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After a political year frequently dominated by conversations about absolutism versus maximalism, it is perhaps appropriate to reflect on the somewhat rational nature of our field with respect to these same distinctions. Science is committed to the notion that there is an objective truth out there in the physical world, and that we can best approach it by reference to that physical world. At least in our introspective moments, however, scientists also realize that we can never "know" that objective truth with complete certainty, that all of our ideas are provisional, and the best we can hope for is an asymptotic approach to truth. In the meantime, what we judge as "true" is the hypothesis that currently most closely fits the evidence, the observations we make about the physical world. In order to do this, we operate under a set of rules. We make observations and report them in a way so that they could, in principle, be verified by someone else. The ideas we accept should be supported by these verifiable observations. In practice, however, only tiny fractions of all scientific observations are actually verified by someone else. Thus, like the tax system, much of science operates on trust. When we read a paper published in a reputable peer-reviewed publication by an author at a reputable institution, we have reasonable confidence that everything was done honestly; we treat the data as accurate, and fairly evaluate what we read on that basis. We may then use the author's conclusions to build on in our own work. Thus science grows, and we approximate the "truth." We cannot go back and verify every observation and conclusion; if we did we would never get anywhere. It's certainly not a perfect system. Sometimes (perhaps even frequently) excludes "outsiders," discourages iconoclasm, encourages conformity, and perpetuates paradigms beyond their useful lifetime. Once in a while, there is outright fraud, and "truth" sometimes takes a while to come out. Yet for every frustrated author who dies in obscurity, but is later seen as a visionary, there are hundreds or thousands of individuals simply doing sloppy science that the system catches and which it tries to impose some discipline that, it is hoped, leads ultimately to a more reliable understanding of the physical world. Not everyone is an Alfred Wegener just because they get poor reviews. Sometimes it's because they do poor science.

Which brings us to the prolific work of Edward Petuch, the latest example of which is the book here under review. For more than 20 years, Petuch has been publishing on the abundant Neogene mollusc faunas of the eastern United States. He has written a host of papers on these faunas (Petuch, 1987, 1988a,b, 1991, 1992, 1994, 1997), he started his own journal, Bulletin of Paleomachology (which published only four issues in 1986 and in which he authored all but 21 of its 82 pages); and he has published many other smaller papers in a variety of venues. In all of this work, Petuch has discovered and identified some genuinely amazing things (most of which are summarized in the present volume). I have been with him in the field, in the shell pits of the Florida Everglades. I have seen some of the bizarre and previously unknown fossil mollusk he has recovered from the Piliekootsenee spoil pits there, including eight-inch olive shells with orange spots and an undoubted endemism (of uncoiled pinnulariid gastropods. I have seen some of the fossil corals and reefs around the Everglades that previously had been known, but that he (along with Jack Meeder) highlighted and reinterpreted (I think reasonably) as "pseudo-atomic". In 1982, he published and formalized the earlier work by Muriel Hunter (in Weisbaden, 1981) on the stratigraphy of the famous Pilolecsea shell beds at Sara, Florida, and his scheme has formed the basis for most all of the research on these incredible faunas that followed. His idea of "geographic heterochrony" probably is a useful one. He has a strong and original visual sense, frequently expressed in well-executed drawings of ancient communities. The present volume includes some nifty "simulated satellite images" of various moments in Florida's geologic past.

The problem is that Petuch's systematic work is so consistently sloppy that it is difficult to tell which of his numerous ideas and newly described taxa are valid, and which are not. This is not an issue of tiny details. Serious methodological problems are evident in almost all of Petuch's publications (including this one), which make his abundant descriptions of new taxa at best problematic and at worst indecipherable. Descriptions often are short and illustrations poor (although both are considerably improved in the present volume), usually without reference to previous work by anyone else. Locality and stratigraphic information is often lacking, inadequate, or demonstrably incorrect. Type specimens often have been hard to find and verify. All of this is made worse by the fact that he is a taxonomic splitter of truly prodigious proportions, erecting new genera when other workers on the same faunas would not recognize even a new species (e.g., Ward, 1962). His operational species concept, such as it is, is not only tautological but tautological; there is seldom any mention in his descriptions of variation within populations or species, and essentially all "paleospecies" are by definition endemic to very narrow blocks of time, space, and environment (Petuch, 1997:9).

No one to my knowledge has ever tried to comprehensively catalog these problems in Petuch's work. It is fair to say that it would be a daunting task. Petuch (1995) made a start. More recently, Ward (pers. comm., 2004) has gone further, itemizing, among other examples, giving different localities for the same specimen in succeeding publications, giving multiple names for the same specimens, publication of specimens and refriguing in succeeding publications, and naming of new species from beds that are not present in the locality given. (This work is currently in press.) The ubiquity of these issues ultimately prevent Petuch's systematic work from doing what Geerat Vermeij's carefully worded Foreword to the present volume says it hopes it will be: a "solid " descriptive foundation for future research.

The structure of the book follows that of several previous works, which is to say, it applies a dubious analytical approach to questionable systematics. The author names numerous new taxa (11 new genera, 5 new subgenera, and 37 species of gastropods, and one species of bivalve), and then uses the apparent endemicity they indicate to designate paleobiogeographic units. The entire enterprise is reminiscent of the most profitless of nineteenth-century natural history: a combination of the compulsion to name as many species as possible and the urge to name and (especially) really biogeographic entities. Petuch classifies the Neogene history of the eastern U.S. coast into a series of "paleoecosystems", which are then divided into "subas"es, "provinces," and "communities." These "seas," he explains, are "true seas in the strict oceanographic sense... bodies of salt water... structurally bound on at least three sides... occupying... geologically discrete basins... (and containing) their own distinct distributions of currents and water masses, and... their distinctive endemism... "neogenoecosystems" (p. 1). Separate "provinces" are recognized "if at least 50% of the species-level taxa are endemic to each area" (p. 21). Yet, there are no tables of numbers of taxa that define the provinces, and no detailed analysis of the "structural bounds" that make these "seas" geologically discrete. There are only the names, and once named, the seas and subas and provinces are treated as real. And that's only the beginning. There are also new names for ancient archipelagos, currents, embayments, extinct species, forays, gyres, islands, lagoon systems, lakes, platforms, primary reef pockets, reef systems, reef tracts, shoals, and straits. Nowhere, however, is there adequate discussion of the actual usefulness of proposing these names, or consideration that they might be completely artificial constructs imposed on continuums. They are simply "recognized" or "discovered." Changes in climate and episodes of extinction and diversification are similarly treated. (Petuch 2004: 1) cites no less an authority than Sloss (1963) for his terminology, but I can find no mention of "seas" or "paleoecosystems" in Sloss's paper. Sloss does, however, make a comment that one wishes Petuch had taken to heart: "Although the names [of stratigraphic sequences] are mere appendages to the concepts and principles involved, it is a further purpose of the present paper to demonstrate the utility of, and the necessity of, applying names to the six sequences recognized" (Sloss, 1963, p. 94).

Petuch knows that he has critics, but his comments appear to have no effect at all. I have heard him say that he is not writing for his critics or current peers who do not "understand" him, but for "posterity," and that he does not care what reviewers think of his work.
know personally that this resistance to criticism has been in part responsible for the changing series of publishers of his many books, as well as his habit of publishing frequently in out-of-the-way journals, including his own. I once heard a colleague say, with the profusion of journals and publishers out there (to say nothing of the Web), it was probably possible to get any scientific article published somewhere, no matter how bad it was. Petuch's bibliography is telling evidence that this may be true.

Why does any of this matter? Ironically, it matters because Petuch has discovered some new and important things. If he were clearly wrong in everything he did, then his work could be safely ignored in toto. But, instead, we have the worst possible situation: some of what he says is probably correct, but who can tell which is valid and which is fantasy? It will take a generation to evaluate it all. Instead of leaving behind such a mess, with his energy and ability he (still) could instead create a huge body of work that could actually be used for something. This isn't just an issue for “posterity.” The current validity of new taxa is no more bookkeeping task in the age of taxonomic databases and “analytical paleobiology.”

David Raup once said that paleontology was the only scientific field he knew of that had been at it for centuries and still didn't trust its data, but we mostly do of course, and that makes much paleobiological research possible. The reason we trust our data is that we trust our colleagues to be honest. The whole edifice of our profession depends on it.

When I decided to do my dissertation work on Cenozoic mollusks of the coastal plain, one of my professors asked seriously, “Hasn't all that been done?” The answer was and is clearly “no”; even in one of the most paleontologically explored areas of the planet there is still much that is new and important to discover. It is a real pity that one of the most prolific workers on these fabulous faunas muddles rather than enhances that process of discovery.

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