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SYSTEMATIC PALEONTOLOGY

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WITH THIS issue we mark publication of the 75th volume of the *Journal of Paleontology*, and celebrate the occasion with a series of review articles on the systematics of major groups of fossils. Instructions to authors were very broad: we suggested consideration of the history of study, current problems, and future directions, but otherwise left authors to focus their reviews as they saw fit. We hoped in this way, with a mix of traditions and approaches, to fashion a general overview of the systematics of fossil organisms as practiced today. With the enthusiastic efforts of the contributors, I think we've been successful. The papers in this issue comprise authoritative reviews of the state of the art in various branches of paleontology. But even if one is not concerned with the details of particular groups, the contributions provide a fascinating sense of where the discipline is, and where it might be going. Although concerned mainly with systematic history, they nevertheless provide a flavor of the kinds of concerns we have as a community for the future development of our science.

It seems appropriate to preface such an issue with a brief consideration of the current and future role of the *Journal*, and of systematic paleontology in general. I can make no special claim to insight or experience, but as a working, field-based, systematist many of the concerns and predictions contained in these articles resonated with me. So, as a thoroughly unsolicited preamble to our diamond anniversary issue, I'd like to offer the following thoughts on the status and future of descriptive paleontology, along with some suggestions of areas, both empirical and conceptual, that we might perhaps pay more attention to. My comments pertain mostly to invertebrate paleontology. Obviously, the *Journal* is an important forum for many other fields as well, including but not limited to vertebrate paleontology, paleobotany, and ichnology. However, I'll stick to a subject I'm at least minimally qualified to address.

THREE KINDS OF PALEONTOLOGY: A CASE FOR INTEGRATION

It's not always fair to attempt gross generalizations, but it seems to me that there are three main groups of publishing invertebrate paleontologists with a significant connection to systematics. First, there are workers whose primary concern is description. These people tend to be field-based, and their work tends to emphasize ''traditional'' approaches to taxonomy, biostratigraphy, and paleobiogeography. This has been the natural constituency of the *Journal*, including in the past both academic and industrial workers emphasizing applied paleontology. Applied or not, my sense is that this group remains numerically dominant—people who document new species of fossil organisms and their geological context. The *Journal* still publishes more papers of this kind than any other. One aim of the current editors is to encourage authors to broaden the applicability of these descriptions so that they are of interest to a larger readership.

A second group, which has emerged in invertebrate paleontology largely in the past decade, comprises workers whose approaches are more analytical. Their primary concern is phylogenetic patterns, and their application to our understanding of evolution. Quantitative analyses are now routine in systematic biology in general, and are becoming so in paleontology. Scarcely an issue of the *Journal* passes these days which does not contain some kind of computer-based phylogenetic or morphometric analysis, and most issues contain several. Increasingly, formal analyses (both phylogenetic and morphometric) are becoming a core activity and a substantial component of the Journal.

A third group of paleontologists includes workers whose main activities are syntheses and analyses of existing taxonomic information. Prime among these are the taxic paleobiologists, who use massive compilations of taxa and ranges to examine large-scale patterns in the history of life (e.g., Sepkoski, 1981, 1984). Although there is occasional overlap, these kinds of studies are a stock in trade of our sister journal, *Paleobiology*.

This tripartite division is a simplification, and there are lots of workers who do none of the above, or all of the above. Other emerging fields, especially cross-disciplinary ones like geobiology, are certain to have a major impact in the future. There is still a considerable amount of paleoautecology produced. Taphonomy has a substantial following, and so on. Still, I believe the above three concerns cover a fair amount of the material published by the *Journal of Paleontology* and *Paleobiology*.

Each of these three approaches is of immense value, and all are necessary components of our science. Description (both morphologic and of occurrence in time and space) is the lifeblood of paleontology, and the source of enduring data. Good descriptive work has a utility long outlasting that of most ideas based upon it, and its influence may span decades. Phylogenetic analysis is critical: there is simply nothing more fundamental to all biological pursuits than understanding the shape of the hierarchy of life and interpreting its cause. Moreover, phylogenies are key to understanding the origination of taxa and morphologic innovation. Finally, if we want to appreciate what happened on our planet and why, we need to weld together these factors—phylogenetic structure and geologic context—into a general description of history, and to invent macroevolutionary theories to account for it.

Unfortunately, there has been no small amount of friction between the camps. Descriptive systematists are not always comfortable with numerical phylogenetics, as disagreement has often revolved around the importance of stratigraphy (e.g., Boucot, 1979) and definitions of morphologic characters. At the same time, there has been long-standing mutual suspicion between specimen-based and literature-based groups (see, e.g., Foote, 2001). And, of course, there is a history of strife between taxic paleobiologists and cladists (e.g., Patterson and Smith, 1988; Sepkoski, 1989).

At present, different workers engage in each of these pursuits, and by and large they publish their results in different places (explicitly so, in view of editorial policies discouraging competition between sister journals, e.g., Erwin and Wing, 1996). While there is talk of rapprochement (Foote, 2001), it has only just begun, and has a long way to go before the full range of possibilities and benefits are recognized.

All three activities are mutually dependent. Fieldwork and descriptive paleontology reveals (some of) the components of the hierarchy of life. We can't estimate the shape of that hierarchy—phylogeny—without detailed understanding of those components. And, though some would disagree, I do not think we can make

further serious progress in our understanding of macroevolutionary phenomena without drawing phylogeny into a central role.

There are also practical concerns about the divisions. An impending shortage of trained taxonomists is going to be keenly felt in paleontology (e.g., Feldmann and Manning, 1992; Lee, 2000). In the current state of affairs, though, it is difficult to see how this trend might be reversed. Bluntly, publishing descriptive systematics is a difficult way to find a job. Traditional employment for ''traditional'' paleontologists—industrial jobs, and academic training centers for industrial jobs—is under serious threat. Phylogeneticists have a niche, albeit a highly competitive one, as curators in natural history museums. A significant share of major academic positions in the past 20 yr have gone to paleobiologists—analytical paleontologists working under the umbrella of evolutionary paleoecology.

I don't wish to champion or denigrate any of the camps—I think they are equally important. The point is that concentration in them carries unequal professional rewards. If they are truly mutually dependent, this cannot be good news for the future health of our profession.

I think a vital future for paleontology may require more than conciliation and cooperation between workers with these different concentrations. I think we should dispense with the distinctions altogether, and work towards a training in paleontology which integrates the approaches, from micro to macro, from specimen to algorithm, and from outcrop to library. A paleontologist should be able to collect a fossil and document its morphology and context. A paleontologist should be able to formulate an explicit hypothesis of relationship, founded on hard-earned knowledge. A paleontologist should be able to use phylogenetic hypotheses to illuminate studies of evolutionary process, of biogeographic change, of global diversity through time, of unique contingencies, extinctions, and explosions.

We need to train a new generation of paleontologists who can return to the field and develop new data just as they analyze historical questions. Soon enough it seems that very few people may be employed primarily to create descriptive data. If we fail to return empirical, field-based science to a central position in our profession, and to emphasize its place as a necessary responsibility in a process of discovery, we may simply grind to a halt. Paleontologists must become paleobiologists, and paleobiologists must become paleontologists. People who describe fossils as an end in itself risk professional marginalization and people who know fossils only as abstract information risk rapidly diminishing returns, as a static pool of literature data is endlessly reanalyzed.

A MORE ANALYTICAL JOURNAL

The *Journal of Paleontology* has long served as a storehouse of primary paleontological knowledge, one of the principle archives of descriptive data. That should never change, and probably won't. Good descriptions of new fossils are always ''significant,'' and must always have an outlet. Like the profession, the future health of the *Journal,* however, depends on more effective integration of descriptive, analytical, and synthetic approaches. There is an undeniable trend of increasing analytical content in the *Journal,* almost always set on a foundation of new empirical data. This may be the way of the future: we can never stop developing new data, but we need to do something with these data as we go along. As a complement, we should instill in our students the ambition to address ''big-picture'' concerns, but grounded in an ability and responsibility to develop their own field data and expertise in a particular group or groups of fossil organisms. A distinction between systematics and synthesis should become archaic, and is best ultimately abandoned, or replaced with a distinction between pure description, and a process of interpretation.

A MORE EMPIRICAL JOURNAL

The *Journal,* in the future, may well adapt to a dual role through the nature of its submissions. Online publication should be a boon for description—as costs of publication shrink, both the scope and scale of documentation can increase. Presently, we are used to a paradigm in which image data are limited by costs of paper reproduction of plates (and to a certain extent, it has to be said, by tradition). These limits should largely disappear, more quickly than perhaps we expect. Whatever shape a new paradigm for electronic dissemination of information takes (e.g., MacLeod and Guralnick, 2000), it will almost certainly allow a quantum leap in the depth with which fossil taxa are illustrated and described. In concert with an increase in analytical studies, I foresee much greater opportunities for traditional description. All new fossil data are important, and electronic outlets should allow greater opportunities in the future for providing more complete documentation and establishing their ''significance.''

TASKS FOR SYSTEMATIC PALEONTOLOGY

There are, I believe, a few major issues or questions that we will need to grapple with in the next while, if description and systematics are to be reemphasized and realize their promise:

1) How do we ''image'' fossils? Technical aspects of paleontology are one of the most comprehensively ignored facets of our science, yet they control the nature of the data we present. It almost feels old-fashioned to worry about the details of photographing fossils, yet conversation with colleagues and postings on PaleoNet indicate that there are very few places to turn for advice. We still, as a community, operate mainly on word of mouth about technical details, and we largely do pretty much what our graduate advisors taught us to do. Images, however, are no longer an afterthought to a study—they are not merely illustrations. They are an investigative tool, they comprise the data we examine digitally, and they have to large extent become the language we use to communicate. I think it is imperative that we raise the accepted professional standards for imaging, and at the same time develop some form of web-based center for collection and exchange of information on photography, digital imaging techniques, and equipment.

2) How do we extract data from images and delimit morphologic characters and taxa? Although still largely the domain of neontologists, the topic of character concepts, in particular, has recently generated much excitement in the systematics community and has been the subject of several edited volumes (e.g., Scotland and Pennington, 2000; Wagner, 2000; Wiens, 2000). Character selection and delimitation are necessary prerequisites to any meaningful phylogenetic analysis, and thus lie at the interface between traditional description and phylogenetics. This little explored area of research is critical to integrating description and analysis, and bringing the first two camps together.

3) How do we generate phylogenies? Debate, of course, rages on this issue. As a community, we have a unique perspective on phylogenetic issues—we have access to the dimension of time (and if you don't think this makes paleontology unique, try talking to a neontological systematist, for whom time tends to be the present, or a progressive expansion to the present, or a relative nesting of cladogram nodes). We have an enormous opportunity to express our relevance to systematic biology in general (even molecular systematists acknowledge that we are required, if only to calibrate molecular clocks), but we need to be very careful about how we marshal our information. If we let time actively govern the shape of our phylogenies and hence our classifications (through the use of stratigraphic ''data''), we simply throw away our unique insight. Time can calibrate our phylogenies and give

them enormous predictive power; phylogenies artificially constrained by and fitted to time and stratigraphy are impossible to apply to any of the great temporal questions we can otherwise address. Resolution of the debate over whether phylogeny should be read from the rocks is central to the future development of paleontological systematics.

4) How do we use systematics to examine diversity change through time? This may seem like a dense question, since the research program pioneered by the late Jack Sepkoski has already done what most (myself included) would consider a pretty reasonable job in this area. But taxic paleobiology, it is well understood, has used taxa as proxies for biological species, and as such has treated rank-equivalent taxa as genuinely equivalent. How else to proceed? Well, that's the question. I'm convinced that we need to think about the interface between phylogenies and diversity, in order to develop a more generally defensible connection between the two. Such work will necessarily take place at the species level. To anyone who considers the amount of work required prohibitive and who wishes to grapple with global diversity right away, my personal reply is that: 1) the amount of work is only prohibitive if nobody is doing it, and if nobody is being trained to do it; and 2) thanks to Jack and the research program he fashioned, I think we have a pretty good idea about what the current taxic database tells us. We know what the forest looks like; now it's time to reexamine the trees. And perhaps if we do this in a rational and explicit way, with scientists trained to create and assess descriptive and phylogenetic data, we will discover that the forest is in fact a larger, wilder, and very different place.

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