making his belief public in the actual talk, he might believe that perhaps CH could be decided somehow in the future. This seem not to be compatible with what he actually wrote in the letter to Gödel (Gödel believed CH to be false!), where he talks about absolute undecidability, but even so he wrote just that this *seemed* to be the case. At any rate, Tarski did not share Gödel's intuitions about a future, when stronger axioms for ST might be found



that would bestow an absolute truth value on CH (see below, a further quotation from Givant's letter).

So in the 40's Tarski must have thought of ST as something not very reliable, in the sense of a good basis for his foundational projects. However, although ST could not be a universal language in the semantical sense, nor in the completeness sense, it was still capable to be a universal language in the mathematical sense: all mathematics can be reduced to ST, on pain, of course, of having to face the question of the non-standard models, and so the question of the low profile characterization of its "intended" model.

## 5 Are there large cardinals?

Large cardinals are those which cannot be constructed, i.e. their existence proved, by using the usual ZFC axioms. Inaccessible cardinals (IC) are among large cardinals (though there are many more classes of them), and are so exorbitantly large that the hypothesis of their existence can be used as a test about the ontological implications of ST and about the reliability of set-theoretic intuition. Tarski made a few contributions to the theory of those cardinals in the 30's and 40's, but did not return to the subject until the 60's. As we shall see, his latter attitude about ST was more skeptical, so this subject is useful to understand his philosophical evolution, especially because Tarski did not believe in large cardinals (John Addison 1996, personal communication).

In Tarski [59] and [60] there seems to be a rather bold attitude about the existence of IC, to the point that in them he "formalized the existence of arbitrary many inaccessible cardinals as an axiom, phrased in such a way that most of the axioms of set theory including the Axiom of Choice are derivable" (Kanamori [34], pp. 20-21). Curiously enough, this was done little after Zermelo's ontologically enthusiastic second order formalization of ST in 1930.

In his 1943 paper with Erdős ([9]) certain properties of cardinals are described which imply inaccessibility, and the difficulties to solve the problems involved are finally suggested not to depend on the nature of IC, but rather on the methods used, so (I quote after Kanamori [34], pp. 70-71):

It is quite possible that a complete solution of those problems would require new axioms which would differ considerably in their character not only from the usual axioms of set theory, but also from those hypotheses whose inclusion among the axioms has previously been discussed in the literature and mentioned previously in this paper (e.g., the existential axioms which secure the existence of inaccessible numbers, or from hypotheses like that of Cantor which establish arithmetical relations between the cardinal numbers).

As we shall see in a moment, the belief in the possible need of those "new axioms" was rejected when Tarski returned to the subject many years later.

Thus, in his paper to the Stanford 1960 conference we find something else which is useful for understanding Tarski's deep attitude towards ST, and the way

in which he may have taken philosophically inspired options in his technical work. The paper (Tarski [63]) has to do with certain very large cardinals; in particular, with the problem whether new set-theoretical axioms are needed to solve some problems related to IC. And this is said almost exactly with the same words than the ones used in the former quotation from 1943, except that instead of “It is quite possible...” we find “It seemed even plausible that...” ([63], p. 125).

The expression “new axioms” are probably somehow referring to the “stronger” axioms Gödel had said that, when they are added to the usual axioms of ST, they could solve, for instance, CH and similar problems. Tarski does not mention Gödel explicitly though, and says that those axioms would differ from general CH or existential hypotheses assuring the existence of IC, but it seems to me quite clear that he was thinking of the ontological problems raised by Gödel (see below).

The paper uses infinitary languages and the compactness theorem for them (as introduced in Tarski 1958), and is based on results obtained by Hanf, one of Tarski’s students, “as a solution of a problem suggested by the author” ([63], p. 125), so it is clear that Tarski seemed to have been looking for something in that direction. This is very interesting, as the main claim of the paper is that *those strong axioms are not needed*, and that the cardinals involved in those problems behave “like all the accessible cardinals” (*ibid.*). This is not a denial of the existence of large cardinals, which had pleased Tarski indeed, as he did not believe in them, but at the end of the paper we can find some considerations with a clear philosophical flavor, precisely in that negative direction. Thus, we may be here at one place, probably unique in his publications, where Tarski’s philosophical nominalistic attitude towards ST seems to be closely related to his technical options and results.

According to Tarski ([63], p, 134):

The belief in the existence of inaccessible cardinals  $> \omega$  (and even of arbitrarily large cardinals of this kind) seems to be a natural consequence of basic intuitions underlying the “naive” set theory and referring to what can be called “Cantor’s absolute”.

The reader may think that the “natural” intuition mentioned was shared by Tarski; after all, as we have seen above he did not renounce to work with any entity involved in classical mathematics, in the Platonistic sense of “entity”. But things are very different, as this is followed by:

On the contrary, we see at this moment no cogent intuitive reasons which could induce us to believe in the existence of cardinals  $> \omega$  that are not strongly incompact [weakly compact], or which at least would make it very plausible that the hypothesis stating the existence of such cardinals is consistent with familiar axiom system of set theory.

It seems then clear that the “naive” intuition mentioned was not his intuition, and that some of the consequences of the belief in “Cantor’s absolute”

seemed to him, not only undecidable by the usual axioms of ST, but even probably inconsistent with them. As we have seen above, and we shall see still in a clearer way below, Tarski was a hardcore empiricist, so it is very likely that his intuitions about mathematical objects were mostly inspired in empirical objects, in those objects that can be visualized or imagined according to the model of the physical objects, and of the actual sets of them. That is why we called his type of mathematical intuition physical intuition.

Therefore, "naive" mathematical intuition must have seemed to him something not reliable, as it may lead us to entities which are not duly justified. Hence the tone of the reference to naive intuition and Cantor's absolute, which may well seem even of an ironic character. If this interpretation is correct, for Tarski classical mathematics should not embrace large cardinals, or at least some of them, the very large ones, and "believing" in a mathematical object should involve some connection with the empirical world.

This seem to me plausible according to the way he finished the paper: we need just add to the usual axioms of ST the statement that every cardinal  $> \omega$  is strongly incompact [not weakly compact] "to preclude the existence of 'very large cardinals'" (*ibid.*). He does not openly defend such an option, and even if "very large" cardinals were eliminated, there could still be other large cardinals, but the way in which he describes those who may be opposed to the new, limitative axiom is very suggestive:

Such a decision, however, would be contrary to what is regarded by many as one of the main aims of research in the foundations of set theory, namely, the axiomatization of increasingly large segments of "Cantor's absolute".

Thus, it seems clear to me that he did not consider himself among those "many", even more so according to the way he closes the paper with an implicit criticism of those who are ready to accept new "construction principles" and existential axioms, while "are not prepared to accept any axioms precluding the existence of such cardinals" (*ibid.*). So the implicit reference to Gödel's Platonism seems once again clear enough. As a result, this may show Tarski's inclination not to be among those who favor stronger axioms for ST, but rather among those who eventually might favor limitative axioms instead. We will return to this issue later.<sup>22</sup>

If my interpretation is correct so far, the 1962 paper would be Tarski's first attempt to supply strictly mathematical results as grounds to defend a philosophical position, even though this is made in perhaps a very subtle way (the conviction and caution attitude again!).<sup>23</sup> And the most curious thing is that Tarski only returned to technical work in ST, in the 60's, just to try to state

<sup>22</sup>Tarski's usual tension between nominalism and realism appears here once more time: while he is somehow attacking the ontological exuberance of large cardinals, he does so by using non-finitary reasoning, based on infinitary languages, despite these languages should be rejected by his finitary "instinct".

<sup>23</sup>Kanamori ([34], p. 42), in noticing the philosophical flavor of Tarski's language here, quotes him at length, then describes Tarski's words as "guarded", and mentions the possible, realist

results, some of which may be seen as dealing with what we can call the ontological credibility of certain large cardinals, and of certain existential set theoretical axioms to justify them. So the philosophical implications of these results seem to me highly relevant to understand the way in which he might have been concerned about the relation between the technical work on the least intuitive part of ST, the highest infinite, and his intuitive notion of set.

Gödel had suggested in his 1947 paper on CH that the very concept of set may require that the axioms of ST are extended by axioms of infinity to cover very large cardinals, and then ([23], p. 182):

Very little is known about this section of set theory; but at any rate these axioms show clearly, not only that the axiomatic system of set theory as known today is incomplete, but also that it can be supplemented without arbitrariness by new axioms which are only the natural continuation of the series of those set up so far.

So, Tarski's 1962 "philosophical" remarks were not passed unnoticed by Gödel, who made some comments about it in a footnote to his 1940 famous memoir about CH, added in 1965 ([23], p. 97, fn. 2). There Gödel describes what Tarski did by saying that some new, strong axioms of infinity were there "formulated and investigated", then he seems to agree with Tarski in that those axioms are not supported by set theoretical intuitions. Yet he adds that even so the truth of those axioms are supported by "strong arguments from analogy", and he mentions certain technical result in the field.

Nevertheless, I think the agreement must have been superficial, for as we have seen the end of the Tarski paper clearly suggests that he might favor even limitative axioms instead of existential axioms for very large cardinals. Probably that is why when Gödel quotes Tarski's words "basic intuitions underlying the 'naive' set theory" (see the quotation above) he does it incorrectly, as he actually writes: "basic intuitions underlying abstract set theory"! In this way he replaces "naive", a word which he probably disliked as applied to his own form of mathematical intuition, by "abstract", a much more neutral, so acceptable a term.

Given Gödel's perfectionism, this must be due, not to a lack of care in quoting, but rather to some sort of *lapsus calami*, probably due to his unconscious Platonistic tendencies. It seems to me quite clear that Gödel noticed Tarski's implicit attack against Platonism, as well as his calling "naive" to certain intuitions about ST which Tarski did not share. Thus, although Gödel and Tarski may have shared that no form of reasonable intuition may lead us to so large cardinals, it is clear to me that for Gödel mathematical intuition was much more powerful and reasonable than for Tarski, who saw set theoretical "intuition" as rather unreliable a tool. So I think we can accept as a result that Tarski distinguished between naive intuition, which for him must have meant arbitrary, artificial, Platonistic

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influence of Cantor and Gödel. As we are seeing in the main text, things are much more convoluted from the philosophical viewpoint.

intuition, and “cogent” intuition, which I take to mean solid, reliable intuition, somehow related to the physical world.<sup>24</sup>

However, if we had available just the above evidence, our understanding of Tarski's intuitive notion of set would still be rather incomplete. Fortunately, there are two more pieces of unpublished evidence, which can be found in the Tarski papers: Tarski's contributions to two meetings which took place in 1965, in Chicago and London respectively. As we are going to see, both contributions add important insights to his formerly known opinions on ST. In the first case, there was no proceedings published, but Tarski did not do anything to publish the transcription of his contribution, or any other ideas in a similar vein. In the second case, it seems he did not allow the organizers to publish his contribution. Let us see what we can find of interest there.

## 6 The contribution to the Chicago meeting

Tarski's contribution is the transcription of a tape containing what Tarski said in Chicago in 1965, at a joint meeting of the ASL and the APS (April 28-9, Marcus chairman), and in particular in a Symposium about the philosophical implications of Gödel's incompleteness theorems (Tarski chairman). The invited speakers to that symposium were Montague, Putnam and Benacerraf. Montague gave a brilliant, yet difficult talk about some sort of generalization of Gödel's incompleteness results; Benacerraf presented a precursor of his “God, the Devil and Gödel”, published in 1967, then Putnam probably read a precursor of his celebrated paper “Mathematics without foundations”, published in 1967.

In the transcription, Tarski is referring to those three talks, but he only mentions Putnam's name, and devotes most of his contribution to comments on Putnam's talk, although he obviously takes advantage to expose his general philosophical position, probably for the first time in public. He says to be an extreme anti-Platonist, a nominalist, a materialist, and clearly rejecting all *a priori* knowledge, from his empiricist and physicalist tendencies. That must have sounded as a bomb to many in the audience, who did not know anything about Tarski's actual philosophical tendencies, which were clearly strange given Tarski's very well-known works in ST, metamathematics, semantics, algebra, and many other parts of “high” mathematics. Since I am planning to study Tarski's general, philosophical ideas in a further paper, and since this contribution, together with the one to the London meeting (see below), will eventually be published, I will limit myself here to analyze the materials dealing directly with ST.<sup>25</sup>

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<sup>24</sup>In 1970 Tarski mentioned to Mycielski “the Platonic belief of Gödel that sets can be seen (seen, not imagined) in our minds almost like physical objects”, and that this belief “is bewildering” ([45], p. 217). It is difficult to find a most clear rejection of classic mathematical intuition, so I believe this should be taken as a further confirmation that for Tarski naive intuition was not reliable at all.

<sup>25</sup>Thanks to Paul Benacerraf I was able to somehow reconstruct the information about the talks actually given in that meeting. Unfortunately, Hilary Putnam told me he does not re-

In the text there are just two passages where Tarski seems to be reacting to Cohen's 1963 results, which were very recent at that time. In the first one, and after mentioning Putnam and the possible discussion between a Platonist and an anti-Platonist, he goes on to say that the anti-Platonist referred to in Putnam's talk "was of a very mild genre", and that he is "a much more extreme anti-Platonist". Then we find this:

So, you see, I am much more extreme; I would not accept the challenge of Platonism. You agree that continuum hypothesis has good sense, it is understandable. No, I would say, it's not understandable to me at all. I would not be able to use like your anti-Platonist the terms "can", "be able", in such a way that it has no time coefficient. Now, one could think that from this point of view the whole problem vanishes. If one doesn't understand, then why bother when certain strange phenomena appear, strange things happen. Well, it doesn't vanish entirely; it takes another aspect. You think why other people are so impressed by it, why other people use it, why is it so that this Platonic way of thinking found such a firm ground in our society, in our culture, seems to have been very useful and very helpful in the development, in the progress achieved.

First of all, when he says that CH is not *understandable* to him I think we can interpret him as meaning that his intuitions about the notion of set, his physical intuition, did not support an hypothesis so far away from what we know and believe about the physical objects, the actual sets of objects. That would be consistent with the former uses of "intuition" that we saw above when dealing with the large cardinals issue: intuition must have meant to him something similar to visualization, to our capability to imagine something in a similar way to which we imagine something physical. That is why we called it physical intuition. Therefore, Tarski seems to me to be thinking of his well-known ultimate indistinction between logical and factual truths, between mathematics and physics.

But what about the "time coefficient" puzzling expression? When I read parts of Tarski's contribution in a seminar in Berkeley,<sup>26</sup> Henkin told me that the reference to a time coefficient was not to be understood in the intuitionistic sense, but just referring to something which to discover a yes or no response is a matter of time. This could mean that for Tarski talk about CH in general, abstract sense, had no meaning at all unless it is inserted in a particular historical time, then related to the state of the research on CH in that moment. And this will more or less coincide with my former interpretation about understandability of an abstract, mathematical hypothesis rather in a proof-theoretic sense (see

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member anything about that forty years ago event. It seems that nobody replied to Tarski's shocking remarks, which is not strange; as Benacerraf said to me in correspondence, although he was taken aback by what he heard, and of course he did not agree with it, Tarski was among the gods, and no one replies to a god! Tarski's philosophical position was so unexpected that Benacerraf even "forgot" it when he wrote "Mathematical truth", published in 1973, where Tarski's position is taken to be quite the opposite to the one made public in the meeting.

<sup>26</sup>In June 1996, Mathematics Department, with John Addison, Charles Chihara and Leon Henkin. That session was not only very insightful for me, but also a big pleasure, as it took place in the same room, and chairs, where Tarski used to meet his colleagues and collaborators when he was in his department.

above). So the “yes or no” issue by Henkin is not to mean anything about “constructibility”, but just about the insertion of an hypothesis in a more general, scientific context, which may well be historically changed.

To my knowledge, the expression “time coefficient” appears just one more time in Tarski's writings: in his 1944 divulgative paper on the semantic definition of truth. There he says that acceptability of an empirical theory or hypothesis must be relativized “to a given stage of the development of a science”: “In other words, we may consider it as provided with a time coefficient: for a theory which is acceptable today may become untenable tomorrow as a result of new scientific discoveries” ([61], §21).<sup>27</sup>

So the last part of the quotation seems now to be clearer. Even when we declare CH to be relative to a more general body of global knowledge, the problem does not vanish “entirely” but it takes another aspect, i.e. it goes far beyond a question of pure mathematical provability. Once we insert the problem of the understandability of CH in a historical context, where mathematics is closely related to other scientific theories, and then is treated as an empirical science, in the sense that its hypotheses have to be evaluated in a more general context than pure mathematical provability, as for instance fruitfulness of consequences, and useful relations to our empirical knowledge of the world, then the Platonistic tendency shows its only hope of an actual justification: pragmatical justification. So Platonism, if accepted in some ways, should be considered as an heuristic tool, because it helps us to expand our view of the world, as long as we regard it as just a working tool, not a body of beliefs in a transcendent world. Thus, the entities which we arrive at by using that tool should be calibrated with our usual, pragmatic, relative standards, always closely connected to the empirical, the physical only actual world.<sup>28</sup> This seems to me to be compatible with Corcoran's proposal of a Tarskian “Platonistic reductionism”.

As a matter of fact, Tarski himself admitted, in conversation with Chihara, that he accepted that for Gödel his results were important for philosophical reasons, and that a nominalist could hardly have discovered them (Chihara 1996, personal communication). So it is clear that Tarski accepted that Platonism could be used as an heuristic tool to reach certain results which are *prima facie* very far beyond our physical intuition. Yet this could also be regarded as a rather puzzling assertion: did Tarski mean that he could not have reached Gödel's results by himself, in being a nominalist? Hardly. First because he was actually convinced that he was very close to have reached those results himself, and that with more

<sup>27</sup>Also, Frost-Arnold suggests in correspondence the following, just as a conjecture: “The ‘time coefficient’ stuff may be related to the 1940-41 conversations. For there, Tarski says a finitist-nominalist must get rid of ‘x is derivable/ provable’, because it is an irreducibly modal notion, and replace it with ‘x is derived/ proved’. But at any given time, only a finite number of theorems will ever be proved/ derived, and what is proved will always be relative to a given time”. This is obviously food for thought, yet I still like my own interpretation best, mostly because it is based on textual evidence.

<sup>28</sup>Including the mental, since for Tarski, following Kotarbinski, the psychical should be somehow reduced to the physical.

time for research this could probably have been done. And, most interestingly, because as a working mathematician he was a methodological Platonist, no matter he may have thought that ultimately his abstract entities could somehow be reduced to physical ones.<sup>29</sup>

The second passage can be found at the very end of the contribution, i.e. after Tarski already gave his explanation about the probable historical origin of logical truths (so expanding what he wrote in the 1944 letter to White, published in [67]), and after his assertion that the laws of logic (especially the law of excluded middle), which were born to deal with physical objects, were then wrongly applied to abstract notions. Then he adds:

It seems now pretty certain [than? then?] there are expressions in set theory which we consider sentences -this means expression of truth or false- and regarding which we shall never have (it seems likely at least) any possibility of deciding whether they are true or false. And the natural question arises whether it still makes sense to stick at [to?] this logic, old logic, when dealing with this type of sentences. So in this sense I would not go as far as you -than in mathematics really truth is only provability. I don't know which provability, but at any rate maybe the notion of truth is simply not proper, this classical notion of truth, for mathematical sentences. I think that this is the negative conclusion which one could draw from these developments starting with Gödel, and therefore I am quite interested in attempts of [at?] constructing set theory on the basis of some non-classical logics, simply as an experiment. We shall see to what it will lead.

First he mentions sentences which we could call absolutely undecidable, by following the terminology he used in his letter to Gödel of 1946 (see above). In the context of the contribution this was probably done by thinking of CH, especially by taking into consideration the former quotation and Cohen's famous results from 1963 about the independence of CH from ZFC.

Second, since the quotation immediately follows his claim that the law of excluded middle was born in physical, historical contexts and was then wrongly applied to abstract notions, his question about applicability of the old logic may be referred to classical, bivalent logic, the logic with just two truth values. So, when he finishes by asking for a formalization of ST in a non-classical logic he might have been thinking of some system of three valued logic, as the ones used by some of his teachers in Poland (mostly Lukasiewicz). And this is interesting, for it allows us to relate the issue again to his well-known attack to the factual-logical distinction, as applied to the truth of sentences in his 1940-1 conversations with Carnap, and also to his well-known reference, in his 1944 letter to White, that logical laws are not different in nature from empirical truths, but only in degree. They are just more general and also historically older, so some empirical, fundamental experiences may make us to be inclined to change them (and he mentions quantum mechanics).<sup>30</sup>

<sup>29</sup>For details about Gödel's view of Platonism as an heuristic tool, see my [50]. Also, it contains a comparison between Tarski, Gödel, and others, concerning the similarities between mathematics and physics.

<sup>30</sup>It is also possible that Tarski was thinking of some form of intuitionistic logic, perhaps in



Finally, the issue that truth is not just provability. As this is apparently referred to Putnam's talk, I looked for any such assertion in Putnam's actual publication of "Mathematics without foundations", and it is not to be found there, though the paper as published might be different from the paper actually read by Putnam, and even the transcription of the tape might not be totally fair here to what Tarski actually said.

Anyway, Tarski was probably inserting Putnam's general talk in the classical context that in mathematics the only criterion to be used for the working mathematician is provability, including of course Tarski himself (*pace* Gödel's incompleteness and undecidability results). But even if it was totally clear what provability should be (which seems not to be clear for Tarski as per Putnam's actual talk, were modal notions are used, which Tarski was not very fond of), then provability is not enough, in the sense that the classical notion of truth, the bivalent truth, is supposed to underlay our results when applying the standard provability methods. And as we have seen for Tarski the law of excluded middle is rather suspicious when applied to hypothesis like CH, which are very far beyond our physical intuition. Thus, when he writes that his negative conclusion about classical truth started with Gödel's results he must have been thinking, not only in the obvious impossibility of continuing to identify truth with provability after them, but mainly in the similarity between logico-mathematical truth and factual truth that was so dear to him. Therefore, the analysis of the second quotation lead us to very similar conclusions that the ones reached with the first quotation.

## 7 The London meeting

About three months after the Chicago meeting, Tarski participated in the 1965 International Colloquium in the Philosophy of Science, held at Bedford College, London, from July 11 to 17. One of the main sections of that meeting was devoted to Problems in the Philosophy of Mathematics, whose proceedings were published in 1967, edited by Lakatos ([38]). Due to the spectacular results by Cohen in 1963, two sessions were devoted to the recent results in ST, with talks by Mostowski and Bernays, followed by rather hot discussions, especially after the first one, where Tarski was the chairman. Also, Kreisel gave his famous talk "Informal rigour and completeness proofs", and heavily contributed to the discussions, especially attacking Mostowski's presentations and ideas.<sup>31</sup>

The gist of the context of the discussions, in connection with Tarski's contribution, seems to me to be this. Kreisel defended the role of second order logic in formalization, and pointed out that there is an asymmetry between the inde-

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order to dispense with the law of excluded middle, as in his writings there are a few places where he pointed out some intuitionistic flavor of his work, but for him intuitionism limited classical mathematics in an intolerable way, so this possibility seems rather unlikely.

<sup>31</sup>Mostowski was a former student of Tarski in Warsaw, under whom he wrote his 1939 Ph. D. dissertation, although the official supervisor was Kuratowski.

pendence of CH and that of the parallel postulate in geometry, for the latter is also independent in second order geometry and the first is not: CH is decided in second order ZFC.<sup>32</sup> Also, he attacked Mostowski's saying that the notion of set is vague, and the "old-fashioned" belief, either realist or idealist, that what the mathematician does is analyzing intuitive notions then establishing their properties. On the contrary, he defended "informal rigour" and the need to introduce new, stronger axioms to decide important problems, by using second order logic when needed.<sup>33</sup>

Second order logic was criticized by Kalmar and Mostowski. The latter admitted that CH is decided in second order ZFC, but added that this is useless as no one knows how it is actually decided. Also, he said that second order logic is a part of ST, so trying to develop both at the same time makes no much sense. Finally, Mostowski insisted that the present situation of ST is problematic, and that it will probably lead to a bifurcation into different set theories (by which he must have been referring, for instance, to Cantorian and non-Cantorian set theories, i.e. theories in which CH is added to the usual axioms and theories in which it is not, or to similar bifurcations as a way to absorb the recent, controversial results in ST).

Tarski made a contribution, probably after Mostowski's talk, in which he explicitly mentions the discussion between Kreisel and Mostowski. His contribution was not published in the proceedings, but a transcription, probably from a recorded tape, is in the Tarski papers in Bancroft Library. I suppose he did not allow the organizers to publish his contribution, but the reason is unknown, although he was probably not satisfied with it, perhaps because it showed too much of his true philosophical tendencies (if so, this may well be a further sign of a "conviction and caution" attitude). The following is an attempt to analyze its content, but just in what is relevant to his notion of set.<sup>34</sup> This may well be the darkest scenery about ST described by Tarski, with the possible exception of the Bucharest talk in 1971 (see below). In the contribution, Tarski discussed two main questions: second order formalization and the present state of ST, to which the following two sections are respectively devoted.

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<sup>32</sup>Kreisel apparently did not know that Zermelo pointed this fact out already in 1930; see Moore in Gödel [23], p 157, fn.

<sup>33</sup>Kreisel's global position shows a clear influence of Gödel on the need to introduce stronger axioms to try to decide CH and similar problems, as well as about the asymmetry between the independence of CH and that of the parallel postulate; see Gödel 1986, p. 267. In addition, Kreisel's contributions have a clear intuitionistic flavor, especially when he criticizes the belief in a previous, intuitive notion of set, independent of any formal description of it. In this connection Kreisel's position seems to me very close to Dummett's well-known arguments that there is no model of mathematical entities, e.g. of the notion of set, apart from the precise, theoretical description of it; see Dummett [8].

<sup>34</sup>Solomon Feferman and Ignacio Jané helped me dating the transcription. Jané finally suggested the London 1965 meeting, then everything was on the right place. This contribution will eventually be published, together with the contribution to the Chicago meeting.

## 8 Second order formalization

Tarski referred to it as “the use of the notion of set in the second order formalization of an arbitrary theory”.<sup>35</sup> He starts by pointing out that first order formalization is very useful to formalize a large part of mathematics, and that in doing so it “has gained a very wide range of agreement, but not an absolute one” (p. 2). Then he mentions certain objections as having been formulated (presumably by Kreisel in his well-known paper read there), and he illustrates the objections with the example of first order formalization of natural numbers theory in the usual way: the axioms, followed by formalizations of proof, theorem, etc. Then the objection may be raised that in formalizing the notion of proof “you will again use notions such as ‘set’ whose exact meaning is by no means clear to you” (p. 2), which is not an elementary notion. Then he said:

Now I would answer that this use of non-elementary notions is not necessary in formalization. I have often stressed that it is not necessary in formalizing (if by that one means normal first order formalization) to define what is a formula, what is a proof, what is a theorem. Formalization may consist in a series of autobiographical statements. Of course the statements must include such sentences as: “I recognize the following statement as true, as an axiom, and I don’t want to analyze it” (and I write down the axiom). Then I say, “I shall also recognize as true any sentence  $b$  under the condition that I have previously recognized as true two sentences, one of which is  $a$  and the second of which is  $a \supset b$ .” And this is enough. You see that in this formalization I do not actually use any such theoretical term as set; and I do not need a definition of a sentence or a formula. I have said quite clearly what I am going to do in developing arithmetic, and this procedure is quite satisfactory for various purposes.

Several things are remarkable here. First, the notion of set is admitted to be a non-elementary notion, so requiring a theory, ST, to give it a precise meaning, and perhaps to transform the notion into a clear concept. Second, the mixture of formalization and axiomatization seems to me rather unclear. If you just offer a list of autobiographical statements which you postulate as true, this is just an axiomatization, though of course you can also formalize those statements later, for instance by using first order logic.

But of course, he goes on, the use of “theoretical terms” from ST is unavoidable in formalizing other theories, as can be seen by trying to state the simplest example: the rule of induction. And the same could happen with a second order theory:

Now this is so in this case, and if in this case you still admit that the formalization by these means of the rule of induction clarifies to some extent a certain theory, then I think we should also recognize the fact that *other* set theoretical devices may be useful in formalization: and second order logic is just such a device.

In view of this passage, Tarski’s acceptance of second order logic seems clear. However, his use of the logic of PM in many of his works, which involves

<sup>35</sup>This is interesting because Tarski is using the word “notion”, presumably as opposed to “concept”, and it is well-known that he considered “notion” to be hopelessly vague.

higher order logic in that it uses variables of many different types (not just individual variables), was already well-known. So the question is: why is Tarski saying so? It seems to me that in the context of the discussion between Kreisel and Mostowski he was inclined to share Mostowski's rather suspicious attitude before second order logic, as what he says is that second order logic must be regarded as a set theoretical device, and he even emphasized the word "other", to make clear the power of ST as a supplier of theoretical devices. But this is exactly the position showed by Mostowski when he said that second order logic is but a part of ST (see above). Tarski was very polite in the introduction of his contribution: he said to be "close" to Kreisel, but made very clear that he knew that there was no disagreement between Mostowski and himself. What was turning out instead was rather that he did not like second order logic as an instrument on its own.<sup>36</sup>

## 9 The 1965 state of set theory

As for the second question, Tarski seemed to be very interested in pointing out that the unsatisfactory state of ST may be due to the notion of set itself, and to the way we "think" we understand that notion. First he says that in that discussion "we all use terribly loose terms, all of us, and my discussion will not be on any better level, or on any different level, than the discussion of others" (p. 1). Then he goes on to develop this impression like this:

We have heard a lot about the unsatisfactory situation at present in set theory, about the possibility or ramifying set theory, and that in fifty or a hundred years there will be no single set theory, but many set theories, so that set theories will be like groups, for example. Maybe this will all come true, but it will not mean that set theory as such will vanish, because in developing set theory we certainly have in mind some aesthetic model, or rather some rule which singles out a privileged *class* of models, since we do not know exactly what model we have in mind.

For one thing, it may be shocking to many to read that for Tarski the possibility of different set theories, depending of the use of special axioms or hypothesis (like CH for example), may be regarded as unsatisfactory. After all, his pragmatical position in philosophy was close to formalism (and no longer to Lesniewski's "intuitionistic formalism"), and his general philosophical position was a clear nominalism. So maybe he was just showing the optimistic side of his Platonistic reductionism here.

Also, his skepticism about the vanishing of ST in the future, due to the consequences of Cohen's and similar results, is not consistent with what he said to Carnap in the 40's (see above), that it would be a guess and perhaps a wish that "general" ST will disappear in the future. In 1965 part of the future had already

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<sup>36</sup>A further sign that Tarski "came to doubt the foundational significance of higher-order logic" is that his famous 1959 paper entitled "What is elementary geometry" was devoted to the construction of a first order foundation for geometry, so "correcting" former second-order attempts in the 30's (Corcoran [5]).

arrived and we find Tarski defending classical ST with classical arguments: that we have “in mind some aesthetic model” of set!

However, how do we know that we have a model of set in mind? In the context of Tarski's philosophical ideas, this must have been referred to our empirical knowledge of sets, to our physical intuition, as he rejected any a priori knowledge or any mathematical intuition in the Gödelian sense. Thus, to qualify that model as “aesthetic” should be understood in the context of our perception of physical objects, shapes and forms: as something we can somehow visualize or imagine, as we imagine physical objects not present before us.<sup>37</sup>

If this is right, some of the reasons for not allowing the contribution to be published might be emerging here, as Kreisel was precisely strongly criticizing the “old fashioned” conception that we first discover a model in our mind then we make it precise with a mathematical theory, e.g. through a set of axioms. We do not know whether Kreisel did reply to Tarski in that discussion, probably not, but it would have been very easy for him to use the Dummett [8] style arguments against Tarski's old fashioned position: we do not have any access to those models independently of our theories, because we cannot make them precise without those theories. After all, Tarski may well be recognizing this, at least in part, when he closes the passage by speaking, not of a particular model, but of a whole class of them, “since we don't know exactly what model we have in mind”. But this is still worst, as the only way to characterize a class of models is a set of axioms, and axioms are the raw material of deductive theories.

All in all, it seems to me that Tarski was trying to be too cautious in connection with what he probably believed: that we possess an intuitive notion of set which develops in contact with the physical sets of objects laying around us, in our ordinary relation and interaction with the world, and this notion is very different from many other mathematical notions. In this sense physical sets are finite, so a finitistic position was always sympathetic to Tarski, as can be clearly seen in many passages of his conversations with Carnap, as the one quoted above. Otherwise, if we interpret his words as supported by the implicit acceptance of some form of mathematical intuition, in the Gödel style, then Tarski's position would not be very different of that of a literal Platonist, so not the words expected from a Platonistic reductionist. Everyone who worked close to Tarski knew very well that Tarski rejected “classical” mathematical intuition. As Givant wrote to me:

Gödel was in some sense a Platonist. He thought deeply about the Axiom of Choice and the Continuum Hypothesis, and believed the former to be true and the latter to be false. Tarski did not share these “intuitions”. For him it was not evident that the Axiom of Choice was true, any more than it was evident that the Continuum Hypothesis was false, and he did not comprehend Gödel's intuitions. At one point I heard Tarski express some sentiments that might even be interpreted as ultrafinitistic.

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<sup>37</sup>The strange use of “aesthetic” here could perhaps be also related to Tarski's love for classical mathematics, or to his attraction for the beauty of mathematical theories, as ST; see above in the conversations with Carnap section. Yet I still prefer the interpretation in terms of visualization.

Let us continue with Tarski's text. The differential character of the notion of set, as compared with many other mathematical notions, or concepts, is also that we use it in ordinary language, in ordinary life. That is why Tarski insisted that we should not regard ST as any other mathematical theory, because "the fact that we permit ourselves to use ST in non-formalized ways in everyday language shows, I think, that ST is very different from group theory" (p. 4). And the same point was made before, in p. 3:

The fact that we use set theoretical notions in metamathematical or metalogical discussions seems to me very important, because in such discussions we use the word 'set' in a non-formalized way. Thus there is a difference between set theory and other kinds of mathematical theories.

This is perfectly consistent with my above interpretation in terms of physical perception: we do "perceive" sets, but we do not perceive groups, or vectorial spaces, and the like. The historical basis of the most elementary truths of logic, in close contact to everyday life, must have been for Tarski very similar to the case of the historical origins of ST: our empirical relationship with physical reality. That is why we understand the notion of set in a previous manner, without any recourse to mathematical theories. So the way in which we possess a model of set in mind is just a visual way, a spatially imagined way, similar to a physical object, no matter how complicated or convoluted. And this seems to me the only alternative open to us if we do not want to make of Tarski a true Platonist, not just a methodological, a tactical one. (Yet see the last section below for more problems about perceived sets.)

Also, it allows us to better understand the impact of Cohen's, and similar results: in making the "bifurcation" of ST possible, Cohen's results showed that our knowledge of the intuitive notion of set, as reflected in classical ST, is not precise. If we have to allow the ramification of ST, and the appearance of many different set theories, what convincing basis would have we to say that our physical intuition is reflected in this or in that particular ST? Here Tarski might be facing a similar problem that the one which led him to consider higher logics as ultimately artificial, or to say that there is no difference in nature between mathematical theories and physical ones, or to defend that in applying classical logic to abstract entities we are forgetting the historical, empirical origins of logical truths. But if classical ST can be defended just by appealing to physical intuition, the notion of set can no longer be respectable to mathematicians! And this is, I think, the big problem for Tarski, who always wanted to make logic and metamathematics acceptable to mathematicians as full mathematical theories. If bifurcation of ST is confirmed, then the notion of set is hopeless. This leads us to the end of Tarski's contribution, which is really gloomy.

The problem is, according to Tarski, that when ST is used in metamathematical discussion "we rarely ask anybody to explain the set theoretical notions". And this is a problem, because very different set theoretical notions can be used, and some of them may lead us to "deception". Tarski gives an example: someone

might prove the categoricity of a theory by using “a restricted naive set theory” with no inaccessible cardinals, so his proof, in starting from an “artificial assumption” is a kind of deception. Then Tarski finished the contribution this way (pp. 4-5):

For this reason I consider the situation in set theory to be even more unsatisfactory than do other people. I am not consoled when I am told that the time will come when there will be many set theories, many different models, and so on. Recent results in set theory show that there are many set theoretical models for our usual set theoretical axioms. And the problem of whether or not there is one optimal model remains open; it is a problem of fundamental importance.

To be noticed first is Tarski's reference to inaccessible cardinals, whose content does not seem to coincide with what he wrote in the Stanford 1960 paper (Tarski [63]). As we saw above, in that paper Tarski seemed to somehow favor a limitation of the more exuberant extensions of ST to very large cardinals, in particular to some inaccessible cardinals. Also, he seemed to call “naive” ST one which is not restricted in any way, so perfectly able to pursue “Cantor's absolute”. But now he makes clear that such a limitative restriction to eliminate inaccessible cardinals would be an artificial assumption, for it may lead us to unreliable proofs. And worse, he now calls “naive” precisely to the ST resulting from such limitations, while under our former interpretation above naive ST was the totally unrestricted one, which was to be somehow rejected from the viewpoint of Tarski's physical sense of intuition. So we are in trouble.

A first attempt of a solution might go like this. Our sane intuitive notion of set, the one based on physical intuition, is the only one capable to provide us with an ontologically reliable basis for the ultimate reduction of the whole of higher mathematics to something acceptable from a finitistic viewpoint, or at least to an infinite basis which seemed to him somehow reducible to the former. But this physical intuition does not allow us to go very far, as it is essentially limited to finite languages and Tarski had no intention whatsoever to renounce to the beauty of classical mathematics. So if we limit the usual ZFC too much, that would force us to renounce to be able to manage with important theories and proofs and with their metamathematical properties. So while his Platonistic reductionism pushed him to restrict the ontological extension of ST, his needs as a working mathematician pushed him in the other direction. And this dilemma has no solution, as the motto of this paper shows.

But what about “naive” ST? Is this the unrestricted or the restricted one? Well, Tarski might be calling naive to any ST whose axioms are somehow determined by a priori beliefs. Belief in the full Cantor paradise is an a priori belief, but any restriction of the usual extensions of ZFC to very large cardinals, without any basis in physical intuition, could be a priori as well, even when it might favor the strong limitations dictated by our intuition of physical sets, in the sense that any restriction whose resulting entities are not based on, and justified by, a well-known process of reduction might be arbitrary then artificial. Unless we accept something like this, the only other alternative would involve to limit the

range of the Cantor absolute, and this, again, would be contrary to the distinction between naive intuition and physical intuition.

Of course, some inconsistency on Tarski's language is always possible, especially because he was no professional philosopher nor was he very concerned about developing his philosophical options in much detail. But he was obsessed about the use of a very precise terminology, so I think he was aware of his former uses of the word. In addition, I tend to believe that his philosophical inclinations, though not very sophisticated, were tremendously permanent and invariant (even under every permutation of the world onto itself!).

Be that as it may, the end of the contribution is totally clear: the reason of Tarski's judgment about the unsatisfactory situation in ST was not only based on Cohen's results, which are not even explicitly mentioned, and on the ramifications which might result from them. (As a matter of fact he was convinced about the absolute indecidability of CH in ZFC since at least 1946; see above.). But also on the problem of the legitimate extensions of ZFC: in the question whether or not we are to allow the addition of strong axioms, or of limitative ones, to the usual axioms of ST. So when he talked about the problem of finding the "optimal" model as a fundamental problem he was totally serious. The optimal model would be the one which, while providing us with a ST capable to be consistent with our physical intuitions of sets, it would nevertheless be clear enough to decide between too large and too short extensions of ST without resorting to a priori, or theological, options. Therefore, the main problem remains open: where to look for that optimal model?

When I sent the text of the London contribution to Solomon Feferman he wrote to me: "These remarks indicate the extent to which Tarski accepted set theory and 'core' set theory (e.g. ZFC) as expressing a definite concept". I fully agree, but to what extent those remarks show Tarski believing in such a definite concept? I think he believed that ST, as we know it, is hopeless to grasp such a definite concept. But this was no problem for him, as he did not believe in the existence of concepts at all! He was a nominalist, so he believed that in contact with our physical intuition of sets we develop just set theoretical notions, but that unfortunately those notions are not enough to develop higher mathematics without using Platonistic entities. And this tension has, again, no solution. Through the independence results, and the problem whether or not ZFC should be extended with no restriction, it seems to me that for Tarski the standard axioms of ST show some sort of essential indeterminacy, or relativity (to use Skolem's word): they are not the sort of thing capable to transform our intuition of sets into something precise, let alone into a Platonic concept, so our intuitive notion of set is to be kept essentially indeterminate.



## 10 Are set theoretical notions logical?

In his semantical and mathematical practice of the 30's Tarski applied a rather broad sense of logicity, where many notions not *prima facie* belonging to logic were described as exhibiting a "general logical character" ([64], p. 170). Among them he clearly included set theoretical notions, particularly membership, and even some from the domain of cardinal arithmetic (p. 171). This was justified this way: "The calculus of classes is a fragment of mathematical logic and can be regarded as one of the interpretations of a formal science which is commonly called *Boolean algebra* or *the algebra of logic*" (p. 168). Yet as we have seen in former sections of this paper, this procedure involved the standard use of a simplified type theory, and although type theory offered mathematical universality, it made true domain universality impossible, so type theory was finally abandoned in favor of standard ST, at least for the definitions of the most fundamental semantical notions. Thus, in dispensing with type theory, the general logical character of set theoretical notions was no longer clear.

In the famous 1944 letter to White, an ambiguous attitude about the issue is apparent: "sometimes it seems to me convenient to include mathematical terms, like the  $\in$ -relation, in the class of logical ones, and sometimes I prefer to restrict myself to terms of 'elementary logic'. Is any problem involved here?" ([67], p. 29). But it was not until 1966 that he returned to the issue in a more detailed way.

In that year Tarski gave a lecture in London, entitled "What are logical notions?", which was later edited by John Corcoran and appeared published in 1986 ([66]). The text is now well-known and have been deeply discussed, but it contains something very relevant to Tarski's viewpoint about set theoretical notions, so at least some brief discussion here is mandatory. I am talking about Tarski's question whether or not set theoretical notions, i.e. mostly the membership relation, are logical notions. The question is very important because the language of ST is mathematically universal, so capable of expressing the whole of mathematics, thus the real question is whether or not mathematical notions are logical.

According to Tarski's definition, logical notions are those invariant under all possible one-one transformations of the world (the universe of discourse) onto itself. Tarski's response to the question is ambiguous. If you use PM type theory, a clearly higher-order logical theory, membership is a logical notion, so ST, then mathematics, is a part of logic. The PM fundamental universe contains all individuals, "which we may interpret as the universe of physical objects" ([66], p. 152). Yet type theory requires that in addition to the most basic universe of discourse, that of individuals, you have to construct further domains containing classes, relations, and the like. On the contrary, if you dispense with the hierarchy of types and regard membership as an undefined, primitive relation between individuals, so that you only have a unique universe of discourse, then membership is not a logical notion (it is not a logical relation between individuals), so first order ST, then mathematics, is not a part of logic.

To finish, Tarski associates each method to a different type of mind: philosophers would favor the first one, characterized by a monistic conception of logic, ST and mathematics, while mathematicians would prefer the second, where mathematical notions and theories have an independent ontological status on their own. However, Tarski does not tell us which method he liked best at bottom (“As you wish!”), exactly as he already did in the letter to White, where he sees no problem in either possibility. So we can speculate in view of our former discussions, just to see whether or not some option would be more coherent with which we have learnt from the other evidences above.

A further hint is contained in Tarski’s contribution to a meeting in 1955, where he made some comments about Skolem’s relativism about set theoretical notions (see Bellotti [2], p. 406). The important point is contained here (I quote after Bellotti):

The Löwenheim-Skolem theorem itself is not true for a certain particular interpretation of the symbols. In particular, if we interpret the symbol ‘ $\in$ ’ of a formalized theory of sets as a dyadic predicate analogue to any other predicate, then the Löwenheim-Skolem theorem can be applied. *But if instead we treat ‘ $\in$ ’ like the logical symbols (quantifiers, etc.), and we interpret it as meaning membership, we will not have, in general, a denumerable model.*

I take this to mean that when ST is constructed the second way, the axiomatic one, LS holds, but when we use, for instance, PM type theory, then membership becomes a logical notion, and LS does not hold. We should not forget that type theory uses a higher-order logic, and in second-order logic LS does not hold. Thus, if we regard ST as a part of logic it is released of the relativity problems intrinsic to axiomatic ST on its own. Bellotti takes Tarski’s words to involve that he was finally decided to see membership on a par with the usual logical notions. I do not think this is right, since as we have seen in the first section above Tarski dispensed with type theory to complete his semantical program, but it could anyway be added to the things we need to consider to try to answer the question: which one of the two above methods should be chosen?

As we have seen in the first section above, if type theory is chosen, which is what the historical Tarski used to do in some of his writings, ST is a part of logic, but although we do have mathematical universality, we do not have domain universality (we do not have a single universe of discourse). Also, the fact that the basic domain, that of individuals, can be identified with the physical world, is a further advantage, as it fits very well Tarski’s physical intuition. Finally, second order logic (and higher ones) is inevitably involved, so the former limitation of LS is just a partial one, as LS does not hold in higher order logics. And we must remember that Tarski did not like second order logic, and probably shared Mostowski’s conception that second order logic is a part of ST! (see above).

On the other hand, if ST is not logical we still have mathematical universality, and also true domain universality, but individuals, as well as sets, belong to the same, single universe of discourse, since in first order ST sets can be individuals. Thus, physical intuition, acting mostly on physical objects, works just in an

analogical way with sets of them, not in a literal way, as sets are only “individuals” in a metaphoric sense. Finally, the usual ST is a first order theory, and this is good, but then LS applies, with the well-known inconvenient consequences.<sup>38</sup>

Therefore, no option is satisfactory, as we do not get mathematical universality, domain universality, ontological credibility, clear intuitive access, and lack of indeterminacy at the same time, in any of them. I think that is why Tarski did not make clear which one was his own option: we may well be before another example of a tension, a philosophical tension, with no solution. After all, he was no longer very interested in ST and preferred, by far, a sort of algebra powerful enough to do the same job without any of the usual set theoretic problems, including indeterminacy.

In 1971 Tarski gave a talk in Bucharest, which had a rather philosophical content related to his conception of ST and its present state. In the talk Tarski insisted on the 1965 gloomy lines, but that time there was no tape recording what he said, and to my knowledge no transcription was made, so very little is known for sure. Givant did not attend the talk, but he spoke to some of the people who listened to Tarski there, then wrote this to me: “I believe people who heard Tarski's talk found it profoundly pessimistic. I would not say that his point of view was shared by many philosophers and logicians, though I would guess that it might have the tacit support of many mathematicians”. So, his initial hopes that mathematicians accepted foundational endeavors as being fully mathematical seemed to end in a failure, at least to the extent that those endeavors were somehow based on ST.<sup>39</sup>

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<sup>38</sup>As it was mentioned above in the main text, in order to avoid the applicability of LS then of Skolem's relativism to the usual axioms of ST, Zermelo published a second order axiomatization of them in 1930, but obviously this could not offer the goods which Tarski wanted.

<sup>39</sup>This seems to somehow coincide with what James T. Smith, who actually attended that talk, has written to me:

Detail on Tarski's gloomy Bucharest talk. I don't remember very much, but here goes. The most spectacular results since 1963 had been in metamathematics of set theory. That was generally the buzz in the corridors. (I do remember another buzz having to do with automata theory -Lindenmeyer systems, etc.- but that might have been just in the crew I was hanging out with.) Tarski spoke about the set theory results, and concluded that however spectacular and amazing they are, they may never lead to adequate answers about the foundations of mathematics. These are my words not his. I mean foundations in the sense of fundamental principles. I don't think he listed his questions, which would have revealed some of his philosophy. I think he was saying, you people won't be satisfied by these results, not that these results won't be answering his questions. I think he did not say anything like “these questions are meaningless” -that would have been less gloomy, for we'd have been freed of the problem. I think he said something to the effect that the questions -whatever they are- would have to be reformulated to admit satisfying answers.

Thus, if no results in ST would never lead us to the right answers to the questions involved in the foundational task, he might have felt that he wasted a lot of time and effort for nothing.

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