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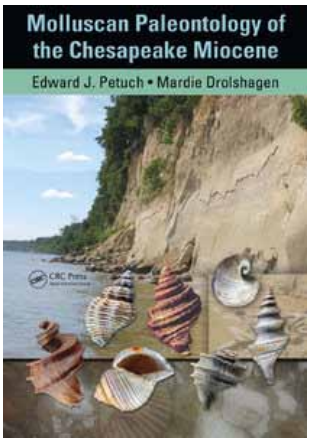

Molluscan Paleontology of the Chesapeake Miocene, by Edward J. Petuch and Mardie Drolshagen, 2010, CRC Press, 160 p. + CD-ROM, ISBN: 978-1439811597, $109.95

Peer review certainly isn't perfect. It doesn't guarantee that no "bad science" gets published or that only "good science" does. Indeed, some scholars in the humanities have recently been experimenting with other approaches, such as using the Internet to expose work to collective judgment of a much wider audience. In the meantime, however, peer review remains the primary technique scientists have to validate what we do. Not understanding or accepting this, at least in principle, can lead to unfortunate results. Witness the ongoing controversy over whether the overwhelming scientific consensus about anthropogenic global warming is due in part to discrimination against alternative views in professional journals (e.g., www.ncdc.noaa.gov/paleo/globalwarming/peerreview.html, and www.realclimate.org/index.php/archives/2005/01/peer-review-a-necessary-but-not-sufficient-condition/).

If a scientist publishes outside the peer review system, he or she certainly enjoys some advantages. It is usually faster and much less annoying. If you find an indulgent publisher, you might be able to write just about whatever you want. And there is certainly no reason why excellent science cannot be published this way. Like so many other things in life, however, the problem is not with those who do things well, but with those who do not. Since all scientific conclusions are, by definition, provisional (i.e., we never know for certain what the answer is), we must rely on the building of scientific consensus to decide what we think is (at least for now) supported by the evidence. And, so far, the only way to do this is peer review. As philosopher Daniel Dennett puts it, "The discipline of submitting your claims to the judgment of peer review, where you have to respond to your critics or withdraw or revise your assertions, is the chief antidote to wishful thinking" in science (Dennett, 2006, p. 45).

This is particularly important in systematics, in which contributions are rigidly cumulative and all previous work must be dealt with by subsequent authors. When sloppy systematic work appears outside of peer review, it is not just a problem for systematists. Other people (including many nonscientists) use systematics for other purposes—to measure diversity, to reconstruct evolutionary patterns or processes, or to recommend conservation strategies. And these users need to be able to rely on the data in the literature as having passed through at least some kind of critical vetting.

Although descriptive (alpha) systematics is viewed by some as the intellectual equivalent of stamp collecting, the quality of basic systematic data is increasingly at the center of major theoretical and practical controversies in several fields. In conservation biology, for example, "taxonomic inflation" has become an important issue (e.g., Sangster, 2009, and references therein). In paleontology, the completeness, validity, and meaning of our systematic data continue to be topics of vigorous discussion (e.g., Alroy, 2002; Smith, 2007; Benton, 2010). Sloppy systematics makes all systematics look bad, and all paleontologists have an interest in maintaining the quality—and perception of quality—of systematics.

The book under review is the latest episode of a major example of our field's collective inability and/or unwillingness to maintain the quality of systematics. For more than 25 years, Edward Petuch and coauthors have been publishing on the abundant Neogene marine mollusk faunas of the southeastern United States. At last count, ten previous books on these faunas (Petuch, 1987, 1988a, 1988b, 1991, 1992, 1994, 1997, 2004, 2008; Petuch and Roberts, 2007) have been published by at least five different presses. In these and other publications, Petuch has, by his own account, described hundreds of species of fossil and living mollusks. Although he has published in the mainstream peer-reviewed literature (e.g., Petuch, 1976, 1982a, 1982b), none of his major systematic works that contain all these taxa have been subject to serious peer review.

Systematics has long acknowledged the existence of splitters and lumpers, and this has essentially been accepted as a cost of doing business. As noted by Donovan and Van Den Hoek Ostende (2009), for example, the most egregious splitters are known and can be ignored. As I suggested in a review of one of Petuch's previous books (Allmon, 2005), however, the problem with Petuch's work is that although almost all of it is sloppy, it cannot be discounted wholesale. This is because he has discovered and reported some genuinely new and amazing

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things. His systematic work is so inconsistent and error ridden, however, that it is difficult to tell which of his numerous ideas and taxa are valid and which are not.

Evidence that Petuch's previous work is problematic is abundant and long standing, and any review of the present book must take this history into account. In a review of Petuch (1988), for example, Donovan (1991, p. 53) concludes that "The lack of peer review supports my fear that other malacologists will now regard the ecphorine gastropods as being grossly oversplit." Indeed, in a subsequent review of *Ecphora*, Ward (1992) synonymizes at least 36 Petuch species names, stating at one point that, "It is clear that Petuch does not recognize variability within a species, or within a population of a species at a single locality" (1992, p. 127). In the present volume, Petuch answers Ward's critique only by saying that, "This lumping of the other 32 taxa does not take into consideration the evolution and extinction patterns of the ecphora [sic] lineages discussed previously in this chapter and obfuscates any attempts at a fine-tuned stratigraphy…" (Petuch and Drolshagen, 2010, p. 43). Left unanswered is whether they are valid species or not.

In reviewing his work on muricid gastropods, Vokes (1992a, 1992b, 1994) accepted 18 of Petuch's names and rejected 9. Hendricks (2008) reviewed 47 species of *Conus* from the Pliocene and Pleistocene of the southeastern United States previously described by Petuch, and he accepted only seven. Rosenberg (1996) and Filmer (2001) reported percentages for all of Petuch's Recent species-level taxa recognized as valid by other authors; these were 71% among Western Atlantic gastropods and 52% among worldwide *Conus*, respectively. At my request, Rosenberg recently reanalyzed his Malacolog database (Rosenberg, 2005), which is a synthesis of the literature on living Western Atlantic gastropods, and found that of the 152 modern species in the database named by Petuch, only 89 (59%) are currently regarded as valid by other authors.

There have been many other problems over the years. As noted by Petit (1995), Petuch (1987) republished, with permission, photographs of specimens of Recent mollusks from Bayer (1971). In several cases, however, Petuch erroneously cited the provenance for these specimens. For instance, for the gastropod *Fulgurofusus brayi*, Bayer (1971) listed its locality as "P-781 off eastern Colombia, 11°30.1' N, 73°26.5' W, 567–531 m" (p. 175), and the size as "28.5 mm" (plate caption); Petuch, however, gave the locality as "150 m depth off Golfo de Uraba, Colombia" and the size as 37 mm (Petuch, 1987, p. 122, pl. 18). Petuch (1994) names and figures a holotype for a new subspecies, *Strombus gigas pahayokee* (although it is labeled as "n. sp." [p. 82]), but he does not include a description of the taxon. Examination of specimens figured in Petuch (1994) shows that the holotype of *Busycon superbus* (specimen UF 66317) has a completely "rebuilt" siphonal canal, and the holotype of *Scaphella oleiniki* (UF 66377) has both the siphonal region and lip "rebuilt." Yet these restorations are not mentioned in the text or figure captions. Ward (2008) documents that, in his work on the gastropod genus *Ecphora* alone, Petuch has given multiple names for the same specimen, altered the same specimen in images in succeeding publications, named species from beds that are demonstrably not present at the locality given, and figured specimens that clearly did not come from beds he indicated.

Numerous errors have also been found in the disposition of Petuch's types. Donovan (1991, p. 53) summarized the problem in Petuch's early work: "Too many of the figured specimens are anonymous, with no indication of where they are deposited, and in at least one example, a type specimen is in the author's private collection, rather than in a recognised research institution." It is true that more recently Petuch has deposited his types in public institutions, but it is also true that, as Donovan noted, his types have been frequently "hard to find and verify." At my request, collections managers from two institutions in which Petuch previously deposited type material provided information on the condition of that material. One noted that there were many problems, including incorrect citation of catalog numbers, name changes after the specimen was donated, holotypes becoming paratypes, and so-called types of names that were never published in the manuscript. The other reported that for a single monograph, there were plate number reversals, specimens deposited as holotypes that were not figured, measurements incorrect (some by an order of magnitude), incorrect figure designations, different specimens given the same number, and designation of paratypes not listed in the publication. Bieler and Bradford (1991) found numerous errors in Petuch types in the Delaware Museum of Natural History, including numbered type specimens never received, citation of incorrect numbers of specimens, specimens cited by incorrect numbers, and presumably figured specimens not agreeing with figures. In his 2004 book (Petuch, 2004), it is extremely difficult to match up figures with actual specimens deposited in the American Museum of Natural History. Specimen numbers do not appear in the captions, and the numbers that appear in the systematic section do not refer to specific figure numbers; sometimes, there is no match at all.

Petuch chose to deposit the types for this book in the collections of my own institution. I was pleasantly surprised to find that all of the type specimens of the 28 new species he describes (20 gastropods, 8 bivalves) match their illustrations and numbers in the text. Most of their measurements match as well, although the holotype of *Busycotypus martini* actually measures 125 mm instead of 178 mm as stated by Petuch and Drolshagen (2010, p. 83, 128). Other (nontype) specimens of the new taxa are listed as "in the research collection of the senior author." There is, however, no information given about repositories for the hundreds of figured specimens of previously described species. These are basic points that would have come up in any peer review of this work.

I was also pleased to see that some aspects of Petuch's presentation have been improved in this book over its predecessors. The photos are better. He also admits to two previous errors: on p. 75, he states that a list of 20 mollusk species "were incorrectly designated as coming from Shattuck Zone 16. This should now be emended to state that they came from Shattuck Zone 17"; and on p. 77, a list of 54 species "were incorrectly designated as coming from Shattuck Zone 17. This should now be emended to state that they came from Shattuck Zone 18." The species descriptions are more complete and mention the work of other systematists more than almost any of Petuch's previous works, although they still make virtually no mention of variability within species and almost every variant is a new taxon.

Despite these improvements, the present book is missing numerous basic features that one would expect a serious re-

view to have recommended, such as a locality map, a diagram of his proposed "seas" versus stratigraphy, a diagram showing temperature through time (since this is mentioned frequently in the discussion), and family names in the index. More serious is its lack of simple quality scholarship. For example, on page 16, Petuch breezily says that "Marine biogeographers such as Valentine (1973), Briggs (1974), and Petuch (2004) have divided the coastal areas of the world into discrete geographic areas called 'provinces' … defined by the '50% rule'…" Neither Valentine nor Briggs endorses this rule, however; indeed, Valentine (1973, p. 337) specifically states that "there is no special reason to employ any particular arbitrary level of endemism," and virtually any recent general discussion of biogeography or paleobiogeography (e.g., Cecca, 2002) emphasizes the subjectivity of biogeographic units. In any case, neither in this book nor elsewhere has Petuch ever presented summary quantitative data on endemism to support his prolific biogeographic naming.

Similarly, in both his 2004 book and the present volume, Petuch includes extensive discussion of former "seas" along the southeastern United States coastal plain. It is one of his major themes, and one would therefore expect him to devote serious attention to the definition of terms and concepts. In the 2004 book, he cites Sloss (1963) as the authority for his use of the term "seas" as "bodies of salt water . . . structurally bound on at least three sides . . . [occupying] geologically discrete basins, [containing] their own distinct configurations of currents and water masses, and . . . their own distinctive endemic organisms and ecosystems" (Petuch, 2004, p. 1), yet Sloss never mentions any such definition in that paper (Allmon, 2005). In the present book, the authors persist in referring to "true seas in the oceanographic sense . . . [which] fit all the criteria for designations as true seas…" (Petuch and Drolshagen, 2010, p. 1), this time giving no references at all other than Petuch (2004). I have scoured literally dozens of geology texts and spoken to numerous colleagues, and nowhere can I find a mention of any formal definition resembling this one. My AGI *Glossary of Geology* defines "sea" simply as "An inland body of salt water" or "A geographic division of an ocean" (Gary et al., 1972, p. 637). The significance of this tempest in an embayment, besides its pseudoscholarly shoddiness, is that it clearly reveals the authors' main objective: recognizing seas and subseas creates isolated worlds that need to be populated by swarms of new endemic species, whether or not they are supported by the evidence.

Petuch (personal communication, 2006) has previously protested such criticism of his work by saying that his work has been fundamentally misunderstood. Titles such as *A Field Guide to the Ecphoras* (1988a) and *Atlas of Florida Fossil Shells* (1994), he claims, show that these are not intended as primarily technical books, but rather guidebooks for enthusiastic collectors. Similarly, it is stated that the present volume is a "field guide" (Petuch and Drolshagen, 2010, p. xiv) for "any fossil collector" (p. xviii). Yet in style and substance (not to mention price), it is a technical monograph (from a mainly technical publisher) that defines numerous new taxa and other phenomena, and as such it should have borne the burden of peer-review scrutiny.

All scientists make (and even publish) errors, sometimes despite peer review. Many described taxa are later synonymized or otherwise invalidated, but the nonspecialist world is encouraged to trust scientific conclusions because the peer review system is there to verify that those conclusions are not just opinions. If science becomes just opinions, then we are all in trouble. The uncomfortable fact is that Petuch has frequently and consistently done poor science, and has successfully managed to get a considerable amount of work published outside the peer-review system for many years. The result is not necessarily wrong; but it is a mess that will take someone (probably many someones) a long time to straighten out. Perhaps most importantly, Petuch's colleagues (including me) have been thinking and saying much of this among ourselves for 20 years or more, but we did not do much about it. That is perhaps as much a comment on us as on him.

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Warren D. Allmon
Paleontological Research Institution and Dept. of Earth & Atmospheric Sciences
Cornell University
1259 Trumansburg Road
Ithaca, NY 14850, USA