

DESCENDED FROM DARWIN  
INSIGHTS INTO THE HISTORY OF  
EVOLUTIONARY STUDIES, 1900–1970

Joe Cain and Michael Ruse, Editors

American Philosophical Society  
Philadelphia • 2009

TRANSACTIONS  
of the  
AMERICAN PHILOSOPHICAL SOCIETY  
Held at Philadelphia  
For Promoting Useful Knowledge  
Volume 99, Part 1

Copyright © 2009 by the American Philosophical Society for its Transactions series, Volume 99. All rights reserved.

ISBN: 978-1-60618-991-7

US ISSN: 0065-9746

Library of Congress Cataloging-in-Publication Data is available from the Library of Congress.

## CHAPTER 9

---

### THE “DELAYED SYNTHESIS”

#### PALEOBIOLOGY IN THE 1970S

*David Sepkoski*

In the period between 1970 and 1985, the discipline of paleontology underwent a major transformation. Despite its important role in the emergence of the theory of evolution in the nineteenth century, paleontology was largely marginalized by the evolutionary community during much of the twentieth century. The reasons for this are complicated, but in general they stem from the fact that genetically minded biologists, in particular, perceived paleontology to be incapable of employing the kinds of methods and evidence necessary to contribute to evolutionary theory. As a result, genetics emerged in the mid-twentieth century as the central discipline in evolutionary studies, a transition that was cemented by the advent of the modern synthesis.

Beginning in the 1970s, however, paleontologists began to assert their independence and to make strong claims about the centrality of paleontology within evolutionary biology. Between 1970 and 1985, a number of theories were advanced that argued the fossil record—the traditional domain of paleontologists—had genuine significance for understanding the mechanisms by which evolution happens. An influential group of scientists, including Stephen Jay Gould, Thomas Schopf, and David Raup, began to agitate both for the central importance of paleontology and for paleontologists to revise the goals and agenda of their discipline. They focused particularly on developing quantitative techniques using new technologies, such as mainframe computers, in order to model macroevolutionary patterns and extinction dynamics. This drew attention to the importance of the fossil record as a source for evolutionary theory, particularly by borrowing and incorporating methods used by ecologists and population biologists (e.g., logistic equations, biogeographical models, etc.). The name for this new subdiscipline that the architects of this transformation settled on was “paleobiology,” which reflected not only their assumption that paleontology ought to be more biologically oriented but also their deliberate attempt

to situate their discipline explicitly within the broader field of evolutionary biology. Many of the scientists involved in this work have labeled the period a “revolution,” and indeed as early as 1971 Gould confidently predicted “there’s a revolution going on in ecology and biogeography. . . . The next great innovator in paleoecology will be the man who successfully learns to understand this revolution and transfer its insights into paleontology” (Gould to Sepkoski, April 28, 1971. Sepkoski Papers.) I argue that this recent development was, in effect, the delayed “completion” of the modern synthesis begun 40 years earlier.

Calling this transformation a “completion,” rather than a “rejection,” an “alternative,” or a “replacement” is appropriate, because it most accurately captures the motivations of the proponents of the new paleobiology. Indeed, in looking back on this history, Gould has consistently referred to his motivation as a desire to *amplify*, not replace, the synthetic view of evolution. In his final major work, *The Structure of Evolutionary Theory*, Gould wrote that his lifelong goal had been “to expand and alter the premises of Darwinism, in order to build an enlarged and distinctive evolutionary theory that, while remaining in the tradition, and under the logic, of Darwinian argument, can also explain a wide range of macroevolutionary phenomena lying outside the explanatory power of extrapolated modes and mechanisms of microevolution” (Gould, 2002, p. 1339). I argue that the entire paleobiological movement was indeed “in the tradition” of synthetic evolutionary theory, despite any “alteration” of premises about the importance and roles of macroevolutionary and microevolutionary processes. The framers of the modern synthesis explicitly crafted an important role for paleontology; this was, after all, why the predecessor to the Society for the Study of Evolution was called the Committee on Common Problems in Genetics, Paleontology, and Systematics. In many respects, however, that promise of inclusivity was unfulfilled, and paleontologists were excluded from the evolutionary “high table.” When John Maynard Smith commented in a 1984 *Nature* essay, “the paleontologists have too long been missing from the high table. Welcome back,” he was acknowledging a correction, not a replacement, in the disciplinary organization of evolutionary biology (Maynard Smith, 1984, p. 402).

Building on an extensive literature in the history of science dealing with the importance of journals and learned societies for creating disciplinary identity in the sciences, this essay examines the intersection between paleontologists’ intellectual motivations, their political agenda, and their practical obstacles. Many of the general points that I make about the role of journals in institutionalizing ideas echo David Hull’s comments about the social processes of scientific argument and consensus.<sup>1</sup> In *Science as a Process*, Hull described the development of cladistics as a conceptual approach to systematics through its institutional establishment in the Society of Systematic Zoology and its journal *Systematic Zoology*. One of his central arguments is that the content of scientific debate cannot be separated from the mechanisms that support and disseminate scientific research—and that these mechanisms are often dependent on the idiosyncratic tastes of individuals, such as journal editors and referees. This was equally true in the case of the establishment of paleobiology as a discipline, and the establishment of the journal *Paleobiology*—one of the movement’s earliest successes—serves as a microcosm for examining the development of a new identity for paleontology.

THE ORIGIN OF *Paleobiology*

Despite the delay, there are a number of extremely intriguing similarities between the events of the 1940s and the delayed paleobiological response in the early 1970s. In order to highlight those similarities, I focus on institutional aspects of the transformation of paleobiology, and particularly on the establishment of a new journal—*Paleobiology*—which served as an outlet for the new agenda in paleontology. Betty Smocovitis and Joe Cain have both shown the importance of a central “discipline builder” or “community architect” in effecting the institutional transformations necessary for the promotion of the intellectual agenda of the modern synthesis (Cain, 1992, 1993, 2002; Smocovitis, 1992, 1994, 1996). In the case of the modern synthesis it was Ernst Mayr who, according to Smocovitis, “played the roles of historian, philosopher, organizer, and general promoter of evolutionary biology” (Smocovitis, 1994, p. 1). Mayr kicked off this agenda by organizing, with Dobzhansky, the special symposium titled “Speciation” at the 1939 AAAS meeting, which preceded the founding of the Society for Study of Speciation. While that organization was short-lived, it was succeeded by the important Committee on Common Problems in Genetics, Paleontology, and Systematics (CCP), whose goal was to unify practices in related fields of evolutionary study. In 1946, an AAAS meeting produced the initiative to found a new journal—*Evolution*—to represent this new orientation.

The circumstances surrounding the establishment of the journal *Paleobiology* and the paleobiological agenda were strikingly similar: In 1969, Tom Schopf—then a young Assistant Professor at the University of Chicago—began planning a special session on “Models in Paleontology” for the upcoming Geological Society of America meeting. With support from James Valentine (who would become President of the Paleontological Society [PS] in 1974), Schopf lined up a group of speakers populated mostly by younger, like-thinking paleontologists (such as Gould, Raup, and Tony Hallam), as well as evolutionary biologists (Michael Ghiselin and E. O. Wilson were approached; Ghiselin accepted, while Wilson begged off, but suggested Daniel Simberloff instead). In a letter soliciting participation, Schopf described the symposium as an opportunity “to identify and evaluate the theoretical models which are guiding (by accident or design) the development of various parts of our science,” since “the theoretical framework . . . dictates where one looks [in empirical data] and how one goes about the descriptive process . . . in this way we can encourage the analytical ‘problem oriented’ approach in Paleontology” (Schopf to Raup, March 7, 1970. Schopf Papers, Box 3, Folder 30). The symposium took place at the GSA meeting in 1971, and the papers were collected and published the next year in the now moderately famous volume *Models in Paleobiology*, which Schopf edited (Schopf, 1972). The publication history is an interesting story in itself, but I will note here only that while the symposium and book unveiled Gould and Niles Eldredge’s theory of Punctuated Equilibria, the inclusion of this particular theory nearly did not happen: when initially approached to give a paper on “models in speciation,” Gould replied with hesitation that “you have given me a topic that ranks only third on your list in terms of my competence [behind models in morphology and phylogeny].” Nonetheless he agreed, since the other slots were already full, but he suggested a joint effort with Eldredge, whom he described as “our best new thinker” (Gould to Schopf, March 13, 1970. Schopf Papers, Box 5, Folder 14).

The main task following the GSA symposium concerned maintaining the momentum Schopf had begun to engineer. During the symposium Schopf and Gould discussed the possibility of forming an informal research group to plot an agenda for pushing theory in paleontological research, and in the summer of 1972 Schopf invited Gould, Raup, and Simberloff to spend a few days together at Woods Hole, where Schopf had an ongoing part-time research appointment. It is clear that Schopf had in mind similar arrangements among biologists as a model (though not that he was thinking specifically of the example of CCP). As he wrote to Raup,

Of course one can never “program” good research, and in any event research is always done by individuals and not teams, yet the self-conscious attempt to introduce more theory into our mass of facts might be a very useful thing to do. I have heard about similar affairs working well; perhaps the most spectacular were the various outcomes when Lewontin, Ed Wilson, McArthur, Levins and Egbert Leigh met for a week or so over a couple of summers at McArthur’s New Hampshire farm in the early ’60s. (Schopf to Raup, March 5, 1972, Schopf Papers, Box 3, Folder 30)

In another letter, Schopf noted that two “prominent schools” in paleontological thought were defining themselves: one which took “a broad view of the fossil record,” but whose members (including Valentine, Norman Bretsky, and James Stehli) “rarely consider the significance of their data,” and a second group, “philosophically opposed to this school, but not as cohesively united,” who treat extinction and diversity problems as “much more complicated” and who are willing to think theoretically about speciation (Schopf to Raup, April 16, 1972. Schopf Papers, Box 3, Folder 30). Schopf located himself and Raup within this latter school, and expressed the hope that their informal meeting would define the unifying principles of their agenda more clearly.

In preparation for the Woods Hole meeting, Schopf circulated an agenda that outlined the major problems to be addressed. He noted that “invertebrate paleontology is now characterized by a strong emphasis on facts of history, often stressing unique events,” which “has led to historical models where the ideas are closely tied to empirical summaries” (Schopf to Raup, Gould, and Simberloff, April 20, 1972. Schopf Papers, Box 3, Folder 30). It was particularly the practice of treating historical models as “empirical summaries” he wished to target; in his mind, models could be much more. Schopf proposed “examin[ing] these and other patterns in terms of equilibrium models” as a strategy, noting that “this approach has worked extremely well in a field very similar to ours, i.e., biogeography, with the species equilibrium.” He suggested that a central goal should be to understand “the processes underlying [the] patterns” of diversity, morphology, and phylogeny through time, and to ask “what are their long-term equilibrium consequences.” In outlining the processes to investigate, Schopf made the interdependence of paleontology and biology clear: “1) speciation theory, including population genetics and the species equilibrium”; “2) the constraints imposed by size, shape and habitat on organized protoplasm”; “3) the unity (or disunity) of biochemical pathways, including models of reproduction”; and “4) is there an equilibrium model of phylogenetic development?”<sup>2</sup>

The meeting went off as planned, and the outcome was the groundbreaking joint-authored paper titled “Stochastic Models of Phylogeny and the Evolution of Diversity,” published in the *Journal of Geology* the following year. Essentially, this paper

proposed that by generating random phylogenetic trees (using a computer program), one could test whether these would replicate certain aspects of actual phylogenies—thus demonstrating whether actual patterns of origination and extinction had stochastic variables (Raup, Gould, Schopf, & Simberloff, 1973). Despite the apparent success of the venture, a year later Schopf was feeling gloomy. He wrote Gould, Raup, and Simberloff “to confess varying degrees of uneasiness [*sic*] about trying to hold our loose arrangement together,” because each member seemed to have independent research interests that were not immediately compatible (Schopf to Gould, Raup, and Simberloff, September 28, 1973. Schopf Papers, Box 3, Folder 30). His motivation had not been to create a permanent research team, but rather by example to promote “the romantic, idealistic notion that Paleontology, rich in evidence, weak in theory, strong on history, nearly devoid of equilibrium models—that paleontology could be redirected by the self-conscious application of a way of science not found previously in paleontology.” The specific problem he noted was that whereas he had “thought our relative strengths could be dedicated to a higher purpose which would result in a quantum increase in understanding the biological meaning of the fossil record,” it was unclear how next to proceed. Some members of the group (Gould in particular) favored a continuing series of “4 authored papers” to “establish a specific point of view which will provide a focal point for our distinctive approach.” Others, however, were concerned that not all members could contribute equally to each project, and “without a face-to-face meeting of the 4 of us again to reestablish possibilities for mutual input,” the success of the collaborative effort on the stochastic models paper would be difficult to reproduce.

“Where does this leave us,” Schopf wondered. He expressed the hope that the brief collaboration would be noticed, since “I cannot help but think that this approach will revolutionize the fundamental questions in paleontology.” But he also recognized that some kind of continued institutional support would be necessary to propel the movement. Here Schopf pointed to the historical example of the collaboration between Max Delbrück, Salvador Luria, and Alfred Hershey, which produced innovation in molecular biology through informal meetings at Cold Spring Harbor. “We could well follow that example,” Schopf wrote, “and offer a joint course or workshop stressing stochastic processes, and equilibrium theory, in the context of the fossil record in particular, and evolutionary biology in general.” The rest of the group was reasonably receptive to this idea, and indeed the group (which Simberloff had given the tongue-in-cheek nickname “Radical Fringe in Paleontology”) met one more time at Woods Hole, in December 1973. As later correspondence shows, however, this original collaboration would not stand up as a formal arrangement—in part because of practical considerations and in part because of philosophical disagreements.<sup>3</sup> In any event, shortly after his pessimistic letter, Schopf had found a new avenue for promoting his vision far more accommodating toward the diversity of viewpoints within the group: at some point between September and December of 1973, he decided to found a journal.

### ORGANIZING THE JOURNAL

Throughout the 1960s and early 1970s, major works in evolutionary theory were not published in the standard paleontological journals—such as *Journal of Paleontology* (*JP*). To the extent that paleontologists published work that had theoretical and

multidisciplinary implications, those papers appeared in journals read by geneticists and population biologists—publications like *Systematic Zoology* and *Evolution*.<sup>4</sup> While quantitative paleobiology was in its infancy during the late 1960s and early '70s, many of its methods were adapted from the much more established techniques developed in the field of systematic biology during the preceding few decades. As Joel Hagen has shown, a “statistical frame of mind” emerged among biologists after World War II that took advantage of advances in computing technology that grew out of the postwar era (Hagen, 2003). In particular, two important books promoted quantitative techniques in this field: George Gaylord Simpson and Anne Roe’s *Quantitative Zoology*, which was first published in 1939 and later revised and updated by Richard Lewontin in 1960, and Robert Sokal and F. James Rohlf’s *Biometry*, which appeared in 1969 (Simpson, Roe, & Lewontin, 1960; Sokal & Rohlf, 1969). While Simpson and Roe’s original volume was relatively simplistic by the standards of Sokal and Rohlf’s more advanced text, both works were extremely important for pointing the discipline in a more statistical direction and for demonstrating how quantitative analysis could help solve biological problems—such as population dynamics—that were too complex or had too many variables to study empirically. At the same time, journals like *Systematic Zoology* began to publish papers that were quantitative in orientation, making quantitative ideas in systematics accessible to a wider audience.<sup>5</sup> Nonetheless, some paleontologists were not content to see the occasional theoretical paper published in the journal of another discipline, and wanted to have an outlet for new paleobiological work that was by and for paleontologists.

The documentary evidence surrounding this event unfortunately does not reveal the specific impetus for the idea of a new journal. Paleontological Society (PS) reports show that the council was engaged in an active reevaluation of the Society’s goals and commitments between 1972 and 1973, which included efforts to consolidate interest and membership.<sup>6</sup> One stated goal of the council was “the obligation to promote the cause of paleobiology,” possibly by establishing a separate institute. This recognized membership interest in “cross-fertilization and exposure time between biologists and paleontologists.” Accordingly, the council recommended that membership be polled regarding interest in establishing a new journal by the PS, which would both promote work different from the “mostly descriptive articles with very restricted interest” currently published in *Journal of Paleontology*, and also attract readership and submission from workers in the biological sciences.<sup>7</sup> Interestingly, an earlier PS committee had considered the very same proposal, which never got off the ground. David Raup recalls,

Some time along there I chaired a committee in the paleontological society to look into long-range plans . . . and one of the things we considered pretty thoroughly was whether to start a journal like Paleobiology, and we unanimously recommended [stopping this] . . . because we were afraid that to make it distinct from the Journal of Paleontology would be to lead to more strife . . . but Tom didn’t accept that—he didn’t accept anything that he didn’t agree with.<sup>8</sup>

Schopf ultimately got his way, and it is attractive to speculate about possible reasons: Schopf was a confident young voice in the field (having recently moved to the University of Chicago and gained tenure), he had just organized a symposium which

had attracted significant attention, he had sympathy (and formal connections through Woods Hole) with biologists, and, as his letters show, he was at that very moment casting about for a way of advancing the agenda of theoretical paleontology. To this list—as personal motivation for taking on the task, perhaps—we can add Schopf’s own appreciation for the recent history of science: though he does not mention it, is it possible that Schopf had the example of Mayr in mind (who was one of his biologist role-models), who had performed a similar (and similarly heroic) feat of disciplinary engineering 30 years earlier with the establishment of *Evolution*?

At any rate, in late 1973, Schopf wrote a “Draft Report” on the feasibility of a new journal for circulation within the membership of the Paleontological Society. Schopf had been selected by the PS council for this task, and James Valentine, then President-elect of the Society, sent Schopf a letter communicating PS support.<sup>9</sup> While voicing enthusiasm, Valentine also struck a note of caution: “This represents a major undertaking for the PS, of course, and we need to evaluate the whole proposition accurately before acting. A goof could break the society.” Specifically, Valentine was concerned that general PS membership was more conservative than the council, and emphasized that it would have to be “sold” on the necessity for a new journal. He outlined several points he felt were important in selling the idea. One prominent issue was the fact that the PS itself actually did not at the time own a journal: *Journal of Paleontology* was owned not by the PS but by the more conservative Society of Economic Paleontologists and Mineralogists (which is now called Society for Sedimentary Geology, though it kept the acronym, SEPM). Another was economic viability—Valentine estimated that a subscription list of 1,500 would cover expenses, particularly if a large number of subscribers were institutions paying the higher rate (of \$18). The major point of contention was that membership might view the new journal as competition or replacement for *JP*, and Valentine urged Schopf to address these concerns: “The *JP* should indeed continue; the new journal would supplement, not compete. Indeed, the new journal would provide an ideal outlet for summary statements or salient biological conclusions reached in taxonomic and biostratigraphic studies, which might be in *JP* or in a monographic series.” To address these issues Valentine prepared a referendum for insertion in an issue of *JP* including a short description of the journal and a ballot. Valentine noted, “I should expect your main problems of rating feasibility will be (1) how much it will cost; (2) can we get the subscribers; and (3) can we get the manuscripts. The last question is of course linked to problems of format and content. I imagine that a first issue with papers by such people as George Simpson, Th. Dobzhansky, Ernst Mayr and the like—in fact a first year with a heavy scattering of eminent biologists—would help solve both problems.” Valentine closed by resolving that the matter would be taken up at the PS council meeting in San Antonio the next year.

Schopf responded positively to Valentine’s suggestions, noting only that a new class of subscribers might be considered at a different rate: those “who took just the new journal” without being PS members, since “this will add biologists” (Schopf to Valentine, February 4, 1974. Paleobiology Editorial Papers, Box 14). In a letter to his Chicago colleague Ralph Johnson (who would join Schopf as founding coeditor of the journal), Schopf discussed possibilities for the journal’s editorial board, noting, “I think the editorial board should be chiefly younger workers who represent the changing interests in our profession” (Schopf to Johnson, December 17, 1973.

Paleobiology Editorial Papers, Box 14). Johnson's reply, which he attached as handwritten notes to Schopf's letter, reveals many of the concerns of more established PS membership. Regarding the perceived conflict between *JP* and the new journal, he wondered whether *JP* would now "limit itself to papers relating to geological studies of the fossil record? Is systematics and phylogeny geo or bio?" He also expressed concern that "the new journal will begin well and then in several years starve for good papers," noting that *JP* had, in the past, also "attempted to attract ms. of broader interest but without success."

Schopf fared fairly well with personal queries sent to prospective members of the journal's initial editorial board, but responses did reflect reservations. One generally positive response from a prominent paleontologist voiced concern that the new journal "may encourage speculation that is not firmly based on geological and paleontological documentation" (Richard E. Grant to Schopf, January 4, 1974. Paleobiology Editorial Papers, Box 13). This, he noted, could have unfortunate effects: "One will be to foster the notion of a first-class journal that deals in great unifying principles, and a second-class journal of hackwork [i.e., *JP*]." He did, however, note that "mega-thinking is 'in' now," and ultimately voiced his support. Steven Stanley questioned Schopf's comparison between the new journal and the established journal *Genetics*, noting that "the field of genetics has been booming since the turn of the century," while "the current renaissance of paleontology is opening up entirely new avenues of research and, in my view, much garbage is floating about" (Steven M. Stanley to Schopf, February 21, 1974. Paleobiology Editorial Papers, Box 14). Nonetheless, he too accepted appointment on the editorial board. These sentiments were fairly typical: another member wrote to express support for the journal while at the same time cautioning that a new journal might widen the gap between geologists and biostratigraphers on the one hand, and paleobiologists on the other. He closed his letter with the suggestion that "perhaps instead of founding a new journal, we need to do an education job. Don't *reduce* the paleobiological coverage in *JP* Instead, *increase* it. Show the neontologists that *JP* is worthy of their consideration (and, hence, subscription). And, show the paleobiologiphobes that they, too, need the very paleobiology they despise—that they really can't do without it" (R. A. Davis to Schopf, March 11, 1974. Paleobiology Editorial Papers, Box 14).

Among the supportive responses Schopf received, a surprisingly mixed reply came from Niles Eldredge (Eldredge to Schopf, February 1, 1974. Paleobiology Editorial Papers, Box 14). While stating that he of course wanted "to see our science become more enmeshed with theoretical organismic biology," he noted that contributions of paleontologists to biology had been largely ignored by biologists, and wondered whether a new journal would actually reverse the trend. Specifically, he cautioned that "any increase in the literature is a glut," since "our existing journals are already publishing a large quantity of crap, mixed in with worthwhile things." Eldredge expressed doubts that there would be a sufficient quantity of quality manuscripts to fill the new journal, and feared that the new journal would inevitably suffer from too few papers with "potentially theoretically interesting implications" and be forced to rely on "conventional descriptive" papers in traditional systematics. So instead of taking a leadership role in this process, Eldredge simply deferred the question to the general membership of the PS: "If they are for it, I am."

In short, objections to the new journal can be sorted into several major categories:

1. Concerns that the new journal would upset the discipline, by emphasizing the rift between traditional geologically oriented paleontologists and paleobiologists
2. A related concern that the new journal would siphon manuscripts and readers away from *JP*, thus weakening the discipline's flagship journal (which had been on shaky financial ground for some years)
3. Worry that the journal would attract attention away from traditional biostratigraphy and taxonomy, which were viewed as essential foundations for the new approach
4. Objections to Schopf's rather narrow definition of paleobiology as excluding related fields like biogeology and biochemistry
5. A fear that a journal oriented towards theoretical work would have difficulty finding papers of sufficient quality
6. Practical concerns that the cost of a new journal would place a financial burden on the PS and its members, and that the journal would attract insufficient interest from neontologists to meet subscription goals

In response, in early February, 1974, Schopf put together a "Draft Report on the Format and Feasibility of a New Paleontological Society Journal," which was explicitly intended "for distribution to members of the Council and proposed members of the Editorial Board" of the new journal.<sup>10</sup> This document provided a detailed summary and analysis of the factors to be considered by the council, and of the rationale for the journal. It included sections on competing publications, journal specifications, costs and subscription requirements, editorial policy and board structure, and general recommendations. On the subject of existing literature, the report noted that "2–3 times the number of suitable articles required to support *Paleobiology* [as the new journal was now being named] were published" during the preceding year, spread out over more than 23 journals. It also made the optimistic prediction that this number would grow, due to the increase in graduate training in paleobiology and the increasing numbers of biologists willing to consider paleontological topics.<sup>11</sup> The top five outlets for paleobiological work had been the journals *Lethaia*, *J. Foraminiferal Research*, *Evolution*, *Paleontology*, and *Journal of Paleontology*. Interestingly, among these journals the calculations for "approximate percentage of volume taken up by material suitable for *Paleobiology* showed *Lethaia* as the clear leader with 56%, while *JP* lagged at only 18% (compared with 21% for *Evolution*). This was presented as significant ammunition against the claim that *JP* would suffer greatly from the establishment of a new journal. It was also argued that consolidating most paleobiological work in a single journal would attract additional readership and submissions from biologists who did not normally read the other publications.

Outside of financial considerations, the other major area of consideration in the report was editorial policy and structure. The stated editorial policy was as follows:

We seek in *Paleobiology* a broad spectrum of paleobiological thought in both paleobotany and paleozoology, including studies of invertebrates and vertebrates, from both neontologists and paleontologists. We envision that most volumes will deal with biological or paleobiological aspects of morphology, biogeochemistry (organic and inorganic), populations, faunal provinces, communities and ecosystems. Emphasis should be on biological or paleobiological processes. This includes speciation, extinction, development of individuals or of colonies, natural selection, evolution, or patterns of variation,

abundance and distribution, in space and time. Historical analysis of paleobiological themes is also welcome.<sup>12</sup>

This section went on to specify qualifications for members of the editorial board, which naturally included “sympathy” with this philosophical orientation of the journal, but also “best research years ahead of them instead of past them,” highlighting Schopf’s desire that *Paleobiology* be an outlet for young, creative iconoclasts like himself. The suggested initial board reflected this desire: while the more senior Ralph Johnson was tapped as editor, members of the board included Bill Schopf (Tom’s brother), Dan Simberloff, Jim Hopson, Dick Bambach, Karl Flessa, Steve Gould, Dave Raup, Steve Stanley, and of course Schopf himself. Among this group, Schopf, Raup, Gould, and Simberloff were the core members of the “radical fringe” in paleontology who had met, the previous year, to hash out a new approach to macroevolution using quantitative methods—such as stochastic simulation of phylogenies—that promised to transform paleontology from (in Gould’s and Raup’s words) an “ideographic” discipline to a “nomothetic” one. The report closed with a general recommendation to the council that highlighted the issues of most likely appeal to PS membership: the journal would fill a gap in literature and was financially feasible; it would belong solely to the PS and would represent independence from SEPM; it would broaden PS membership by appealing to biologists. Overall, “*Paleobiology* would establish a new image for the paleontological society in a way that could never be done by simply enlarging the *Journal of Paleontology*. *Paleobiology* should be the focal point for ecologists, evolutionary biologists, and other neontologists who are interested in the history of life.”<sup>13</sup>

### PALEOBIOLOGY IS BORN

In March of 1974 a letter was mailed to PS members advising them of the Society’s intent to start a new journal, which included a ballot indicating either favorable or unfavorable response to this proposal. A draft version of this letter made the case by citing a number of familiar factors: 1) the need for the PS to have its own journal autonomous from SEPM; 2) dismissing the concern that *Paleobiology* would weaken *Journal of Paleontology*; 3) citing the frequency with which paleobiological work was currently published in other journals; 4) emphasizing the financial viability of the venture.<sup>14</sup> This memorandum noted that while there was an increased interest on the part of paleontologists to communicate their research with biologists, such rapprochement was hampered by the lack of a biologically oriented paleontological journal that both paleontologists and biologists would want to read. Therefore, “The proposed journal *Paleobiology* is designed to complement the *Journal of Paleontology* by meeting the expanding biological interests of our profession.” The statement went on to explain that “this expansion is partly due to young paleontologists who are increasing[ly] well-trained biologically,” and promised that “contributions from paleontologists on matters of biological interest will be joined by contributions from bioscientists on matters of paleontological interest.” The letter concluded with the “hope that *Paleobiology* would penetrate into biology libraries and into the hands of bioscientists, leading to an expansion of the dialogue that is now established between paleontology and relevant biological fields.”

As reported following the April 1 council meeting in San Antonio, more than half of all members responded, with the overwhelming majority supporting the venture (81% to 17%—861 to 142).<sup>15</sup> Additionally, an encouraging 544 members indicated on their ballots that they would subscribe to the new journal. Many members wrote comments on their ballots—most reflecting the concerns expressed by council members privately to Schopf. Some individual responses are, however, worth noting.<sup>16</sup> One member stated that “the paleobiology bandwagon is for the most part embarrassingly far from where modern biology is at,” and claimed “the best way to communicate with biologists is to publish in biological journals.” Another labeled the proposal “a dangerous dilution of effort,” one member called the letter itself “offensive,” and still others complained about the proposed \$8 annual subscription fee. Several ballots voted against the proposal, but nonetheless stated that the member would subscribe grudgingly, often citing doubts that the journal would truly attract the attention of biologists. On the positive side, one member (Gould) enthusiastically remarked “as Jerry Rubin said—Do It!” another spoke for “young people” who otherwise would “publish innovative work elsewhere,” while another supportive response wondered whether *Paleobiology* would become the “journal for *real* Paleontology” while the existing *JP* would become “a Journal of Paleontological Trivia.” Finally, one of the more sociologically interesting responses came from a female member who supported the proposal: “I think it will be good for the paleontologists. I don’t know what one can *do* with biologists! They seem to have exactly the same opinion of paleontologists that the Victorians did of women—i.e., nothing written by such a low form of life could possibly be taken seriously.”

Acting on this information, on April 1, 1974, the PS council voted 6 to 1 in favor of establishing the journal *Paleobiology* with a first issue planned for the following year. At this meeting it was also decided that Schopf would join Ralph Johnson as founding coeditor, both of whom would serve for an initial three-year term.<sup>17</sup> Despite the apparently clear sentiments of Society membership, two local branches of the PS—the Pacific and North Central sections—objected to the vote, and requested that formal ballots be mailed to membership asking not for *support* for the journal, but rather for *approval*. While he noted the danger in delaying momentum for the new journal, PS President James Valentine acceded, and ballots were duly mailed. When the returns came in membership was still in favor—this time by 78% majority (491 of 632 received). At the November 17 meeting of the PS council in Miami Beach, the initiative to publish *Paleobiology* was therefore officially ratified.<sup>18</sup>

Meanwhile, in April of 1974 Schopf and Johnson wrote to Valentine to formally accept coeditorship for an initial three-year period (Schopf and Johnson to Valentine, April 5, 1974. Paleobiology Editorial Papers, Box 14). In their acceptance, they noted that duties for the first year would be divided between the two: Schopf would “mainly be concerned with setting up the business and printing arrangements,” including “obtaining a printer, designing the format of the Journal, obtaining stationery and printing forms, soliciting advertisers and subscriptions, and handling all communications with the Society.” Johnson, on the other hand, would handle “editorial arrangements,” including “soliciting manuscripts and reviewers, and processing manuscripts for the printer.” This division of labor seems to have favored the personalities of the two editors, and in particular Schopf’s mercurial tendencies were well-suited for the onerous task of contacting libraries and individuals with subscription solicitations.

A month after accepting the task, Schopf wrote the PS council to update his progress. He reported “fighting the war on three fronts,” dealing simultaneously with the tasks of soliciting manuscripts, obtaining subscriptions, and finding a printer (Schopf to Paleontological Society Council, May 16, 1974. Paleobiology Editorial Papers, Box 14). Schopf was optimistic, but it is clear from this letter that the editors had a long way to go: with little more than 7 months before planned publication of the first issue of *Paleobiology*, many details had yet to be secured. The journal had received no manuscripts, although roughly 70 letters had been sent to individual scientists requesting submissions. The choice of printers had been narrowed to two contenders, but no final decision had been made. And the crucial issue of subscriptions still hung in the balance: here the lengths Schopf had gone to assure the success of the journal were most evident. As Gould recalled in his 1984 obituary for Schopf, Schopf had been content with doing this “spadework” for the journal:

I walked into his office one night in Woods Hole, and Tom was in the midst of writing 250 letters *by hand* to libraries that had not subscribed (he knew how much *Paleobiology* needed an institutional basis of support). When I asked why he simply didn't Xerox a single letter, and save himself countless hours of backbreaking, boring, hand-cramping work, he replied that a personal note might get more attention from harried librarians. I do not know whether this ploy worked, but need I say more to illustrate his dedication? (Gould, 1984, p. 283)

In fact, Schopf reported he had written over 350 such letters, and he promised to follow up with the institutions that did not reply. He also outlined an ambitious plan to advertise *Paleobiology* to the widest audience possible: (a) the 1400 members of the PS would be contacted via the next newsletter; (b) he would ensure that sibling societies (like the British Paleontological Association, the Geological Society of America, and the Paleobotanical Section of the Botanical Society of America) printed the announcement in mailings to membership; and (c) related biological societies would be selectively targeted. This third group was critical, since without support and interest from biologists, *Paleobiology* would not achieve its mission. Accordingly, Schopf planned to obtain the mailing lists for three societies—the Society of Systematic Zoologists, the American Society of Naturalists, and the Society for the Study of Evolution—which published three of the most important journals in ecology and evolutionary studies: *Systematic Zoology*, *Evolution*, and *American Naturalist*. In a follow-up the next year, he updated the council about his initial success in obtaining subscriptions. He reported having sent more than 600 letters to paleontologists and geologists in academic departments; more than 2500 advertisements had been sent to biologists; more than 500 solicitations were mailed to members of the British Palaeontological Association; and because “we had some envelopes and ads left over,” an additional 577 nonsubscribing members of the Society for Vertebrate Paleontology were contacted for a second time (Schopf to Oliver, May 8, 1975. Paleobiology Editorial Papers, Box 14). The total subscribers (1,264) included more than a thousand individual subscriptions—which met his expectations—but Schopf was somewhat disappointed with the number of institutional subscriptions. He addressed this issue with repeated mailings to institutional librarians over the next two years, and by 1977 had reached 2,000 total subscribers, including roughly 500 institutions.<sup>19</sup>

## THE FIRST YEARS

The first issue of *Paleobiology* was published in March of 1975. Among the authors represented were some of the leaders in the new biological approach to paleontology, including Gould, Raup, Stanley, Richard Bambach, Karl Flessa, John Cisne, and Schopf himself. In fact, several of these authors contributed to multiple pieces: Gould led the way, coauthoring two papers and penning a review, but Raup followed just behind, with a coauthored paper and another paper of his own. The original MBL group of Schopf, Raup, Gould, and Simberloff produced a spin-off of their initial project titled "Genomic versus Morphological Rates of Evolution: Influence of Morphologic Complexity," which compared "real" rates of morphologic complexity in the fossil record with simulated phylogenies (Schopf, Raup, Gould, & Simberloff, 1975). The most important paper in the first issue, though, was probably Raup's "Taxonomic Survivorship Curves and Van Valen's Law" (Raup, 1975). This paper modified Chicago paleontologist Leigh Van Valen's mathematical model of survivorship to estimate the rate of extinction among extinct and living taxa as a single survivorship curve. The study is significant not just for its technical accomplishment, but also because it marks the beginning of Raup's serious interest in modeling extinction, which would continue through a number of important papers in the 1970s and culminate in the early 1980s with his collaboration with Jack Sepkoski on the theory of "periodicity" in extinction.<sup>20</sup>

The editorial policy Schopf had outlined was also clear in the first issue. In the first place, the selection of papers represented both an orientation toward biologically significant findings in paleontology and a preference for statistical, mathematically sophisticated models. Raup's paper champions Van Valen's work "because it is a major step towards a nomothetic paleontology . . . toward interpreting the evolutionary record in terms of general rules and processes without regard to specific causes . . . it attempts to make generalizations about the fossil record which are not simply enumerations of specific events and causes" (Raup, 1975, p. 83). This is precisely the sentiment Schopf used to promote *Paleobiology's* distinctiveness: the journal would eschew traditional taxonomic and stratigraphic studies in favor of broad-minded analysis of patterns in the fossil record. Schopf, in fact, wrote an introductory editorial touting these values for the first issue of the journal, which was never published but is worth quoting at length:

Rarely does a field of science witness a change in emphasis and direction as severe as has paleontology in the past few years. The general goal remains the same—to determine and to causally explain the patterns of the fossil record. But the scope of approaches now employed involves instruments and methods hardly considered 5 years ago, and in some cases not invented a decade ago. Together with the change in day-to-day activities of paleontologists, the conceptual models used to suggest experiments have lost this special pleading for unique historical conditions. Our practical recognition of this change in paleontology can be seen in our editorial board which includes neontologists and paleontologists. . . . To conclude: biological paleontology has long stood in the shadow of its great counter-part, stratigraphic paleontology, awaiting for the integration of biological principles with the history of life. *Paleobiology* is a vehicle for publication for those seeking to explore the biological implications of the fossil record.<sup>21</sup>

The editorial was likely not published because it was recognized to be overkill—Schopf had, after all, won his battle—but it demonstrates the degree to which the journal had become part of Schopf’s distinctive vision. Ironically, only the previous year Schopf had written to a colleague that he “did not propose this venture to the Paleontological Society, nor am I strongly wed to the idea”; obviously the project had grown on him considerably since that time (Schopf to Francis Stehli, January 3, 1974. Paleobiology Editorial Papers, Box 14).

In its first few years, *Paleobiology* was, without question, an unqualified success. During the initial period of the journal’s existence, the editors received a number of testimonials from colleagues congratulating them on the journal’s success. In 1975, at the end of *Paleobiology*’s first year, Valentine sent Schopf a letter reporting on the journal’s reception in England (where Valentine was spending a sabbatical). He noted that while some paleontologists found the content “over their heads,” students had “reacted quite favorably, especially in departments where they are exposed to some biological aspects of the [fossil] record.” He concluded that “the journal is pumping badly needed biological light into paleo,” and he was “tickled” by its success (Valentine to Schopf, November 18, 1975. Paleobiology Editorial Papers, Box 14). During this time support came in not only from paleontologists and geologists but also from biologists. Everett Olson was sufficiently impressed to send a contribution to the journal’s Patron’s Fund, and called *Paleobiology* “a very fine journal . . . much the best of its type and in many ways unique” (Olson to Schopf, January 4, 1979. Paleobiology Editorial Papers, Box 14). And Stephen Wainright, a prominent biologist at Duke University, was unequivocal in his praise for the journal, surprising the editors with the following note:

Because I am stimulated by the directions that are being taken by many biologists today in weaving ideas and information of the extant and the extinct into evolutionary cloth; and because the best of this material has/is appeared/ing in *Paleobiology*; and because I believe the next 20 years in biology will be dominated in the matter of synthesizing information and ideas by paleobiologists and their “like”; I wish to make the enclosed contribution to the *Paleobiology* Patron’s Fund. You have my joy and respect. (Wainright to Editors, May 6, 1981. Paleobiology Editorial Papers, Box 14)

Financially, the journal was also a success. The goal for the first year was to obtain 1,700 subscribers, and while the initial response fell just short, by 1976 *Paleobiology* had roughly 1,850 subscriptions. More importantly, the journal was in the black for its first two years: the 1976 report to the Paleontological Society Council reported \$23,700 in subscription income, balanced against \$24,000 in editorial costs. Additional costs—including page charges—had been successfully covered by a sufficient number of authors.<sup>22</sup> The next year the journal again broke even, and the number of subscriptions reached 2,000; in 1978, this number rose to 2,100, and the journal actually made a profit. At the end of his second three-year term as editor of the journal, Schopf cowrote a report (with Hopson) to the council summarizing the success of the venture during its first six years. They reported, first, that the journal was continuing to receive a healthy volume of high-quality manuscripts—an average of 80–85 per year, with an acceptance rate of 40–50%.<sup>23</sup> They also drew attention to the yearly increases in subscriptions, and optimistically estimated that the current total could be

increased by at least 25% during the next three years. Additionally, they noted that the journal "has operated in the black every year of its existence," and proposed that the growing Patron's Fund be used as an endowment with income used to support nonoperating expenses, such as reprinting back-issues. Finally, they made the case that *Paleobiology* had genuinely contributed to shaping the field of paleontology: "It is no accident that the success of *Paleobiology* as a journal has paralleled the increasing attention which paleontology in general has received during the past few years."

## CONCLUSION

The subsequent history of the journal—and of the subdiscipline—is another story. Suffice it to say that, contrary to the fears of many, the PS did not fracture, *Journal of Paleontology* did not fold or fade into irrelevance, and *Paleobiology* remained a vital and important outlet for macroevolutionary and biologically oriented paleontology. In conclusion, though, I want to return to the comparison I began with between the activities of Mayr in the establishment of the modern synthesis and what I have called the "delayed synthesis" of paleontology with evolutionary biology 30 years later. One of the most compelling parallels is the similarity between the protagonists of both histories: Mayr and Schopf both engaged in clear examples of what Smocovitis called "discipline building," and faced many of the same hurdles en route to their accomplishments (e.g., a polarized set of attitudes toward theory, a lack of existing interdisciplinary structures, entrenched expectations regarding "proper" questions and methods, anxiety within existing disciplinary membership about launching new scholarly outlets, etc.). It should also be stressed, though, that a one-to-one comparison between Mayr and Schopf is problematic: Mayr was uniquely able to perform a set of related but distinct activities for which a small group was needed in the case of paleobiology. Whereas Mayr could function when necessary as the brilliant and persuasive writer, could provide an unimpeachable reputation in traditional empirical work, and could be a tireless organizer, these tasks (I argue) were divided between Gould, Raup, and Schopf, respectively. Nonetheless, the comparison is compelling.

In a slightly more abstract sense, there are equally compelling similarities available in our historical interpretation of these two events. Cain has emphasized the role of "situationalized cooperation" as an important feature in the establishment of new disciplinary structures: both to cross "disciplinary boundaries in pursuit of common problems," and "to ensure inclusion and to elevate the status of fields and practices otherwise deemed marginal" within an existing disciplinary framework (Cain, 1993, p. 2). I argue examples of just this kind of situationalized cooperation can be found in the establishment of paleobiology. Cain has also noted that, in the case of the modern synthesis, "synthesis" from the perspective of geneticists meant de-emphasizing or "subsuming" traditional descriptive practices; this was very much the agenda of Schopf and his colleagues for their own field (Cain, 1993, p. 17). Finally, in a more recent article, Cain has discussed Mayr's activities with CCP and the journal within the context of some theories raised in the sociology of scientific knowledge. In particular, he describes the CCP as an example of what Peter Galison has labeled a "trading zone," where certain institutions can serve as a conduit for interdisciplinary communication and cross-pollenization between distinct disciplinary cultures (Cain, 2002, pp. 284, 309–310). Without belaboring this comparison overmuch here, I

would like to propose the transformation in early 1970s paleobiology as an additional and potentially quite useful case study of the dynamics of scientific change in recent society. It also marks a significant event in the history of evolutionary biology: paleontology, long the ugly stepchild of the evolutionary community, found a new voice to assert the importance of its methods and data. The establishment of an outlet for unique paleontological contributions to evolutionary theory was a coup for biologically oriented paleontologists and, ultimately, a major step toward the completion of the project of the modern synthesis.

## NOTES

1. Hull (1988). Joe Cain has expanded on Hull's analysis of the SSZ (Cain, 2004). Other relevant histories of scientific organizations include Sally Gregory Kohlstedt's examination of the American Association for the Advancement of Science, and Martin J. S. Rudwick's study of the origins of the Geological Society's journal (see Kohlstedt, 1999, and Rudwick, 1993).
2. For Schopf, revitalization of paleontology was necessary not only to reorient paleontology within the disciplinary framework of evolutionary biology, but also to correct methodological and philosophical deficiencies he believed had prevented paleontology from realizing its full potential. In 1979, for example, he composed a list of "significant advances" in paleontology over the previous ten years, and rated as number one "The methodological assault on paleontological determinism. Stochastic paleontology . . . [is] the most significant theoretical rehauling of paleontology since 1859. It really is conceptually new. Species as particles. A theory from paleontology itself, not beholden to stratigraphy, or biology, or anything but pure and simple laws of large numbers over the vastness of time. What could be more beautiful!" (Schopf to Gould, June 11, 1979. Schopf Papers, Box 5, Folder 14). Schopf imagined a paleontology where the history of life could be reduced to a set of "gas laws" that explained the differential success of species over time, and strongly believed that random factors in the history of life determined evolutionary outcomes. He later wrote in an unpublished manuscript that "I would contend the dinosaurs suffered from bad luck, not bad genes, and if geologic history were replayed on another planet, but with similar ecologic constraints, dinosaurs might well have made it," since the constraints on evolution are so complex that it is hopeless to try to account for individual local adaptations in a deterministic fashion (Schopf, "Narrative." Undated, circa 1981. Schopf Papers, Box 12, Folder 17). Schopf's interest in stochastic, nondeterministic interpretations of the fossil record were shared by his colleagues Raup and Gould, although with different emphasis. Raup was convinced that randomness played a key role in extinction patterns (e.g., Raup, 1988), but distanced himself from Schopf's more enthusiastic embrace of stochasticism as a philosophy. Gould, on the other hand, made contingency a central feature in his own evolutionary interpretation, and shared many of Schopf's concerns about the pervasiveness of deterministic thinking in paleontology (see Gould & Lewontin, 1979, and Gould, 1989, 2002). The point here is that paleontologists had a variety of reasons for changing the emphasis in paleontology, and were motivated equally by professional and intellectual convictions in doing so.
3. Raup, for example, raised significant doubts about what he thought was the too-zealous application of stochastic models by Schopf and the others, which apparently had caused a minor rift during the last Woods Hole meeting (Raup to Schopf, Gould, and Simberloff, April 7, 1975. Schopf Papers, Box 3, Folder 30).
4. Interestingly, the journal *Evolution* had its own controversial beginnings, and owed much of its initial success to the efforts of Ernst Mayr. For an account of the establishment of *Evolution* and the Society for the Study of Evolution, see Cain (1994), and Smocovitis (1994).
5. Sokal and Rohlf first published a description of taxonomy using factor analysis in the journal in 1962, initiating a discussion concerning the use of computers and multivariate statistics, which played out in the journal for the next ten years. Important papers in quantitative systematics

- included Sokal and Rohlf (1962), Dice (1952), Sneath (1961), Mayr (1965), Rohlf and Fisher (1968), and Peters (1968).
6. “Report to Paleontological Society: Activities of the Committee on Paleontology and the Paleontological Society during last 6 months.” Paleontological Society Archives, Box 24.
  7. In this instance, “descriptive” is being used as a pejorative term for the stratigraphically oriented paleontology that Schopf and comrades hoped to unseat.
  8. David M. Raup, interviewed by Patricia M. Princehouse, May 11, 1998. Recorded interview in possession of David Sepkoski.
  9. Valentine to Schopf, November 20, 1973. Paleobiology Editorial Papers, Box 14. There is no record explaining—nor do any of the surviving participants involved recall—how Schopf was chosen for this task.
  10. Schopf, “Draft Report on the Format and Feasibility of a New Paleontological Society Journal.” Paleobiology Editorial Papers, Box 14.
  11. Schopf, “Draft Report,” p. 2.
  12. Schopf, “Draft Report,” p. 10.
  13. Schopf, “Draft Report,” p. 19.
  14. Schopf and Valentine, “Draft Memo,” Paleontological Society Archives, Box 24.
  15. “News and Notes of the Paleontological Society,” June 6, 1974.
  16. Photocopies of ballots are preserved in the Paleobiology Editorial Papers, Box 14. I have suppressed names of individual respondents.
  17. PS Council Meeting Notes, April 1, 1974. Paleontological Society Archives, Box 24.
  18. PS Council Meeting Notes, November 17, 1974. Paleontological Society Archives, Box 24.
  19. Schopf and Hopson, “Solicitation Template,” November, 1977. Paleobiology Editorial Papers, Box 14.
  20. Periodicity was originally unveiled in a series of papers between 1982 and 1986. See Raup and Sepkoski (1982, 1984, 1986). The more general “Nemesis” hypothesis grew out of the work of a number of authors in different disciplines. See particularly L. W. Alvarez, W. Alvarez, Asaro, & Michel (1980, 1984), Rampino and Stothers (1984), Schwartz and James (1984), and Whitmire and Jackson (1984).
  21. Schopf, “Introductory Editorial” (draft). Paleobiology Editorial Papers, Box 14.
  22. Schopf, “Semi-Annual Report,” October 12, 1976. Paleobiology Editorial Papers, Box 14.
  23. Schopf and Hopson, “Six Year Report to the Council of the Paleontological Society Regarding *Paleobiology*: 1974–1980.” Paleobiology Editorial Papers, Box 14.

## REFERENCES

### Archival Sources

- Paleontological Society Archives. University of Illinois Champaign-Urbana Library. RS 15/11/20. *Paleobiology* Editorial Papers. University of Illinois Champaign-Urbana Library.
- Schopf, T. J. M. Papers. Smithsonian Institution Archives. RU 007429.
- Sepkoski, J. John, Jr. Papers. American Philosophical Society Library.

### Published Sources

- Alvarez, L. W., Alvarez, W., Asaro, F., & Michel, H. V. (1980). Extraterrestrial cause for the cretaceous-tertiary extinction. *Science*, 208, 1095–1108.
- Alvarez W., Kauffmann, E. G., Surlyk, F., Alvarez, L. W., Asaro, F., & Michel, H. V. (1984). Impact theory of mass extinctions and the invertebrate fossil record. *Science*, 223, 1135–1141.

- Cain, J. (1992). Building a temporal biology: Simpson's program for paleontology during an American expansion of biology. *Earth Sciences History*, 11, 30–36.
- Cain, J. (1993). Common problems and cooperative solutions: Organizational activity in evolutionary studies, 1936–1947. *Isis*, 84, 1–25.
- Cain, J. (1994). Ernst Mayr as community architect: Launching the Society for the Study of Evolution and the journal *Evolution*. *Biology and Philosophy*, 9, 387–427.
- Cain, J. (2002). Epistemic and community transition in American evolutionary studies: The “Committee on Common Problems of Genetics, Paleontology, and Systematics” (1942–1949). *Studies in History and Philosophy of Biological and Biomedical Sciences*, 33, 283–313.
- Cain, J. (2004). Launching the Society of Systematic Zoology in 1947. In D. M. Williams & P. L. Forey, (Eds.), *Milestones in systematics (Systematics Association Special Volume Series 67)* (pp. 19–48). Boca Raton, FL: CRC Press.
- Dice, L. R. (1952). Quantitative and experimental methods in systematic zoology. *Systematic Zoology*, 1, 97–104.
- Gould, S. J. (1984). The life and work of T. J. M. Schopf (1939–1984). *Paleobiology* 10, 280.
- Gould, S. J. (1989). *Wonderful life: The Burgess shale and the nature of history*. New York: Norton.
- Gould, S. J. (2002). *The structure of evolutionary theory*. Cambridge, MA: Harvard University Press.
- Gould, S. J., & Lewontin, R. C. (1979). The spandrels of San Marco and the Panglossian paradigm: A critique of the adaptationist programme. *Proceedings of the Royal Society of London, Series B, Biological Sciences*, 205(1161), 581–598.
- Hagen, J. (2003). The statistical frame of mind in systematic biology from *Quantitative Zoology* to *Biometry*. *Journal of the History of Biology*, 36, 353–384.
- Kohlstedt, S. G. (1999). Creating a forum for science: AAAS in the nineteenth century. In S. G. Kohlstedt, M. M. Sokal, & B. V. Lewenstein (Eds.), *The establishment of science in America: 150 years of the American Association for the Advancement of Science* (pp. 7–49). New Brunswick, NJ: Rutgers University Press.
- Maynard Smith, J. (1984). Palaeontology at the high table. *Nature*, 309, 401–402.
- Mayr, E. (1965). Numerical phenetics and taxonomic theory. *Systematic Zoology*, 14, 73–97.
- Peters, J. A. (1968). A computer program for calculating degree of biogeographical resemblance between areas. *Systematic Zoology*, 17, 64–69.
- Rampino, M. R., & Stothers, R. B. (1984). Terrestrial mass extinctions, cometary impacts and the sun's motion perpendicular to the galactic plane. *Nature*, 308, 709–711.
- Raup, D. M. (1975). Taxonomic survivorship curves and Van Valen's law. *Paleobiology*, 1, 82–96.
- Raup, D. M. (1988). Patterns of generic extinction in the fossil record. *Paleobiology*, 14, 109–125.
- Raup, D. M., Gould, S. J., Schopf, T. J. M., & Simberloff, D. S. (1973). Stochastic models of phylogeny and the evolution of diversity. *Journal of Geology*, 81, 525–542.
- Raup, D. M., & Sepkoski, J. J., Jr. (1982). Mass extinctions in the marine fossil record. *Science*, 215, 1501–1503.
- Raup, D. M., & Sepkoski, J. J., Jr. (1984). Periodicity of extinctions in the geologic past. *Proceedings of the National Academy of Sciences of the United States of America*, 81, 801–805.

- Raup, D. M., & Sepkoski, J. J., Jr. (1986). Periodic extinction of families and genera. *Science*, 231, 833–836.
- Rohlf, F. J., & Fisher, D. R. (1968). Tests for hierarchical structure in random data sets. *Systematic Zoology*, 17, 407–412.
- Rudwick, M. J. S. (1993). Historical origins of the Geological Society's journal. *Journal of the Geological Society*, 150, 3–6.
- Schopf, T. J. M., ed. (1972). *Models in paleobiology*. San Francisco: Freeman, Cooper & Co.
- Schopf, T. J. M., Raup D. M., Gould, S. J., & Simberloff, D. S. (1975). Genomic vs. morphologic rates of evolution: Influence of morphologic complexity. *Paleobiology* 1, 63–70.
- Schwartz, R. D., & James, P. D. (1984). Periodic mass extinction and the sun's oscillation about the galactic plane. *Nature*, 308, 712–713.
- Simpson, G. G., Roe, A., & Lewontin, R. (1960). *Quantitative zoology* (rev. ed.). New York: Harcourt, Brace.
- Smocovitis, V. B. (1992). Unifying biology: The evolutionary synthesis and evolutionary biology. *Journal of the History of Biology*, 25, 1–65.
- Smocovitis, V. B. (1994). Disciplining evolutionary biology: Ernst Mayr and the founding of the Society for the Study of Evolution and *Evolution* (1939–1950). *Evolution*, 48, 1–8.
- Smocovitis, V. B. (1996). *Unifying biology: The evolutionary synthesis and evolutionary biology*. Princeton, NJ: Princeton University Press.
- Sneath, P. H. A. (1961). Recent developments in theoretical and quantitative taxonomy. *Systematic Zoology*, 10, 118–139.
- Sokal, R. R., & Rohlf, F. J. (1962). The description of taxonomic relationships by factor analysis. *Systematic Zoology*, 11, 1–16.
- Sokal, R. R., & Rohlf, F. J. (1969). *Biometry*. San Francisco: W. H. Freeman.
- Whitmire, D. P., & Jackson, A. A., IV (1984). Are periodic mass extinctions driven by a distant solar companion? *Nature*, 308, 713–715.