Book Reviews

Possibilities for Paleontology

Models in Paleobiology. Papers from a symposium, Washington, D.C., Nov. 1971. THOMAS J. M. SCHOPF, Ed. Freeman Cooper, San Francisco, 1972. vi, 250 pp., illus. $9.50.

Schopf, the editor, suggests very personal motivations for this book in his introduction. He rejects the excessive scholasticism of a paleobiology devoted primarily to description, believes paleobiology should be made relevant to a wider audience, and seems to yearn for the special excitement of deriving and testing the predictions of models. His book is filled with great expectations: "Implicit in it is the belief of the group [of contributors] that paleontology has collected much of its data and basic theses. Explicit is the belief that henceforth paleontologists may and should turn to broader horizons and interpretive themes."

The resulting collection maintains a high level of professionalism—little of the embarrassing pep talk about models and mathematics has filtered through. Ghiselin authors the only paper specifically on methodology, and it is an astute paper by a practicing scientist about the logic of discovery in his field. The other papers stick to scientific issues and are grouped about the headings of morphology, population ecology and evolution, and biogeography and community ecology.

Although all the papers are very thoughtful, only the paper by Hallam actually presents a theoretical model intended for test with paleobiological material. Hallam discusses a model fused from demography and growth studies to explain the size frequency distribution in fossil shell deposits. Most of the remaining papers are essays reviewing earlier approaches, suggesting new ones, and otherwise interpreting existing information in a new conceptual framework. The two interesting exceptions are at the extremes of "all test" and "all theory." Stehli, Douglas, and Kafescioglu test the extension of empirical results on the temperature distribution of hermatypic coral genera to the temperature distribution of planktonic foraminiferan species. But they do not derive the hypotheses under test from any a priori theoretical argument. At the other extreme Simberloff presents a lucid and valuable review of the mathematical theory of island biogeography, including the recent advances following the pioneering work of MacArthur and Wilson. But his discussion relating this theory to paleobiology is brief and vague.

In one of the most interesting and provoking essays, Eldredge and Gould examine the importance of gaps in the phylogenetic record in the context of the principle of geographic speciation. According to this principle, as enunciated by Mayr, new species form in small populations separated from the main species population by a physical barrier to dispersal. If so, then as Eldridge and Gould argue, it is unlikely that the phylogenetic record will sample the new species until some late stage in its formation when the range has increased. Hence gaps appear in the phylogenetic record. Although this idea is certainly attractive, I suspect the case would be more cogent if predictions from a mathematical model were presented for test. As is, we have simply a reinterpretation of known data in a new framework—another new synthesis. And what better topic for a model than the principle of geographic speciation? The principle is currently trading on its intuitive appeal, the authority of its proponents, and its power as a synthesizing principle. But acceptance is transient. To retain acceptance the theory of geographical speciation should be developed to predict a priori how strong a barrier must be to produce speciation, how small the peripheral isolate must be, how different the environment must be on each side of the barrier, how long the separation must be maintained, and, especially, how these factors interrelate. Such a theory would predict where, when, and how fast speciation occurs and would be testable against the phylogenetic record in a much stronger sense than merely providing a new framework for synthesis.

More generally, most of the papers see the past strictly in terms of the present. Indeed, Eldredge and Gould declare, "We [paleontologists] can apply and test, but we cannot generate new mechanisms. If discrepancies are found between paleontological data and the expected patterns, we may be able to identify those aspects of a general theory that need improvement. But we cannot formulate these improvements ourselves." I feel the bias introduced by this commitment is far-reaching. On its face the commitment is false, for it is possible that paleontologists *could* be the first to discover causal mechanisms with long time constants. Moreover, in making this commitment the actual success of those investigating the present is overestimated. A working myth in population biology is that present-day ecology is explainable in terms of the present-day environment, or at most the very recent past—witness the importance in ecology of "equilibrium models" in which the role of the past is lost. Yet the myth of the completeness of the present is in part a necessary response by working scientists to the failure of paleobiology to supply hard, usable predictions about the influence of the past. A strong, healthy paleobiology should be able to predict in a testable manner what, if any, the phylogenetic constraints are to present-day ecology. The Eldredge and Gould commitment would preclude this possibility with a biased methodology.

In all, the book is certainly a thoughtful and interesting contribution, and many will support its intent of introducing rigorous theoretical reasoning, including the use of mathematical models, into paleobiology. But the honeymoon is certain to be short—the idea of using mathematical models has lost its shock appeal by now. The emerging theoretical paleobiology will soon be critically judged on whether models have been developed which are a priori cogent and rigorous and on whether these models have in fact led to new and successfully tested predictions.

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