Modeling deep time: The paleobiological revolution


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Stephen Jay Gould once famously compared paleontology to Cinderella, whose great beauty and considerable virtue were only recognized after many long years of being made to toil in filth and obscurity by her evil stepsisters. Much like the girl in the fairy tale, Gould lamented, paleontology had been ignored and marginalized for most of the twentieth century. Thus, it was with considerable glee that he announced ‘our profession now wears the glass slipper and, if not the queen of the evolutionary ball, at least cuts a figure worth more than a passing glance.’ What brought about this remarkable change in circumstances? Always a friend to contingency, Gould was quick to point out that there was nothing inevitable about his discipline’s recent good fortunes. Nor for that matter had its longstanding obscurity been the fault of anyone but paleontologists themselves. Paleontology’s problems could not simply be chalked up to the infamous ‘poverty of the fossil record.’ Rather, the culprit was a longstanding ‘inadequacy of innovation in theory’ [1].

The story of paleontology’s long and winding road to biological respectability is the topic of a fascinating new book by David Sepkoski. Titled Rereading the Fossil Record, it asks how an ambitious group of mathematically inclined scientists (including the author’s late father) succeeded in rebranding their field. Self-consciously adopting the label of ‘paleobiology’ to distinguish themselves from more traditionally minded colleagues, they set about realigning their discipline’s institutional and epistemic commitments. Rather than continue looking to geology for inspiration and support, paleontologists developed tools with which to extract novel insights about ecology and evolution from the fossil record. But the paleobiological revolution was also a social, institutional, and disciplinary achievement. New theories like Niles Eldredge and Gould’s punctuated equilibria or Jack Sepkoski and David Raup’s periodic mass extinction events certainly mattered a great deal. As Rereading the Fossil Record shows, though, the founding of a new journal (aptly named Paleobiology), the hiring of paleontologists by prominent biology departments, and the widespread publicity given to meetings like the ‘Conference on Macroevolution’ held at Chicago’s Field Museum in 1980 were at least of equal importance.

The paleobiological revolution did not, in fact, give rise to the first generation of paleontologists who sought out a meaningful conversation with evolutionary biologists. Rather, there is a clear line of descent that connects the movement’s main progenitors via G.G. Simpson and H.F. Osborn all the way back to E.D. Cope, a highly prolific thinker whose non-Darwinian theory of evolution helped set the terms in which late 19th century biologists debated the origins and maintenance of organic diversity. That said, major differences between the Young Turks of the 70s and 80s and their disciplinary forbearers did exist. For example, whereas the latter worked primarily on fossil vertebrates, the former tended to specialize in invertebrates. This is important on a number of counts, the most relevant being that invertebrate fossils are far more common and widespread than vertebrate ones and are therefore amenable to quantitative study.

A fascinating subtext that runs throughout Sepkoski’s account goes beyond institutional history and gets at the heart of what it meant to do paleontology and be a paleontologist during this period. The paleobiological revolution indexes a deep shift in what counted as legitimate scientific practice among working paleontologists, one whose true significance comes into focus once it is viewed through the prism of a much larger story about what happened to biology over the course of the 20th century. Despite widespread agreement among historians that the modern synthesis was primarily an act of political consolidation — resembling the creation of imperial Germany during the late 19th century more so than the articulation of a new theoretical framework — it also helped to legitimize the use of mathematical models among biologists. In their efforts to reconcile Mendelian genetics with Darwinian evolution, theoretical population geneticists such as R.A. Fischer, J.B.S. Haldane and Sewall Wright relied on a mode of reasoning that explicitly substituted abstract and often idealized representations for literal descriptions of empirical phenomena. Nobody mistook Wright’s island model of migration — in which each subpopulation receives an equal proportion of migrants from each other subpopulation, including itself — for an empirically plausible picture of gene flow. But it nonetheless came to have enormous influence, helping to spread what Richard Levins would later describe as the strategy of model building in population biology [2].

Still controversial during the 1970s, the claim that idealized and abstract models provide reliable access to deep time endowed paleobiology with an air of revolutionary excitement. For example, an early and much discussed paper with the forbidding title of ‘Stochastic Models of Phylogeny and the Evolution of Diversity’ explicitly cited Levins in support of the bold claim ‘that the true complexity of events in the real world can be adequately rendered by models using relatively few factors.’ Such statements served as a kind of rallying cry, especially when they appeared in The Journal of Geology, a relatively conservative publication usually given to printing descriptive articles communicating new factual data. Indeed, many

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paleontologists were so skeptical of model based science that Raup’s groundbreaking study of adaptive landscapes and theoretical morphospace almost failed to get published in Science because it was based on the analysis of computer simulations rather than fossils themselves. As Raup recalled during an interview later in life, one of his reviewers ‘recommended rejection on the grounds that the work was not science’ [3].

Sepkoski brings his account to a close by quoting a famous review that John Maynard Smith published in 1984. It was not long ago, the eminent geneticist recalled, that ‘any paleontologist rash enough to offer a contribution to evolutionary theory’ would have been told ‘to go away and find another fossil, and not to bother the grownups.’ But now the situation looked different indeed, and Maynard Smith was pleased to be welcoming paleontologists back to the ‘high table’ of evolutionary theorizing [4]. What these remarks fail to reveal is that their author had made common cause with paleobiologists long ago. After all, his own position near the head of evolution’s high table primarily derived from his use of game theoretic models to study evolutionary dynamics. Moreover, the very first issue of Paleobiology contained a highly favorable review of Maynard Smith’s Models in Ecology [5], a book whose title references a programmatic volume that Thomas J.M. Schopf had edited just a few years before, Models in Paleobiology. In the final analysis, paleontology’s escape from obscurity was thus about far more than convincing evolutionists to care about the fossil record. Its success sheds light on a much larger revolution in what it meant to reason about the natural world.

References

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