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CHAPTER FIFTEEN

“Radical” or “Conservative”? The Origin and Early Reception of Punctuated Equilibrium

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In 2002, only months before his death, Stephen Jay Gould published *The Structure of Evolutionary Theory*, the summation of more than thirty years' grappling with the Darwinian theory of evolution. In the community of evolutionary biologists, Gould is probably best known for authoring, with Niles Eldredge, the theory of punctuated equilibrium. In *Structure*, a lengthy, revisionist history of the punctuated equilibrium debate spans the final third of the volume and attempts to set the record straight about the proper interpretation of his theory in relation to neo-Darwinian evolutionary orthodoxy. That this section of the book runs nearly 300 pages is hardly surprising given the tremendous—and often heated—controversy punctuated equilibrium created during Gould's lifetime. As a number of reviews of *Structure* have noted, Gould attempts to strike a fairly delicate compromise: on the one hand, he argues that punctuated equilibrium does *not* challenge the essential principles of Darwinian natural selection—it is “a speciation theory of macroevolution, with species treated as irreducible Darwinian individuals playing causal roles analogous to those occupied by organisms in Darwinian microevolution.”¹ Viewed more broadly, however, it is apparent that the unifying theme of *Structure* as a whole is a reevaluation of what it means to be Darwinian, particularly in the context of the Modern Evolutionary Synthesis of the mid-twentieth century. The chapter on punctuated equilibrium is merely the centerpiece of a broader argument that, in total, spans more than 800 of the book's 1,343 pages, and draws ammunition from macroevolution and species selection, development (ontogeny and phylogeny), adaptationism, and the history of the Modern Synthesis itself. As the capstone to a life's work in evolutionary the-

ory, *Structure* is clearly a frontal assault on neo-Darwinism; punctuated equilibrium is just as clearly the lynchpin of that attack.

Reviewers of *Structure* have also noted that Gould presents nothing particularly new in the book; nothing, that is, if one has been following his career since the mid-1970s. Over the decades since the publication of *Ever Since Darwin* (in 1977), Gould's public and scientific personae have been somewhat at odds: while to the general public he was the sober defender and promoter of scientific rationality and literacy, to his scientific colleagues he was the ardent proponent of a radical (and perhaps misguided) view of evolutionary change. Yet despite the brashness of many of his claims on behalf of punctuated equilibrium over the years, one is brought time and again back to the reconciliatory, even conservative justifications Gould made for his theory: in *Structure*, for example, he recalls the common "misunderstanding" of his critics that "I proclaimed the total overthrow of Darwinism, and . . . that I intended punctuated equilibrium as both the agent of destruction and the replacement."² For the historian attempting to assess the significance of Gould's scientific work, as well as for the scientist or philosopher trying to evaluate it, there is an immediate and important question: just how radical did Gould intend to be?

This essay will attempt to resolve some of the uncertainty surrounding this question. First, ignoring for the most part the literature retrospectively assessing the significance of punctuated equilibrium, I will examine what Gould and his colleagues thought about the theory at the time it was conceived and shortly afterward. Second, I will argue that punctuated equilibrium cannot be separated from the broader development of theoretical paleobiology promoted during the mid-1970s by a group of paleontologists dedicated to revising the goals, agenda, and status of paleontology. Third, I will argue that when viewed in this context, punctuated equilibrium is in fact part of a larger movement that involved not just Gould and Eldredge, but his colleagues David Raup, Thomas Schopf, Steven Stanley, and others as well, which sought to redefine paleontological evolutionary theory largely without the directional causes central to traditional Darwinian evolution via natural selection. This approach made innovative use of kinds of quantitative statistical analysis and computer simulation that were new to paleontology, pioneered modeling of macroevolutionary patterns as stochastic (or random) processes, and also rehabilitated the evolutionary significance of extinction dynamics (particularly mass extinctions), all of which posed questions about the ubiquity of natural selection as the sole causal mechanism in evolution. Punctuated equilibrium was not even necessarily the most iconoclastic or radical product of this movement.

Finally, however, I will argue that a dichotomy between the labels *radical* and *conservative* is not appropriate for describing these theories or the motivations of the authors of punctuated equilibrium. Even among the central figures in the new paleobiology (some of whom cheekily called themselves the “radical fringe” in paleontology) there was significant disagreement concerning the proper theoretical approach with which to challenge neo-Darwinism. In particular, I will focus on the relationship between Gould and Tom Schopf. Schopf was Gould’s close friend and collaborator, and he provided important institutional support for Gould’s work (in addition to editing the volume *Models in Paleobiology*, where punctuated equilibrium first appeared in print, he was founding editor of the journal *Paleobiology*, where many of Gould’s scientific papers were published). He was also the most strident advocate of stochastic modeling and nondirectionality in macroevolution, but at the same time one of the most vocal opponents of punctuated equilibrium. The dynamics of Gould and Schopf’s personal friendship and professional rivalry, particularly represented in their correspondence, serves as a microcosm for the broader debates in evolutionary paleobiology during the 1970s. By examining this dynamic I will argue that the paleobiological response to neo-Darwinism involved elements of both radicalism and conservatism, and that in surprising ways, what might be seen as radical by one observer may well have been conservative to another.

THE ORIGIN OF THE THEORY

In light of the great significance punctuated equilibrium was to have in his later career, it is ironic that Gould did not even want to write about speciation theory in the first place. In early 1970, Schopf was organizing a symposium for the Geological Society of America’s 1971 annual meeting on the subject of “Models in Paleontology.” In soliciting participants, Schopf described the session 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,” Schopf continued, “we can encourage the analytical ‘problem oriented’ approach in Paleontology.”³ Schopf asked Gould to contribute a paper on models in speciation, and Gould’s response was decidedly ambivalent: “A damned good idea, your symposium. I’m flattered by your invitation and will gladly accept. My only hesitation is that you have given me a topic that ranks only third on your list in terms of my competence [behind models in mor-

phology and phylogeny].” Nonetheless he agreed, since the other subjects were already taken, but he suggested a joint effort with Niles Eldredge, whom he described as “our best new thinker.”⁴ The eventual compromise was to coauthor the paper with Eldredge, which Gould presented at the conference but for which Eldredge was first author in the subsequent published proceedings. The result was the now famous essay “Punctuated Equilibrium: An Alternative to Phyletic Gradualism.”⁵

The classic Darwinian evolutionary model assumes that species change very gradually over vast amounts of time (tens of millions of years or more), developing in response to equally slow and gradual changes in environment that produce adaptations that ultimately lead to the appearance of new species. A central assumption is that this is a constant, inexorable process, and that the tempo of evolution is unchanging. The major driving force in Darwinian evolution is the mechanism of natural selection, which presses individual organisms to compete with one another and their environments, rewards beneficial adaptations, and punishes less-successful species with extinction. That the fossil record was incomplete—with its intermittent gaps and jumps, and notorious absence of transitional missing link species—was of no concern to evolutionists, who contended that it was simply the result of an imperfectly preserved record.

However, beginning with the unorthodox intuition that the fossil record was in fact a much more accurate record of the history of life than had been previously assumed, Gould and Eldredge proposed a radical revision of this standard narrative. They argued that the pattern of evolutionary history really was composed of fits and starts, consisting of long periods of evolutionary stasis (or “equilibrium”) “punctuated” by shorter periods of rapid speciation. This theory presented some very significant revisions of Darwinian evolutionary theory: by suggesting that species can act as independent units in natural selection, Gould and Eldredge upset the orthodox Darwinian assumption that natural selection can only bring about adaptive advantages in single organisms. Punctuated equilibrium instead proposed that entire species have life spans, with a birth, a long, stable period of existence, and a death, followed, in many cases, by offspring species.⁶

This theory was not composed in a vacuum—it was based solidly on previous work done by scientific luminaries like the biologists Ernst Mayr and Theodosius Dobzhansky and the paleontologist George Gaylord Simpson—but it attracted immediate attention for Gould (who became its spokesman) and caused eventual controversy. The theory effectively undercut the traditional understanding of the tempo of evolution by seeing phyletic gradual-

ism as “very rare and too slow, in any case, to produce the major events of evolution,” as Gould and Eldredge wrote in a reassessment of their theory in 1977.⁷ According to many evolutionary biologists, this was tantamount to heresy: Darwinian evolution required a slow, steady progression over which natural selection could operate, in order for the accumulation of adaptations in individuals to gradually produce sufficient variations to cause speciation. If, as Gould proposed, species remained stable and unchanged for millions of years, only to suddenly branch off as new species or disappear completely, then what mechanism could account for this pattern?

THE IMMEDIATE RECEPTION

From the very beginning, the authors and Schopf were conscious of the potential for misreading punctuated equilibrium: for example, Gould objected to Schopf’s draft introduction to the volume, which characterized the Gould-Eldredge paper as a statement of “the way that metaphysics impinges on considerations of speciation.”⁸ Gould clarified that his paper did not assume “an untestable statement about the world,” but rather presented “an a priori testable theory,” and was therefore not metaphysical. The theory, Eldredge and Gould argued privately to Schopf, was simply a defense of the significance of allopatric speciation (speciation based on geographic isolation), which was a concept firmly established in current paleontological thought, but whose implications for macroevolution—namely that it challenged traditional phyletic gradualism—were not widely appreciated.⁹ Nonetheless, throughout the editing process Schopf worried about possible objections to the paper, and cautioned the authors to moderate their tone: “You self-consciously appear to give the type of case you can easily attack,” he warned Eldredge. “That comes through, Niles, and that is precisely what will turn people off to your argument. Rather than educate, you may offend. That was the tone of the review[s]. . . . Given that response, Niles, you cannot be oblivious to the potential general response.” “However,” Schopf concluded, “the manuscript stays just as you have given it.”¹⁰

As Eldredge recalls, Schopf “really hated the punctuated equilibria paper. He really hated it.” As a consequence, Schopf sent the paper out to a dozen reviewers, and forwarded only the reviews that confirmed his own assessment.¹¹ One of these editorial reviews of the chapter summarized what would characterize many of the later reactions to the published piece. The referee began by commenting on the elegance of the writing, its “quality of introspection,” and its “refreshing” emphasis on “the influence of theory on discovery.” How-

ever, the review also questioned whether Eldredge and Gould's presentation of the "species problem" was a straw man, noting (quite alliteratively) that "it has a built-in Platonic paradox for pushing paleontological pundits toward polemic precipices for purposes of publication. Well written rhetoric, or sophistry, or something. Not one whit closer to the problem."¹² This reaction was mirrored in the other referees' comments (all of whom urged publication despite criticisms) and in the reception of the paper once published: the consensus was that it was a flawed but provocative theory, and most agreed that it was an elegant piece of writing. A few paleontologists became immediate converts, while a few others saw no merit in the idea whatever, but most simply withheld final judgment.

Over its first few years of life, punctuated equilibrium attracted relatively little attention. During this time Gould collaborated with Schopf and Raup on other work involving stochastic simulation of macroevolution, including a project Schopf organized with Gould and Raup to better understand macroevolutionary patterns through a variety of statistical and computer-generated models. Schopf 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?"¹³ The result of this initial work was a groundbreaking paper (in 1973) jointly authored by Gould, Schopf, Dave Raup, and Daniel Simberloff, titled "Stochastic Models of Phylogeny and the Evolution of Diversity."¹⁴ 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 likely had stochastic variables.

In the fall of 1976, Gould mailed a manuscript to Schopf at *Paleobiology* titled "Punctuated Equilibrium: The Tempo and Mode of Evolution Reconsidered." This paper, again coauthored with Eldredge, was the authors' first major reconsideration of their theory. In his covering letter Gould described the "rather exuberant manuscript" as "a labor of love," and as a defense of the theory it served several functions.¹⁵ First, more than even the original paper, the new manuscript made punctuationism the center of a philosophical recon-

ceptualization of evolutionary change. Here the authors were more explicit about the exact nature of the conceptual reconfiguration their theory brought to macroevolution—in particular by adapting Steven Stanley’s formulation of the asynchrony of micro- and macroevolution published a year earlier.¹⁶ The paper also gave Gould and Eldredge a chance to address some of the criticisms that had been leveled at their theory, and to clarify what they actually intended to say in their first paper. Here the authors dismissed charges that they were motivated by an a priori disdain for gradualism, and that they took a “defeatist attitude” toward the testability of macroevolutionary claims using the fossil record.¹⁷ They also presented a significant amount of empirical evidence to prove the theory by considering a wider sample of taxa than they had in the 1972 essay, and by answering empirical challenges to their earlier conclusions. Finally, Gould and Eldredge extended their model to propose a new and “general philosophy of change” in the natural world. Here they explored the sociocultural basis for gradualism, and proposed punctationism as a new “metaphysic,” which they suggested “may prove to map tempos of change in our world better and more often than any of its competitors.”¹⁸ This was done without any apparent awareness of irony, given Gould’s claim in 1972 that the theory was “not metaphysical.”

Gould and Schopf had by this time developed a friendship through their collaboration on stochastic modeling (which came to be known as the “MBL model”), but this submission was potentially complicated by the fact that Schopf was an avowed gradualist who had privately expressed his strong reservations about punctuated equilibrium to Gould.¹⁹ While Gould acknowledged their difference of opinion, his letters do not suggest he anticipated difficulty having the paper published, or that he worried Schopf’s private beliefs would influence editorial decisions. On the contrary, he invited Schopf to “collect your thoughts on gradualism into a full-scale paper,” since “we would certainly welcome a rebuttal at a higher level than has been directed to us so far.”²⁰ In any case, the paper was accepted with only minor revisions, and Schopf closed his letter acknowledging acceptance of the final manuscript by commenting “we are pleased (and proud) to have your article in *Paleobiology*. . . . I feel this is a most remarkable article, one that will merit and require a lot of careful attention.”²¹

Leaving aside discussions of its conceptual significance (for the time being), from a sociological standpoint this paper is interesting because of what it did *not* do. Gould has written that the article was inspired, in part, because “enough data, argument, and misconception as well had accumulated by the summer of 1976.”²² This may be true, but it is worth noting how little

controversy the 1977 paper provoked, especially compared to the fairly violent reaction the theory saw in the early-to-mid-1980s. As already mentioned, Schopf—who would later publish several fairly harsh criticisms of the theory—was unambiguously pleased with the manuscript. Additionally, one of the paper’s two reviewers was Philip Gingerich, who became a staunch opponent of Gould’s in the 1980s (whom Gould suggests became intellectually jealous of the theory).²³ Gingerich’s review was not especially critical—he gave it a lower rating than the other referee (“good” as opposed to “excellent”)—but he also characterized it as “an interesting and important paper.”²⁴

Why, then, did the 1977 paper fail to cause the uproar that followed later articles (most notably Gould’s 1980 “Is a New and General Theory of Evolution Emerging?”), which will be discussed in the following)? Certainly not because the paper was timid; if anything, the authors had strengthened their argument for the macroevolutionary significance of punctuation, had clarified the anti-neo-Darwinian implications of the theory, and moreover had made a case for broad “metaphysical” punctuationism. It is difficult to know precisely why the 1977 paper did not excite passions the way later articulations of the theory did; perhaps the explanation Gould gives in *Structure* is most accurate: “The early history of punctuated equilibrium unfolded in a fairly conventional manner for ideas that ‘catch on’ within a field. The debate remained pretty much restricted to paleontology . . . [and] most discussion, to our delight, arose from empirical and quantitative studies.”²⁵

THE TURNING POINT

In the three years following publication of the 1977 paper—prior to Gould’s next major articulation of the theory, in other words—the article was cited several times in *Paleobiology*, although nearly always favorably or at least neutrally.²⁶ The single example of criticism to appear in *Paleobiology* was Fred Bookstein, Gingerich, and Arnold Kluge’s coauthored paper, “Hierarchical Linear Modeling of the Tempo and Mode of Evolution,” which appeared in the spring of 1978. In reviewing the manuscript, Raup called it “the best paper to date on the gradualism argument” and commented “it breaks a lot of new ground and thus should have a seminal effect on future research.”²⁷ While the authors do conclude that “we see little use for further speculation based on the generality of punctuated equilibrium,” they do not entirely discount punctuation as an occasional pattern, and overall the criticism is a fairly mild. It is not an attack on Gould and Eldredge’s theory.²⁸

The turning point seems to have come—as Gould himself acknowledges—when Schopf asked him to contribute an essay assessing the “Status of Paleontology—1980” for the fifth anniversary of *Paleobiology*. Gould happily obliged, and in a single issue of the journal published two reflective papers on the state of the discipline. In the first, which he titled “The Promise of Paleobiology as a Nomothetic, Evolutionary Discipline,” Gould both celebrated the recent advances in paleobiology and reiterated his call for further progress toward revision of evolutionary theory based on macroevolutionary modeling. One of Gould’s longstanding concerns was to raise the status of paleontology with respect to other evolutionary fields, and he was pleased to report that “our profession now wears the glass slipper and, if not queen of the evolutionary ball, at least cuts a figure worth more than a passing glance.”²⁹ He found the most significant new direction for the field the widespread acceptance of “[G.G.] Simpson’s procedure of modeling and testing,” and the reduction of emphasis on ideographic studies in the “‘empirical law’ tradition.”³⁰ This emphasis reflected the goal he shared with Schopf of making paleontology more nomothetic, or law-producing; the title of this essay harkened back to a 1974 paper he coauthored with Raup in *Systematic Zoology*, “Stochastic Simulation and Evolution of Morphology: Towards a Nomothetic Paleontology.”³¹

This reference was quite intentional: the aim of Gould’s first 1980 essay was to carve out paleontology’s unique niche among the evolutionary sciences, and the area where he saw most promise for both originality and nomotheticism was in stochastic modeling of macroevolutionary patterns. Macroevolution clearly offered the best opportunity to emerge from serfdom to biology, since “if evolution works on a hierarchy of levels (as it does), and if emerging theories of macroevolution have an independent status within evolutionary theory (as they do), then paleontology may become an equal partner among the evolutionary disciplines.”³² However, Gould also suggested that this contribution might entail a reevaluation of the received view of Darwinism, as codified in the Modern Synthesis. The power of stochastic simulation and modeling in recent years seemed to suggest, Gould proposed, that macroevolutionary patterns did not follow the same deterministic lines as microevolutionary trends. Here was the decoupling of micro- and macroevolution Stanley had forcefully argued for, but Gould saw the potential for an even more profound shift in beliefs about the metaphysics of natural change: he concluded that “the world’s frequent fit to stochastic models might mean that ontological randomness really is an admissible way to encompass part of our universe—and that our preferences for determinism are a cultural prejudice born of the ideographics that prevail at the scale of our short personal existence.”³³

If a hint that Gould was using the occasion of the fifth anniversary of *Paleobiology* to attack the sacred cows of neo-Darwinism was detectable in the first essay, it was overwhelming in the second, “Is a New and General Theory of Evolution Emerging?” Much later, reflecting on the difficulty punctuated equilibrium had over the years, Gould located the origin of much of the controversy here: “the received legend about this paper . . . holds that I wrote a propagandistic screed [claiming] . . . first, the impending death of the Modern Synthesis; and second, the identification of punctuated equilibrium as the exterminating angel (or devil).”³⁴ Whether this interpretation is fair, it is notable that the two papers, taken together, more explicitly challenge the synthetic account of evolution than either of the Eldredge-Gould papers of the 1970s. Perhaps the most controversial statement in “New and General Theory” is the oft-quoted claim that “if Mayr’s characterization of the synthetic theory is accurate”—that is, if evolution is guided solely by the accumulation of small genetic changes and natural selection, and if macroevolution is nothing more than microevolution writ large—“then that theory, as a general proposition, is effectively dead, despite its persistence as textbook orthodoxy.”³⁵

In *Structure*, Gould protests on a number of grounds that his critics have read this and similar statements unfairly. He argues in particular that punctuated equilibrium “does not occupy a major, or even a prominent, place in my 1980 paper [‘New and General Theory’],” and he denies that his discussion of “the Goldschmidt break” promoted saltationism.³⁶ I will not attempt to assess whether Gould is justified in feeling misunderstood, but I will note that the language he used in “New and General Theory” to describe the synthetic account is fairly unequivocal: “I have no doubt that many species originate in this way [i.e., via neo-Darwinian mechanisms]; but it now appears that many, perhaps most, do not.”³⁷ Is it surprising that these statements caused biologists to sit up and take notice of what had formerly been seen as a fairly benign macroevolutionary theory of interest mostly to paleontologists? However, despite the fallout that resulted, this paper (and its companion) must be seen in a very real sense as a triumph for *Paleobiology* (and the discipline): here, finally, biologists (neontologists) were reading the journal and taking its contents seriously enough to become upset, and it is only really after that point that punctuated equilibrium entered general cultural currency.

PUNCTUATION UNDER FIRE

Over the next two years, Schopf finally went public with his own assessment of punctuated equilibrium in a series of highly critical publications. The first

was an item in *Paleobiology*'s Spring, 1981 “Current Happenings” section, which was a recently instituted forum in the journal for news and editorial comment about the field. The second was a scholarly, analytic paper in *Evolution*, titled “A Critical Assessment of Punctuated Equilibrium,” published in late 1982. The third was a letter to the journal *Science*, coauthored with Antoni Hoffman, published in February of 1983.³⁸ In each of these publications Schopf attacked the theory from a different angle—as impartial journal editor, then careful empirical paleontologist, and finally concerned scientific citizen. These attacks raise a somewhat perplexing question: given Schopf and Gould's close agreement about the general agenda and goal of paleobiological work, why did Schopf choose to undermine his discipline's most prominent theorist (and close personal friend) in a publicly and potentially embarrassing way?

Schopf's “Current Happenings” piece, “Punctuated Equilibrium and Evolutionary Stasis,” is presented as an objective summary of the state of evidence for and against Gould and Eldredge's theory. Schopf opens the piece with the fairly neutral goal that “it seems worthwhile to try to place the paleontological and biological evidence in a 1981 perspective,” and the implication is that the essay will consider the pros and cons of the argument.³⁹ However, Schopf's personal beliefs come quickly to the fore. He notes, for instance, that punctuated equilibrium seems to demand “some strongly deterministic factors in order to account for patterns of speciation and extinction in the fossil record,” and shortly thereafter clarifies his intentions with the piece: “the major purpose of this ‘Current Happenings’ is to encourage the quantitative and qualitative evaluation of these limitations and prediction of the punctuated equilibrium model so that a truer picture of evolutionary history may be obtained.”⁴⁰ (Schopf was intellectually committed to a nondeterministic, stochastic view of life, so his use of the label “deterministic” in the first quotation should be considered a kind of epithet).

Schopf then systematically enumerates what he interprets as the weaknesses of punctuated equilibrium: (1) The incompleteness of the fossil record: the appearance of suddenness (or punctuation) of speciation is an artifact “almost guaranteed” by the state of fossil knowledge, and “indicates nothing of any meaning about the process of evolution which led to these classes.”⁴¹ (2) Artifacts related to the recording of taxa durations: since taxa are commonly assumed to be present for the entire duration of the geologic stages in which they originate, their “book value” is often exaggerated. (3) Insufficient morphological information: “because only the most resistant and most numerous of hard parts can ever be studied, paleontologists must recognize species by

recourse to only a small part of an organism's actual evolutionary change."⁴² (4) Poor sample population: it is easier to discern species-level evolution in organisms with well-defined hard parts, so we tend to omit organisms with "simple, relatively undifferentiated forms." (5) General limitations of taphonomy: short-lived species are much less likely to be preserved, and hence recorded. The majority of these complaints have to do with *signal* errors—i.e., observations about perceived limitations on generalizing from fossil data. Notably, only the potential weaknesses of the theory are considered, and there is no mention of any of the recent studies that attempted to produce empirical verification of punctuated equilibrium.

It appears that Gould obtained a copy of the piece shortly before it was to be published, and his reaction was surprised and indignant. He did not object to Schopf's critique of the theory *per se*, but rather questioned the use of his position as editor to present it:

I must confess—and I expressed this to Jim [Hopson, Schopf's coeditor] when he called me for another reason two weeks ago—that I am not altogether happy with the forum that you have chosen for the piece. If it had been submitted as a regular article, I would have welcomed it entirely (while disagreeing strongly, of course, with its conclusions). In a sense, I am flattered that you consider punctuated equilibrium as a "happening"—and therefore worthy of inclusion in your section. Yet I confess that I do not think it fair for you, as editor, to use this section as a forum for expressing personal viewpoints on issues of the moment.⁴³

Gould went on to stress his belief "that accounts of happenings may and indeed even should express a point of view," but added "ideas aren't events—and I would argue that the editor of such a section should not use his prerogative as a platform for expressing personal opinions about theoretical issues." It seems that Gould objected particularly to the ostensibly objective way Schopf had presented the piece: "I don't think I feel angry about this, but I am not unconcerned either. I just don't see how one man's viewpoint can become a kind of official line in one section of a journal."

This disagreement did not cause serious damage to Schopf's friendship with Gould, but it did begin a new, more directly combative, phase of their relationship. For the next several years (until his untimely death in 1984), Schopf devoted considerable energy to attacking punctuated equilibrium in print and elsewhere. His paper in *Evolution*, published in late 1982, added more empirical depth to the arguments he raised in his "Current Happen-

ings” piece, and takes a scholarly, measured approach to arguing against general morphological stasis in favor of gradualism (for example, the paper opens by claiming to “take the position of a devil’s advocate” with regard to punctuation). However, this appearance exists only in the final, published version; the manuscript Schopf originally submitted was far more partisan, and often seemed to associate criticism of the theory with its author. One review of the manuscript stated bluntly, “I don’t feel that this paper needs to be riddled with *ad hominum* [sic] references to Steve Gould. From a supporter, the term ‘Gouldism’ would appear laudatory; in this ms it simply seems to be sarcastic.”⁴⁴ Another reviewer found it “heavily slanted” and “irritatingly polemical and biased,” and recommended extensive revisions before publication.⁴⁵

Schopf’s motivation is not entirely clear, but a third review—from a paleontologist acquainted with Schopf—sheds some light. Her comments draw attention to Schopf’s “overly conciliatory tone” and “evident awe of Steve,” and suggest “Tom could probably do him far more honor by sharing his views with him than by monumentalizing his name with an as-yet insufficiently tested theory.” The review also expresses the sort of frustration with the fad-dishness of the theory that was only just then beginning to publicly surface:

the overly conciliatory tone he [Schopf] takes towards Gould, can only delay the acceptance of his ideas both among those of us who never particularly cared for punctuated equilibrium but found it difficult to voice dissenting views at meetings (I was there, so were you) or even in print and those who knew a bandwagon coming when they saw it and jumped without too critical a look. I do not agree with him that “punctuated equilibrium was put forth in a spirit of conjecture and refutation, utility and testing, thesis and antithesis.” My recollection is that punctuation was pretty much forced down our throats—some of us swallowed, others gagged, and, for a time refutation and testing and antithesis received very short shrift indeed. There are fashions in science—punctuation was one of them.⁴⁶

Here the intriguing possibility is raised that Schopf was—perhaps unconsciously—using his criticism of Gould the scientist to promote the reputation of his friend. This is even more apparent in the letter of 1983 he and Hoffman sent to *Science*. The ostensible purpose was to question whether “a static hierarchy [is] a true and correct view of life.”⁴⁷ Again, however, the published version differs significantly from the original manuscript. The opening paragraph of the first draft begins “S. J. Gould arguably is becoming the most im-

portant single force in the shaping of current popular evolutionary thought,” and describes him as “a very fine human being who values scholarship,” who “for [his] skill at argumentation and conceptual organization is widely and deservedly admired.”⁴⁸ The second draft of the letter is toned down considerably, but is still (in spite of its criticism) quite effusive: the new opening paragraph opines “we admire very much [Gould’s] efforts and readily acknowledge that no paleontologist has contributed as much to the popularity of evolutionary theory.”⁴⁹

The point here is not to psychoanalyze Schopf, but rather to seek a nuanced appreciation of his actions—both editorially and otherwise—toward Gould’s work. In the published version of the letter to *Science* the phrases quoted above are omitted, and most readers would have been unaware of Schopf’s appreciation for Gould’s ideas and character. Indeed, Schopf’s more casual acquaintances were often surprised by his personal affection for Gould. In one particularly striking instance, Schopf chided the prominent biologist Norman Horowitz after Horowitz had written a harsh review of Gould’s *Mis-measure of Man*. In his letter, Schopf upbraided Horowitz for labeling Gould a “Marxist,” and in a follow-up described Gould as “one of the most decent and humane humans I have ever met.”⁵⁰ Schopf’s feelings about both Gould and punctuated equilibrium were clearly quite complicated, and even conflicted. In his editorial capacity at *Paleobiology* and elsewhere (as editor of *Models*, for example), Schopf again and again provided a platform for helping Gould launch and establish his evolutionary beliefs—but he was also quite capable of using such positions to attack Gould’s theory.

In late 1982, Schopf sent Gould a letter explaining his position toward punctuated equilibrium in very candid terms. His major objection, he wrote, was that the theory had “got taken too far.” He worried that “the many biases of the fossil record that needed carefully, and systematically, to be looked into, never got looked into,” and that the effect it would have on future work could be harmful: “unless those who are brought into the field learn the rigor of testing, it will be for naught.”⁵¹ Still, he explained, “I don’t have a campaign against P. E.”; rather, “I do have a campaign *for* rigorous testing of these ideas.” Part of Schopf’s justification for his critical stance—and for his continued warm regard for Gould—was that he felt Gould was not entirely responsible for the excesses connected with the theory:

I don’t think it’s entirely your fault that P. E. got out of hand. As I see it, the “press” (Roger Lewin et al.) discovered Steve Gould, and what Steve Gould *happened* to be on was P. E. If it had been some other issue, then

that issue would be well known. The press recognizes personalities. It publicizes what those personalities are saying and doing. Sometimes, those particular sayings, and doings, are beyond their worth. I *think* this is what happened to P. E. The press didn't discover P.E. It discovered S. J. G.—and S. J. G. happened to discover P. E. Pure accident. Five years later, or earlier, it would have been different.

In Schopf's stochastic account of the reception of the theory, events might have been entirely different if the clock were wound back and restarted—just as trilobites might have survived in an alternate evolutionary scenario.⁵² Schopf was very clear, however, that his feelings on the matter did not jeopardize his affection for Gould: “So, Steve, I have felt and do feel, very close to you. But, I have to (had to) go my own way on P. E. But I'll defend you as a person as long as I can write.”

INTERPRETATIONS OF THE THEORY

Before concluding, I want to return to the more general question about the relation of 1970s (and later) paleobiology to Darwinian orthodoxy. The expected protagonist in this tale is Gould, and indeed there are reasons why his work should be viewed as central to the subversive set of ideas that came out of the “radical fringe” of paleontology to challenge Darwinian evolutionary theory. But despite being the most recognizable character, is Gould the real radical in this story?

Historians, sociologists, and philosophers have turned Gould's openness concerning his political views and their relation to his scientific work into a minor cottage industry. However, I submit that the real radical was not Gould, but Schopf. Consider the circumstances that led to punctuated equilibrium: at the time Schopf was organizing the original models symposium, Gould was a promising, if unspectacular young professor at Harvard known for his work on Bermudan snails and his review paper on allometry. He might truly have never even done his work on punctuated equilibrium had it not been for Schopf—a fact that was not lost on either man. In a very personal letter to Schopf in 1977, Gould reflected on this fact: “I have been dragged literally (once by you and once by Niles) into the two creative things I have done,” and he noted “my frame of mind, [was] so correctly identified by you at one of those dinners as conservative scientifically, despite general social and political views.”⁵³ Where, then, does the assumption that Gould was a committed evolutionary radical, and punctuated equilibrium his manifesto, come from?

In 1982, Ernst Mayr published an essay titled “Speciation and Macroevolution” in the journal *Evolution*. In it, he gave an account of two possible readings of punctuated equilibrium that, I argue, have significantly influenced the historiography of the theory. Mayr acknowledged Gould and Eldredge’s debt to him, writing that after being “totally ignored by the paleontologists for almost twenty years,” their theory explained “some of the most important findings of the paleontologists . . . in terms of my theory of peripatric speciation.”⁵⁴ He went on to specify that a “moderate” or “Mayr version” of punctuated equilibrium involves only a “slight translation” of his 1954 theory “into vertical terms.” However, he also notes (ominously) that a “drastic or ‘Goldschmidtian version’” of punctuated equilibrium was presented in Gould and Eldredge’s 1977 paper, which suggests that speciation is based on major mutations.⁵⁵

Michael Ruse has identified three historical phases in Gould’s thinking about punctuated equilibrium, and I suspect Ruse draws heavily on Mayr’s interpretation. The first phase of the theory is represented by the 1972 Eldredge-Gould paper, and according to Ruse offers “a fairly straightforward extension of orthodox Darwinism.”⁵⁶ Ruse comments, however, that between this first phase and the later second phase (which he dates to Gould’s 1980 essay “Is a New and General Theory of Evolution Emerging?”) something important happened: Gould was now “downplaying the role of natural selection,” and accordingly “the father figure had changed from Charles Darwin to Richard Goldschmidt.”⁵⁷ Ruse freely acknowledges that at the time his own essay was written (it was published in 1989), Gould “categorically denies that he himself was ever a saltationist in Goldschmidt’s or anyone else’s sense.”⁵⁸ Nonetheless, Ruse argues that “one can fairly say . . . Gould (especially) was starting to think of evolution’s processes through a lens or filter of discontinuity. . . . In his own mind, he was starting to highlight the essential abruptness of evolution, as opposed to its continuity.”⁵⁹ Finally, the third phase represents Gould’s final position, and dates to about 1982. This, Ruse comments, offers “a pull-back from extremism,” though not a “retreat” from hierarchical evolution. The final version of the theory offers a defense of punctuationism, but at the same time disavows Goldschmidtian saltationism.

If one looks at the early history of punctuated equilibrium (as this chapter has done), Ruse’s model appears valid. Immediate reactions—both initial reviews and comments in the scientific literature—suggest that the theory was seen as mildly controversial but hardly upsetting by most paleontologists and biologists who encountered it. Intense reaction begins in the early 1980s, shortly after Gould’s infamous essay in *Paleobiology*. Where I am inclined to

differ with Ruse, however, is in his apparent assumption that these phases reflect actual, substantive modifications to Gould’s conception of the theory. This is what I believe he has inherited from Mayr—he has made Mayr’s “moderate” and “drastic” readings of punctuated equilibrium into actual stages in the theory’s development. Ruse reiterates the position that Gould modified punctuated equilibrium in important ways in his chapter on Gould in *Mystery of Mysteries*, arguing that while Gould’s final position is amicable toward natural selection, “one sees that other factors, including brute chance, come increasingly into play.”⁶⁰

However, I would point out that brute chance was *always* a central component of the theory. If it was not as explicitly mentioned in the 1972 paper as it was in later publications, it was in the minds of Gould and his collaborators on stochastic modeling as early as December of that year, when the radical fringe group met in Woods Hole. The very first publication of the group (the 1973 paper in *Journal of Geology*) explicitly ties stochastic evolution to punctuated equilibrium:

In evolutionary terms, this [stochastic model of phylogeny] describes a situation where Phyletic transformation is absent and where new species arise only through speciation. We do not view this as an artificial simplification constructed to ease our calculations; Eldredge and Gould (1972) have argued that it corresponds to biological reality.⁶¹

In a series of follow-up publications, Gould and his collaborators continued to link punctuated equilibrium with a potentially stochastic and nondirectional view of evolution. For example, a paper coauthored with Raup in 1974 used “a somewhat idealized form of the evolutionary model presented by Eldredge and Gould (1972)” as a basis for its simulation of morphologic change. And lest there be any question concerning the relation of stochastic simulations to traditional Darwinism, Raup and Gould’s paper proposes to “discard the questionable model of Phyletic gradualism,” concluding that while “this paper is not an attack upon the concept of uni-directional selection. . . . Over long periods of time, undirected selection may be the rule rather than the exception in nature.”⁶²

CONCLUSION

Why, then, did Schopf fall out with the theory? Schopf’s initial support of punctuated equilibrium reflects his general commitment to expanding pale-

ontology into evolutionary theory, and more specifically his conviction that Gould's theory fit within the program of nomothetic, stochastic paleontology. As he described the group's work to an editor at the journal *Science*:

the important thing about all of this work is that it derives from a quite different conceptual viewpoint than has been used for a century of paleontology. . . . Rather than reading the fossil record 'literally'—and seeking empirical laws derived by summing up individual events[,] our approach more directly utilizes theory and predictions from theory.⁶³

However, he differed with Gould on some fundamental points. Most centrally, Schopf was certain that equilibrium models would ultimately best represent evolutionary change over the long haul. In other words, he believed that patterns of evolution and extinction tended to oscillate around a stable mean—the factors that determined the fluctuations might be considered random, but the law of evolution held that the system will always stabilize itself. This notion recalls Charles Lyell's steady state equilibrium model, and in fact the MBL group acknowledged this debt in one of its final publications, 1979's "The Shape of Evolution: A Comparison of Real and Random Clades." The paper concluded by invoking Lyell's "most cherished belief . . . that earth history, like planetary motion, was in a dynamic steady state," but went on to comment that "this belief represents one extreme metaphor in a continuum that places directionalism and notions of inherent progress at the other pole."⁶⁴ The authors noted that the tendency of paleontologists was to lean toward the directional pole, and to assert "inherent uniqueness for each period of time and . . . 'directions' in earth history." The paper, however, drew the opposite conclusion, and ended by boldly proclaiming "Lyell's metaphor is due for a renaissance."⁶⁵ Schopf's private views on the subject were apparently even more radical, as he expressed to Raup in early 1979:

in my view, all of paleontology, i.e., all of those fossils, is (are) simply a metaphore [sic] for what is really the statistical mechanics of a series of interacting hollow-curves, each hollow curve being appropriate to a given faunal province, or habitat type, and that because ecological disturbance . . . the forces of physics thru air and water and rock kill off individuals, and because of aging in any organism without 'disturbance,' the hollow curves of a given region rise and fall, sometimes becoming enormous, sometimes evaporating all together.⁶⁶

Note the inversion of the Lyellian metaphor here: as the MBL paper framed it, the geometric regularity of planetary motions was a metaphor for the dynamics of earth history. But according to Schopf’s (almost Platonic) formulation, it is the physical processes of natural change themselves that are a metaphor for mechanical geometry.

Despite having the word “equilibrium” in its title, Gould’s theory does *not* suggest the same kind of dynamic steady-state model Schopf favored. In Schopf’s vision, change is constant and minor. In Gould’s, change is virtually nonexistent during lengthy periods of stasis (in fact, “punctuated stasis” might be a better name for the theory), then comes suddenly and with major effect. Gould’s paper in 1982, “Darwinism and the Expansion of Evolutionary Theory,” frames the matter succinctly. The question, as he saw it, asks “is our world . . . primarily one of constant change (with structure as a mere incarnation of the moment), or is structure primary and constraining, with change as a ‘difficult’ phenomenon, usually accomplished rapidly when a stable structure is stressed beyond its buffering capacity to resist and absorb?”⁶⁷ In Schopf’s view, change is easy; in Gould’s, it is hard. As Schopf put it in a letter to Gould that same year, explaining (in part) his sudden attacks on punctuated equilibrium:

I hope you will see it [the letter in *Science* with Hoffman] not as a campaign *against* something (PE), but rather as a campaign *for* something (a view of the world where change is easy—and continuous). I think you hit the nail-on-the-center when you said it is a question of change ‘difficult’ vs. change ‘easy.’ My bias, owing to my Woods Hole bryozoan work—I recall those times with great fondness—is for change as ‘easy.’⁶⁸

Why should this issue have caused Schopf such concern? Ultimately, because it went to the heart of Schopf’s and Gould’s disagreement about the substance of the challenge to orthodox Darwinism paleobiology would make. According to Schopf, species are mathematically reducible entities, analogous to particles, and describable (potentially) by a set of paleontological gas laws. As Schopf’s letter to Gould continued, he explained “I am as convinced as I can be . . . that with 10^6 to 10^7 living species, and $\approx 10^{10}$ over geologic time, that species are particles in a never-ending biological world.” But this mathematical, particulate model had a major liability: geometry and mechanics tend to be paradigmatically *deterministic* sciences, and Schopf wanted to avoid determinism at all costs. So, as he explained to Gould, “in order to *avoid* Raup’s determinism . . . I am *forced* to a view that species durations must be quite

short ($\approx 2 \times 10^5$ yrs). If so, change must be easy.” According to Schopf, what the steady-state model demonstrates is that change (meaning speciation and extinction) occurs not because certain organisms are more or less fit, better or worse adapted, but rather because according to the predictable rules of equilibrium dynamics there will be a natural ebb and flow. The notion that species persist in stable form for tens of millions of years—as punctuated equilibrium suggests—is tantamount to capitulating to determinism; what, besides better fitness or greater adaptive value, could explain the sudden and rare evolutionary transformations the theory predicts?

Viewed from this perspective, it is clear that Schopf’s philosophical objection to punctuated equilibrium is that it is *not radical enough*. In another letter, Schopf spells out his concerns to his friend directly:

it seems to me, that much of the competition thinking is all wrong for understanding evolution. Sure ‘competition’ exists. But, every species is in some way a superior competitor *and* an inferior competitor. . . . If all species *through time* are equally ‘successful’—and if all species *at any given moment in time* are equally successful—then the notion of ‘success’ (*sensu latu*) has no place in evolutionary theory.⁶⁹

He continues, “as for punctuated equilibrium—goodness knows I have nothing against the rapidity of speciation. . . . But, the notion of stasis—that the mean duration of a species is millions and millions of years, then that becomes the MAIN SUPPORT for a deterministic view of life!” In this context, Schopf, the opponent of punctuated equilibrium and proponent of gradualism, is chiding Gould for insufficient radical fervor. “Maybe,” Schopf concludes, “you can find a middle ground. My sense of our field, such as it is, is that punctuated equilibrium becomes nearly the main argument for biological determinism. It would be ironic indeed, Steve, if you above all, in championing stasis, were seen in retrospect as the chief architect of biological determinism.” Whether or not Schopf’s own vision of the pattern of life’s history was misguided, his irony is a useful caution to the historian: as in the case of Gould’s relation to the principles of neo-Darwinism, whether one is “radical” or “conservative” is most definitely a matter of perspective.

NOTES

1. Gould 2002, 55. For reviews of *Structure*, see (among others), Barash 2002; Futuyma 2002; Ghiselin 2002; Hull 2002; Jablonski 2002; Stearns 2002; Wake 2002.

2. Gould 2002, 1005.

3. Schopf to Raup, March 7, 1970. Schopf Papers, Box 3, Folder 30.

4. Gould to Schopf, March 13, 1970. Schopf Papers, Box 5, Folder 14.

5. Eldredge and Gould 1972.

6. A word should be said here about Niles Eldredge’s contribution to punctuated equilibrium. As Gould himself acknowledged on many occasions, Eldredge invented the theory. While this is not the place to investigate the subsequent history of the theory’s association with Gould (often to the exclusion of Eldredge), it is important to state that this is an important story, and one that ought not be overlooked. For a more detailed examination of the content of the theory (and Eldredge’s contribution), see Patricia Princehouse’s contribution to this volume.

7. Gould and Eldredge 1977, 115.

8. Gould to Schopf, April 14, 1971. Schopf Papers, Box 5, Folder 14.

9. See Eldredge to Schopf, June 21, 1971. Schopf Papers, Box 4, Folder 12.

10. Schopf to Eldredge, June 30, 1971. Schopf Papers, Box 4, Folder 12.

11. Niles Eldredge, interviewed by David Sepkoski, 1/18/2006. During the review process, Gould left the United States for a sabbatical year at Oxford, leaving Eldredge to deal with the revisions. When confronted with Schopf’s criticism of the manuscript, Gould counseled Eldredge to not “give an inch,” as Eldredge recalls. “So he made me write a tough letter and I wrote a tough letter. And we prevailed, but he [Schopf] really hated it.”

12. Richard H. Benson, “Review of ‘Speciation and Punctuated Equilibrium: An Alternative to Phyletic Gradualism.’” Schopf Papers, Box 13, Folder 10.

13. Schopf to Raup, Gould, and Simberloff, April 20, 1972. Schopf Papers, Box 3, Folder 30.

14. Raup et al. 1973.

15. Gould To Schopf, December 6, 1976; Gould to Schopf, November 23, 1976. *Paleobiology Archives*, Box 3.

16. Stanley 1975, 648, writes “Macroevolution is decoupled from microevolution, and we must envision the process governing its course as being analogous to natural selection but operating at a higher level of organization.” Gould himself admits that Stanley “developed the implications that I had been unable to articulate from our original section on evolutionary trends” (Gould 2002, 980).

17. Gould and Eldredge 1977, 120.

18. Gould and Eldredge 1977, 146. This section, of course, contains the infamous admission that “it may also not be irrelevant to our personal preferences that one of us learned his Marxism, literally at his daddy’s knee.”

19. Further papers included Schopf et al. 1975, and Gould et al. 1977. See the contribution by John Huss to this volume (ch. 16) for a detailed analysis of the MBL project.

20. Gould to Schopf, December 6, 1976. *Paleobiology Archives*, Box 3.

21. Schopf to Gould, undated (early 1977). *Paleobiology Archives*, Box 3.

22. Gould 2002, 980.
23. Gould 2002, 1014–15.
24. Gingerich, “Review of ‘Punctuated Equilibrium,’” December 31, 1976. *Paleobiology* Archives, Box 3. The review does have a critical tone—particularly about the writing and potential biases of the authors—but does not raise substantive objections to the manuscript.
25. Gould 2002, 980. Eldredge recalls that the original 1972 paper “seemed to annoy virtually everyone,” but the first instances of criticism he cites do not appear until 1980 (Eldredge 2006, 35).
26. See, for example, Hallam 1978; Stanley 1978; Sepkoski 1978, 1979.
27. Raup, “Review of ‘Hierarchical Linear Modeling of the Tempo and Mode of Evolution,’” 1978. *Paleobiology* Archives, Box 4.
28. Bookstein, Gingerich, and Kluge 1978, 133.
29. Gould 1980b, 96.
30. In this sense, the term *ideographic* refers to information that is presented in a pictorial or figurative manner.
31. Raup and Gould 1974.
32. Gould 1980b, 98.
33. *Ibid.*, 1980b, 115.
34. Gould 2002, 1002.
35. Gould 1980a, 120.
36. Gould 2002, 1005.
37. Gould 1980a, 122.
38. Schopf 1981, 1982; Schopf, Hoffman, and Gould 1983.
39. Schopf 1981, 156.
40. *Ibid.*, 158.
41. *Ibid.*, 160.
42. *Ibid.*, 161.
43. Gould to Schopf, April 21, 1981. Schopf Papers, Box 8, Folder 30.
44. Anonymous, “Review of MS by Schopf.” Schopf Papers, Box 2, Folder 5.
45. Anonymous, “Comments on ‘A Critical Assessment of Punctuated Equilibrium,’” 1982. Schopf Papers, Box 2, Folder 5.
46. Davida E. Kellogg to Douglas Futuyma, March 18, 1982. Schopf Papers, Box 2, Folder 5.
47. Schopf, Hoffman, and Gould 1983, 438.
48. Schopf and Hoffman, “Punctuated Equilibrium and the Fossil Record—Draft A.” Schopf Papers, Box 9, Folder 133, p. 1.
49. Schopf and Hoffman, “Punctuated Equilibrium and the Fossil Record—Draft B.” Schopf Papers, Box 9, Folder 133, p. 1.
50. Schopf to Horowitz, August 15, 1982, and September 15, 1982. Schopf Papers, Box 9, Folder 106.
51. Schopf to Gould, September 19, 1982. Schopf Papers, Box 9, Folder 106.

52. Indeed, in his 1979 paper “Evolving Paleontological Views on Deterministic and Stochastic Approaches,” Schopf argued just this case for the history of the journal *Paleobiology*: “its establishment (if at all!), and subsequent development, were far from inevitable. . . . Yet I wager despite these words, some historian of paleontology a decade or so in the future will somehow say that ‘the time had come and the journal was inevitable.’ For those who were there, nothing could be further from the truth.” (Schopf 1979), 338.

53. Gould to Schopf, November 25, 1977. Schopf Papers, Box 5, Folder 14.

54. Mayr 1982, 1127.

55. In 1982, Mayr wrote to Schopf with his own personal feelings about punctuated equilibrium: “Since the Eldredge and Gould theory was expressly based on my 1954 paper [“Change of Genetic Environment and Speciation”] one might think that I would be completely behind the Gould theory. This, however, is only partially the case. I strongly object to the Goldschmidtian interpretation of rapid speciation in founder populations and I likewise do not agree with the complete stasis of other species.” Mayr to Schopf, February 9, 1982. Schopf Papers, Box 8, Folder 32.

56. Ruse 1989, 120.

57. *Ibid.*, 122.

58. *Ibid.*, 122.

59. *Ibid.*, 122.

60. *Ibid.*, 138.

61. Raup et al. 1973, 528.

62. Raup and Gould 1974, 307, 314, and 321. See also Gould et al. 1977.

63. Schopf to Gina Bari Kolata, July 2, 1975. Schopf Papers, Box 3, Folder 60.

64. Gould et al. 1977, 39.

65. This line is literally the last sentence of the paper.

66. Schopf to Raup, February 6, 1979. Schopf Papers, Box 3, Folder 30.

67. Gould 1982, 383.

68. Schopf to Gould, November 22, 1982. Schopf Papers, Box 9, Folder 106.

69. Schopf to Gould, December 4, 1981. Schopf Papers, Box 8, Folder 31.

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