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PALEONTOLOGICAL SYSTEMATICS IN THE 21ST CENTURY: WE NEED MORE SPECIALISTS AND MORE DATA

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As an invertebrate paleontological systematist in the 21st century, I have found that A) I have few other colleagues, and B) those that I have are generally at least a decade older than me. Why is this? Museums and universities worldwide, as we all know, are not replacing systematists as they retire; thus, the perception that there is no job market for the systematist is probably real, in part. And it is certainly true that mathematical modeling of nature, statistical evaluation of data, and phylogenetic methods are seen by many as the "best" or "true" means of yielding data about paleontological problems, so that many younger paleontologists work on those subjects.

But another important factor is the unfortunate way systematics, at least in invertebrate paleontology, is discussed by others in the profession. I hear derogatory remarks about it every year, either directly or via our students, at the GSA (yes, even from my fellow professionals). This certainly does not inspire young professionals to enter a career in systematic paleontology.

I argue that the activities and contributions of systematic paleontologists have been unfairly deprecated by our fellow scientists. We are not "stamp collectors." We do not name taxa just to see our own name in print at the end of a species or generic name. We are in the business of cataloging the biological and paleontological diversity of our planet, and without us, evolution, extinction, biogeography, diversity, disparity, and other intensely interesting biological issues could never be addressed. One needs to know who the players are and how many of them are playing to be able to evaluate patterns in the natural world. Users of any type of database, the *Treatise*, the Sepkoski curves or datasets, etc. need to recognize that systematists have given them their dataset.

Systematists working today do not merely name taxa. Nearly all of us, in our papers describing new taxa, incorporate new findings about antiquity of lineages, biogeographic patterns, evolutionary radiations, or other non-taxonomic observations. The suggestion that I have personally heard, that the systematist cannot add to "paleobiological" studies, is biased, short-sighted, and in fact, wrong. It can be argued that in fact, working with specimens is a much better approximation of the paleontological world than is mathematical modeling. After all, the specimens are real. In my area of specialty, when fossil decapods are collected, we collect *all* of them that we see because they are so rare. The collecting bias is probably against those that are small or badly eroded, although I might add that years of working with 2-10 mm Jurassic crabs have made us quite good at finding the small specimens. Thus, we are generating as accurate a picture as human collectors possibly can of the decapod diversity in any given collecting locality, given the vagaries of fossilization. And the bonus? It's fun. We work with actual specimens, work outdoors in outstanding localities, take students to these fabulous places, and teach them about real specimens with actual, preserved anatomy.

In an interesting commentary in *The Palaeontological Association Newsletter* Number 67, it was noted that the assumptions used for generating divergence times based upon molecular clocks and the statistical reconstruction of ancestral charac-

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ters in paleobiological studies are exactly contradictory (Jenner 2008). Thus, there is a critical need to study actual fossil specimens.

Why do we need systematists? Interestingly, the biological world has long appreciated the need for highly trained, descriptive taxonomists, although this may not be expressed administratively in new hires. Biologists even have a name for the increasing lack of qualified systematists in various fields—"the taxonomic impediment."(Brooks and Hoberg 2001). Biologists in fields ranging from parasitology (Brooks and Hoberg 2001), botany (Landrum 2001), to zoology (Lee 2000; Schram 2004) all recognize the value of trained systematists.

The esteemed E. O. Wilson has called for the description of the world's flora and fauna—and that must be done by systematists (*in* Landrum 2001). Feldmann and Manning (1992), crustacean paleontologists and biologists respectively, addressed this issue in one of paleontology's flagship journals. Even our major funding agency, the NSF, has extensive programs recognizing the need to describe, catalog, and derive phylogenetic hypotheses for the world's biota through their AToL (Assembling the Tree of Life), PEET (Partnerships for Enhancing Expertise in Taxonomy), and BSI (Biological Surveys and Inventories) programs (Schram 2004).

However, although Adrain and Westrop (2003) called for additional funding for such initiatives within the Earth Sciences at NSF, I have seen surprisingly little support for such programs. Recent proposals, such as the Future Research Directions in Paleontology, call for the establishment of research "observatories" and the like (Bottjer 2006). Although often mentioned in passing, missing from these proposals is a clear statement of the importance of training systematists to be experts in certain groups of organisms.

Why do new species of extinct invertebrates need to be described—isn't the Sepkoski Curve good enough as it is? Aren't we just filling in details? Disdain for the description of new taxa fundamentally presumes that there are no new patterns in invertebrate paleontology left to be found. Not so. For example, it clearly can be demonstrated that previous hypotheses about the evolutionary radiations of the decapods are not supportable, based upon the description of new taxa. A major radiation within the group certainly occurred during the Eocene (Glaessner 1960, 1969), but it was not the only one: others occurred during the Jurassic, Late Cretaceous, and Miocene (Schweitzer et al. 2002; Schweitzer and Feldmann 2005, 2008). Systematic evaluation of museum collections and newly discovered specimens has elucidated this pattern. Without the work of systematists within the last two decades, this would not, could not, be known.

A major argument that has been suggested to me is that in traditional descriptive systematics (i.e., not cladistics), hypotheses are not tested. This is simply not true. When a new fossil is found, it is examined and compared to other taxa from the area or others to which it is similar. The working hypotheses are: A) that it is a new taxon, and B) that it belongs to an already named taxon and the systematist simply needs to determine which one. The systematist, through careful observation, description, and comparison, determines which hypothesis is supported by the preponderance of the evidence at hand, and hopefully, beyond a reasonable doubt, if a new taxon is to be named. The hypothesis that a new species has been found is tested repeatedly over time, as new fossils are recovered and compared with holotype and paratype specimens. This is what leads to synonymies in the literature.

Rancor between systematists and other paleontologists is unproductive, and probably counterproductive, to the advancement of paleontological studies. We paleontologists comprise a small group, and there are a small number of jobs, fossils, and areas in which to work. Such animosity only serves to hinder constructive collaborations that could yield paleontological products much greater than the sum of the systematic, mathematical, or other parts. There is a genuine need to collect, identify, and describe new taxa in all groups as well as to analyze the resulting data in mathematical, reproducible ways. Non-systematists who attempt to unravel evolutionary patterns, extinction patterns, ecological trends, and biogeographic patterns without the collaboration of knowledgeable systematists do so at their peril. This is also a relevant time to point out that museums, and those that curate their collections (often systematists), are great repositories of information, and that new species and new discoveries are lying in wait to be discovered in these collections (see Allmon 2005).

It is notable that many paleontologists are attracted to paleontology as an area of study based upon fantastic specimens and the prospect of field work in exotic places. Systematic paleontology fills the bill on both fronts. And in the public arena, people come to museums to see …. Specimens! Of course, they'd also like to know about interesting

research that's going with those specimens, using their tax dollars. But they want to see new species, new occurrences, the oldest bird, the newest dinosaur, the biggest clam, the weirdest snail.

I have noticed over time that vertebrate paleontologists do not seem to experience the kind of antagonism that I have heard directed at invertebrate systematists. Indeed, the naming of a new species of dinosaur, hominid, early fish or amphibian, or Jurassic bird is met with much media attention and plaudits rather than a disdainful, "Well, it's just another new species," the type of comment I often hear directed at invertebrates.

It is perhaps true that invertebrate systematists have not done a sufficiently good job in educating the public and our colleagues in what we do. Recently, my university sent out a media pitch about a new species of Jurassic crab we described from Romania. It got picked up by a local newspaper and some web news sites. This illustrates a good way for invertebrate systematists to get the word out. The public likes to know about new and different science stories, and they like to know about animals, plants, and new organisms in general. We invertebrate systematists can help to turn the tide by publicizing our findings within our institutions and working with our media representatives to get the story out. It can, and does, work.

Within invertebrate paleontology, we might add to new proposals for future research directions an initiative entitled something like "What's New? Foundations for the Future." This could be composed of medium-sized grants (think $50,000 - $500,000) for field- and museum-based work to collect, describe, and interpret new biota. More importantly, such grants would fund master's and Ph.D. students and train the next generation of systematists by funding their work in the field with their major professor as well as their university work. This is critical to such important initiatives as the Paleobiology Database to which I am a contributor, which is of course composed of 100% such taxonomic and field data, and it complements all of the initiatives already outlined in the existing future directions report. Nothing could be more critical to advancing paleontological studies in the 21st century than preserving and transmitting the taxonomic knowledge currently held within our working systematists.

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