Stephen Jay Gould

Reflections on His View of Life

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Editors' Preface

A teacher . . . can never tell where his influence stops.

—Henry Adams (1907, 300), used by Steve Gould
as an epigraph in *The Panda's Thumb*

Although Steve Gould's death on May 20, 2002, provided the immediate impetus for this book, its original motivation came from a review of his book *Structure of Evolutionary Theory*, published just before his death. That review—by someone who in our view clearly had no idea what punctuated equilibrium or species selection were about—suggested to us that Steve's science was even more widely misunderstood than we had thought. We said to each other at the time that someone needed to "do something" about this situation.

Steve's death took most of his students and close colleagues by surprise, although a few of us were aware that he had been ill. For many of us, it left a great hole in our lives. After his death and the several memorial services that followed, the three of us were asked to organize a symposium in Steve's memory at the annual meeting of the Geological Society of America, which convened on November 2, 2003. We invited students and close colleagues of Steve to participate in this symposium, asking each to explore an aspect of his thought from his or her own relatively "intimate" perspective—that is, from the point of view of one who had known well, learned under, and/or worked with him for many years. Our logic was that such people would be more likely to have a clearer-than-average understanding of his

thought and its significance. (Not all of the papers presented at that session are included in this book, and a few that were not presented have been added. Two were originally published elsewhere and are reprinted here.)

Steve Gould was a major and highly influential intellectual figure in science (particularly evolutionary paleobiology) and society over a span of about thirty years of his professional life. Indeed, some assessments during his lifetime deemed him the best-known scientist in the world; what other scientist, after all, merited a guest appearance on the television cartoon *The Simpsons*? Due to his prominence, a small Gould commentary industry had already become established prior to his death (e.g., Somit and Peterson 1992; Selzer 1993; Sterelny 2001). Furthermore, because Steve published two books (*Structure* [2002c] and *I Have Landed* [2001m], his tenth volume of essays from *Natural History* magazine*) in the months just before his death, a number of major review/essays on his life and work appeared around that time, supplemented after his death by various memorials, thereby expanding this industry considerably and laying a foundation for what may well be a significant Gouldiana literature in the future. An "essential" compilation of his writings has recently appeared (McGarr and Rose 2006), as well as an extract from *Structure* (Gould 2007), and at least one major biography is in preparation.

Despite such attention, the present volume is the first (and so far only) book to explore critically Steve Gould's numerous and varied scientific and intellectual contributions, what the connections among them are, and what their long-term impact may be on our understanding of the history of life. It is not a conventional memorial festschrift; such has been published elsewhere (Vrba and Eldredge 2005). It is also not (to use Dick Lewontin's phrase) a "compendium of encomia," nor (as Steve might have said, using one of his favorite words) an attempt at hagiography. Instead, we hope that this book is an informed yet honest assessment of Steve's contributions within the scientific, intellectual, and societal contexts of the late twentieth century. In some sense it is intended as a "reader's guide" to Gould.

*Throughout this volume, citations to publications by Gould himself refer to the cumulative bibliography at the end of the book.

Steve's work was widely quoted and criticized, but—at least in our experience—much less often read thoroughly and carefully and still less frequently fully understood. We would like to think of the essays here as written by "those who knew him best," but this would be presumptuous. We do think we knew him and his thoughts well, or at least a bit better than did most other scientists, including many of his critics. As the chapters of this volume demonstrate, however, familiarity does not necessarily breed agreement. In any case, we wanted to provide what we hope will be some perspective and clarity that we fear might be lost from the scientific community's understanding of Steve's contributions. We wanted to have our say, before the critics and "picklocks of biographers" (Benet 1930) have had their way with his legacy.

Most of the contributors to this volume were Steve's students, to whom he was first and foremost a teacher and mentor. He was not always warm or gentle, or even friendly, to his students, but he valued and inspired excellence, hard work, and accomplishment, and he stretched all of us farther than we thought we could go. He was indifferent to many of the things that excited us (as we were to many of the things that excited him). He was a difficult role model. He decided quickly whom he did and didn't favor, and you usually didn't get a second chance to make a first impression. He didn't always come to our talks at meetings or read our papers. But he worked hard to find us jobs, and he was always very generous to each of us—with his time (when we made appointments), his money, and especially with his mind. For some of us, he was among the most important influences in our entire lives. For all of us, our professional and personal lives are emptier now without him, and we are extraordinarily grateful to have known him well and to have been under his tutelage.

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1

The Structure of Gould

Happenstance, Humanism, History, and the Unity of His View of Life

Warren D. Allmon

I. Introduction

Once, in responding to critics who had attempted to link his views on another topic to punctuated equilibrium, Steve Gould wrote, "I do have other interests, after all" (1982f, 88; see also 2002c, 1005). This was of course very true. Steve read, thought, traveled, talked, and wrote across a wide expanse of time, space, and subjects. He sang Bach and Gilbert and Sullivan; loved architecture, baseball, and numerical coincidences; collected beautiful old books; met with the pope about nuclear war; corresponded with Jimmy Carter about God; once appeared on a TV talk show as an expert on conjoined twins; and published technical papers on allometry, snails, Irish Elks, eurypterids, pelycosaurian reptiles, clams, receptaculitids, the history of paleontology, and human cranial capacity. Despite this breadth, however, one of the central facts of his professional life was that essentially all of his interests were, proximately or ultimately, interconnected in a unusually coherent and explicitly stated intellectual view, not only of the history of Earth and its life but also of the philosophy of science and the nature of human thought.

Steve said as much. He described himself as an "urchin in the storm" for what he called his "personal, stubborn consistency of

viewpoint" (1987f, 11) and said that he regarded "the subject of worldviews, or paradigms," as essential "for the unification of all creative human thought…" (1995k, 104). In *The Structure of Evolutionary Theory* (2002c, especially 24–48), he laid out the connections between the various parts of his views,[1] and this did not go completely unnoticed by reviewers and commentators. Philosopher Michael Ruse (who seemed to understand Steve more than most critics), has described (1992, 1999) the connections among the several aspects of Steve's view of life, and after Steve's death, a few reviewers and eulogizers commented on the linkages within his distinctive world view (e.g., Durant 2002; Stearns 2002; Bradley 2004; York and Clark 2005).

By and large, however, critics and commentators have not delved deeply into the fundamental logic and interconnectedness of Steve Gould's oeuvre. This oversight is unfortunate because it is, in my view, only by understanding the internal structure and logic of the full swath of Steve's thinking and writing (as I suggest below, they're more or less the same thing) that we can fairly judge their utility and value as contributions to evolutionary theory and paleobiology, clearly the areas on which he wished to make his most lasting mark. If his ideas are atomized into their component parts, they can be too quickly judged and too easily discounted, misunderstood, or unfairly criticized.[2] It is only by connecting the conceptual dots among the various components that the potential value of his ideas can be evaluated fairly.

It is ironic that it is difficult for us to understand Steve's view of life, for perhaps more than any other scientist, he left us a roadmap to his thought. "Many scientists," comments David Hull, "possibly most scientists, just do science without thinking too much about it" (1999, 1131). Steve was not among them. He laid out not just the nature of his own biases and influences, but the nature of the biases and influences that must encumber all science. He was a tireless advocate for the view that science is an inescapably human activity, based in empirical observations of the natural world but never separable from human biases and preconceptions. His "favorite line" (1992 o; 1995k, 147) was from a letter Charles Darwin wrote to Henry Fawcett in 1861: "How odd it is that anyone should not see that all observation must be for or against some view if it is to be of any service!" and he was

constantly mentioning the tension between the subjective and objective sides of science. Some examples:

> Scientists often strive for special status by claiming a unique form of "objectivity" inherent in a supposedly universal procedure called the scientific method. We can attain this objectivity by clearly the mind of all preconception and then simply seeing, in a pure and unfettered way, what nature presents. This image may be beguiling, but the claim is chimerical, and ultimately haughty and divisive. For the myth of pure perception raises scientists to a pinnacle above all other struggling intellectuals, who must remain mired in constraints of culture and psyche. (1992o; 1995k, 148)
>
> Since all discovery emerges from an interaction of mind with nature, thoughtful scientists must scrutinize the many biases that record our socialization, our moment in political and geographic history, even the limitations (if we can hope to comprehend them from within) imposed by a mental machinery jury-rigged in the immensity of evolution. (1995q; 1995l, 345)
>
> An old tradition in science proclaims that changes in theory must be driven by observation. Since most scientists believe this simplistic formula, they assume that their own shifts in interpretation only record their better understanding of novel facts. Scientists therefore tend to be unaware of their own mental impositions upon the world's messy and ambiguous factuality. Such mental manipulations arise from a variety of sources, including psychological predisposition and social context. (2001m, 360–61)
>
> Our ways of learning about the world are strongly influenced by the social preconceptions and biased modes of thinking that each scientist must apply to any problem. The stereotype of a fully rational and objective 'scientific method,' with individual scientists as logical (and interchangeable) robots, is self-serving mythology. . . . This messy and personal side of science should not be disparaged, or covered up, by scientists for two major reasons. First, scientists should proudly show this human face to display their kinship with all other modes of creative human thought. . . . Second, while biases and preferences often impede understanding, these mental idiosyncrasies may also serve as powerful, if quirky and personal, guides to solutions." (1995k, 93–94)
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> When we recognize that we do not derive our concepts of history only from the factual signals that scientific research has extracted from nature, but also from internal limits upon the logical and cognitive modes of human thought, then we can appreciate the

> complex interaction of mind and nature . . . that all great theories must embody . . . [the idea] that mind and nature always interact to build our basic concepts of natural order—becomes especially relevant in our current scientific age, where prevailing beliefs about the sources of knowledge lead us to downplay the role of the mind's organizing potentials and limits, and therefore encourage us to regard our theories of nature as products of objective observations alone." (2001m, 280)
>
> Impartiality [in science] (even if desirable) is unattainable by human beings with inevitable backgrounds, needs, beliefs, and desires. It is dangerous for a scholar even to imagine that he might attain complete neutrality, for then one stops being vigilant about personal preferences and their influences—and then one truly falls victim to the dictates of prejudice. Objectivity must be operationally defined as fair treatment of data, not absence of preference. (1996j, 36)

Yet, even though he emphasized the cultural embeddedness of science, Steve was not a relativist or strict constructivist. He praised "the adamantine beauty of genuine and gloriously complex factuality" (2001m, 207), and stated his firm belief that "we have truly discovered—as a fact of the external world, not a preference of our psyches—that the earth revolves around the sun and that evolution happens" (1995k, 93). "Human thought," he observed, "unlike the evolution of life, does include the prospect of meaningful progress as a predictable outcome, especially in science where increasingly better understanding of an external reality can impose a fundamental organizing vector upon a historical process otherwise awash in quirks of individual personalities, and changing fashions of cultural preferences" (2002c, 591). In many respects, he said, "I remain an old-fashioned, unreconstructed scientific realist" (2002c, 969).

Steve, in other words, told us where scientific ideas in general—and his ideas in particular—came from. He assumed, however, that we were the "educated readers" whom he constantly strived to reach, and expected us to work a little bit to locate and grasp this roadmap—amid the more than 800 items in his personal bibliography (see page 335 of this volume) and/or within the 1,464 pages of *Structure* (2002c)—and most of us simply do not take the time to do so. As several commentators and reviewers have remarked (e.g.,

Orr 2002a; Wake 2002; Quammen 2003; Ayala 2005), it is tragic and ironic that his magnum opus—in which he really does lay all of this out and connect the dots—is so large and so baroquely written that few are likely to ever read it in full. *Structure* will, writes Stephen Stearns, "be bought more often than read and used as a bookend more often than as a book. Much of it deserves attention, some of it is exciting, and some of it is beautiful, but the gems are hard to locate amidst the sesquipedalian verbiage" (2002, 2339).

In short, I fear that Steve's ideas risk being discarded piecemeal or ignored in toto because there are just too many of them, and it is this fear, more than anything, that provokes this essay and also the organizing of this book. In this chapter, I attempt to extract and explicitly lay out the major connections among the components of Steve Gould's worldview. My analysis follows his advice to subject scientific texts to the same "textual analysis" as is common in the humanities (2002c, 521). I try to use his own approaches of "mini-biography" and "intellectual paleontology of ideas" (2001m, 5), which he used on so many other scientists, to elucidate why he came to the conclusions he did. Steve repeatedly railed against the "whig interpretation" of history and the "old style of condescension for an intellectual childhood to compare with our stunning maturity" (1995x; 1998x, 84; see also 1985r, 1991t, 1995p), in which "we commit the greatest of all historical errors: arrogantly judging our forebears in the light of modern knowledge perforce unavailable to them" (1998m, 2000k, 18). "The proper criterion [for judging someone's work]," he said, "must be worthiness by honorable standards of one's own time." (1993l, 186), and it is this perspective I try to take here.

More generally, because Steve was so conscious of these influences, his work is a rare and valuable opportunity to explore the internal and external dynamics of one scientist's effort to construct a coherent and comprehensive conception of natural science. Even though he famously became interested in paleontology at age five (when his father took him to the American Museum of Natural History), he also brought to his mature science a full set of personal beliefs, interests, and biases. As one tries to follow the coherence of his views, we can use his massive literary output to try to investigate to what degree these views may have come about because of, or been strongly affected by, nonscientific ideas. As he wrote in

Structure, "we do need to know why an author proceeded as he did if we wish to achieve our best understanding of his accomplishments, including the general worth of his conclusions" (2002c, 34).

A crucial element in this analysis (and, as he would undoubtedly have said, of productive scientific ideas in general) is that Steve ran his ideas out to their furthest logical limit, even if abundant empirical support was lacking. He referred to this phenomenon (in discussing the work of others) as the "overextension of exciting ideas" (2001m, 303; also 1997m, 326), and "the ultimate fallacy of claiming too much" (2002c, 667). Maynard Smith (1995) complained that when punctuated equilibrium was first put forward, "it was presented as just what one would expect to see if the orthodox view, that species often arise by rapid evolution in small peripheral populations, is indeed accurate. If only they [Eldredge and Gould] had left the argument there!" That they did not, however, is hardly surprising. Most, if not all, exciting new scientific ideas—from bacterial theories of disease to extraterrestrial impacts as causes of mass extinction—are rapidly applied (by their original authors or others) beyond their immediate beginnings. Indeed broad application and explanation of diverse phenomena is one measure of how useful a scientific theory is. In general defense of such extension of the theory of punctuated equilibrium in particular, Steve wrote, "proponents of punctuated equilibrium would become dull specialists if they did not take an interest in the different mechanisms responsible for similarities in the general features of stability and change across nature's varied domains, for science has always sought unity in this form of abstraction" (2002c, 765–66).

Neither this chapter nor this volume can claim to be a thorough analysis of Steve's thought. A minor "Gould industry," devoted to assessing his intellectual legacy, has already begun (e.g., Brown 1999; Ruse 1999; Morris 2001; Sterelny 2001; Orr 2002a; Shermer 2002; Grantham 2004; McShea 2004; York and Clark 2005; Sepkoski 2005; Lewontin 2008) and will, one hopes, continue; there is a posthumous "greatest hits" volume (McGarr and Rose 2006), and at least one major biography is in preparation. It is the fundamental point of this chapter (and most of the other contributions to the present volume), however, that these and future analyses of whether he was *right* must start with whether he *made*

sense. As he put it: "Brilliance, of course, only implies cogency, not correctness" (2002c, 585). My main concern here is not just whether Steve's views are true but that we understand them.

Here I argue that virtually everything that Steve ever wrote—which by his own account was a very large proportion of what he thought[3]—fits into a very clear intellectual framework set by a relatively small number of basic ideas, and that the connections between them—historic and intellectual—were and are very clear, and we can understand them better by exploring that framework explicitly.

II. Steve's *Weltanschauung* and its Discontents

A. His view of life

What was this coherent worldview? What was Steve Gould's "view of life"? To my knowledge, even in all of his voluminous writing, Steve never answered this in one succinct statement. But if he had, I think it might go something like this:

> Life and its history—indeed all of history—are highly and irreducibly complex, and dominated in most cases by unpredictable events. Stability results from structure, which results from this complexity; direction results largely from "random" events and unexpected outcomes, superimposed on—and usually dominant over—patterns created by deterministic processes; patterns of stability, complexity, and history create an inherently hierarchical structure that can only be understood hierarchically; change is often abrupt, disruptive, and unforeseeable in its consequences; progress and improvement in any kind of general sense do occur occasionally, but are not characteristic of most systems or intervals of history. Human evolution has proceeded along these lines as well; we are noteworthy for our consciousness, but are otherwise no different from any other species on Earth. Because our hubris has almost always incorrectly placed us outside and above the rest of nature, much of science consists of adjusting (usually diminishing) human status in the universe. Most of the various fascinating consequences of human consciousness are emergent properties of our brain's complexity; flexibility, contingency, and nondetermination are the hallmarks of our—and all other—evolutionary history. Human values are derived from this highly complex and contingent phenomenon of consciousness, and cannot be properly read, determined, or proscribed by or from

> any external reality or influence. Science is the best method that humans have so far invented to gain understanding of the natural world but, like all human endeavors, it is subject to human foibles which need always to be vigorously identified and countered if science is to progress.

He did, however, write a number of paragraphs from time to time that summed up much of this comprehensive view. Some examples:

> In our Darwinian traditions, we focus too narrowly on the adaptive nature of organic form, and too little on the quirks and oddities encoded into every animal by history. We are so overwhelmed—as well we should be—by the intricacy of aerodynamic optimality of a bird's wing, of by the uncannily precise mimicry of a dead leaf by a butterfly. We do not ask often enough why natural selection had homed in upon this *particular* optimum—and not another among a set of unrealized alternatives. In other words, we are dazzled by good design and therefore stop our inquiry too soon when we have answered, "How does this feature work so well?"—when we should be asking the historian's questions: "Why *this* and not *that*?" or "Why *this* over here, and *that* in a related creature living elsewhere?"... History's quirkiness, by populating the earth with a *variety* of *unpredictable* but sensible and well-working anatomical designs, does constitute the main fascination of evolution as a subject. (1994q; 1995k, 370–71)
>
> The course of evolution is only the summation of fortuitous contingencies, not a pathway with predictable directions.... [We should grasp] evolution as a process causally driven by struggle among individuals for reproductive success, and not by any principle working bountifully for the good of species or any other "higher" entity in nature. We may then view life's history as an unpredictable set of largely fortuitous, and eminently interruptible, excursions down highly contingent pathways. (1995s; 1995k, 332–33)

Both natural and human history were present in virtually every element of his work. Both of these spheres, in Steve's view, shared similar properties. Although both are subject to physical laws, both are histories and therefore constrained within the realm of the physically possible by what has gone before and subject to contingencies, the unexpected "quirks" of happenstance. As discussed

above, our struggles to understand both kinds of history are linked via the necessity of human foibles intervening in our comprehension; both are pursued by fallible and fascinating human beings.

B. At the center of the view: Punctuated equilibrium

Although it has scarcely been mentioned in reviews and commentary, I think that the semi-autobiographical section of *Structure* (2002c, 745–1024; esp. 774ff, 972ff) in which Steve describes the origins, logic, criticism, and history of punctuated equilibrium (hereafter, "PE") is among the book's most valuable highlights. Perhaps more than any other part of this frequently difficult-to-read book, it deserves almost all of its parentheticals, asides, and footnotes. As he obviously intended at least in part (e.g., 2002c, 973), I expect that it will be of great value to future historians of science, and it is the section that I imagine I will be assigning most often to future students, because it contains anything that they could ever conceivably want to know and can get nowhere else. (It has now very usefully been reprinted as a separate paperback volume; Gould 2007.) Most of all, the section makes clear the intellectual and "structural" core of *Structure*, and therefore of his view of life.

Steve was surprisingly inconsistent in acknowledging the central place of PE in his worldview. As illustrated by the quote in the first paragraph of this essay, he occasionally objected that he had "other interests." I am, however, much more persuaded by the realization he attributes to his friend Oliver Sacks who, he writes, "saw the theory of punctuated equilibrium itself…as my coordinating centerpiece, and I would not deny this statement." PE, Steve continues "stands for a larger and coherent set of mostly iconoclastic concerns…[it] led to the reformulation proposed herein for the first branch of essential Darwinian logic….these aspects of punctuated equilibrium strongly contributed to my developing critiques of adaptationism…my sources extended outward into a diverse and quirky network of concerns that seemed, to me and at first, isolated and uncoordinated, and that only later congealed into a coherent critique" (2002c, 37, 39–40).

PE was and is widely misunderstood, at least in part because it is both a narrow idea and also a platform for a much larger set of ideas, both a theory about how speciation looks in the fossil record

and also the basis for a much larger conception of how evolution works. Yet these dual roles are logically connected and can be understood if one tries to do so. As "a theory about the deployment of speciation events in macroevolutionary time," Steve said, "punctuated equilibrium explains how the sensible intermediacy of human timescales can yield a punctuational pattern in geological perspective—thus requiring the treatment of species as evolutionary individuals, and precluding the explanation of trends and other macroevolutionary patterns as extrapolations of anagenesis within populations" (2002c, 755–56).

Similarly, the origins of PE lie both in the details of paleontology and in the wider intellectual and scientific worldview. It was (and still is) based fundamentally on empirical observations about the fossil record, but it is also an obvious part of a much wider intellectual controversy over the nature of change. PE was both a reflection of these influences outside of paleontology, and the conduit for introducing and integrating them into what had been a relatively insular field. In their retrospective of PE on its twenty-first birthday, Steve and Niles Eldredge noted that PE arose within and was part of a distinctive cultural and intellectual milieu; modern science, they argued "has massively substituted notions of indeterminacy, historical contingency, chaos and punctuation for previous convictions about gradual, progressive, predictable determinism. These transitions have occurred in field after field. Punctuated equilibrium, in this light, is only paleontology's contribution to a *Zeitgeist*" (1993j, 227). This view was magnified in *Structure*:

> Punctuated equilibrium represents just one localized contribution, from one level of one discipline, to a much broader punctuational paradigm about the nature of change—a worldview that may . . . be judged as a distinctive and important movement within the intellectual history of the later 20th century. . . . For the punctuational paradigm encompasses much more than a loose and purely descriptive claim about phenotypes of pulsed change, but also embodies a set of convictions about how the structures and processes of nature must be organized across all scales and causes to yield this commonality of observed results. (2002c, 970)

Yet despite all of this apparent clarity, it is my disturbingly consistent observation that many of my colleagues, including perhaps a

majority of professors teaching paleontology, historical geology, and evolutionary biology, appear genuinely to misunderstand PE—where it came from, what it says, and what it implies. Steve used to say that there were two works that everyone talked about but no one read—the Bible and the *Origin of Species*. To this list we might justly add Eldredge and Gould (1972e).[4]

I do not wish to repeat the history or evidence for or arguments about PE here (see Geary, this volume). I would, however, make four points, which I think are important for a more general understanding of Steve's world view.

(1) PE came from a desire to unite paleontology with evolution. The origin of PE was closely tied to the aspirations of two young graduate students to prod paleontology out of the largely lethargic state in which they found it in the 1960s. Niles Eldredge and Steve Gould wanted to be paleontologists, but they also wanted to study the process of evolution. They were both clearly bothered by paleontology's poor reputation and frequently cited *Nature*'s summary: "Scientists in general might be excused for assuming that most geologists are paleontologists and most paleontologists have staked out a square mile as their life's work. A revamping of the geologist's image is badly needed" (Anonymous 1969). Yes, there were exceptions (Gould and Eldredge's advisor Norman Newell was a prominent one), but most invertebrate paleontologists were not well versed or even particularly interested in evolution at the time of the formulation of PE. (Some of the twentieth century's greatest invertebrate paleontologists never did write anything substantive on evolution.)

Recalling the origins of PE, Steve wrote that he and Eldredge "had been particularly frustrated . . . with the difficulty of locating gradualistic sequences for applying these [statistical] techniques, and therefore for documenting 'evolution' as paleontological tradition then defined the term and activity. When I received [Tom] Schopf's invitation to talk on models of speciation [at the 1971 national meeting of the Geological Society of America], I felt that Eldredge's 1971 publication had presented the only new and interesting ideas on paleontological implications of the subject—so I asked Schopf if we could present the paper jointly. I wrote most of our 1972 paper, and I did coin the term PE—but

the basic structure of the theory belongs to Eldredge" (2002c, 775). (See also Schopf [1981] for further details on the strikingly serendipitous origin of the 1972 paper.)

There is also another important factor to consider in tracing the origins of PE. As noted by Stearns (2002) and Orr (2002a), it is revealing that Steve (2001c, 967) says that Thomas Kuhn's *Structure of Scientific Revolutions* (1962) was among the most important influences on the development of PE, not just because it substantively describes a punctuated tempo of change in scientific theories, but also because it methodologically lays out a roadmap for *revolution* in scientific theories. Two smart young paleontologists saw an opportunity to shake up their field, to transform it, to shift its center of gravity from "handmaiden for geology" to the "high table of evolutionary biology." Although PE clearly was originally based on empirical patterns from the fossil record—and, in its initial formulation, proposes nothing beyond application of a particular theory of speciation to paleontological data—Gould and Eldredge quickly realized that it was also a logical basis for liberating paleontology from biostratigraphy, for an independent status of macroevolution as a subfield of evolutionary biology based in part on the unique contribution of their chosen field, paleontology. This is heady stuff, and in this context it can hardly be surprising that Gould and Eldredge sought to run PE out to its maximal logical extent.

As Steve puts it, he and Eldredge set out "to apply microevolutionary ideas about speciation to the data of the fossil record and the scale of geological time . . . to show how standard microevolutionary views about speciation, then unfamiliar to the great majority of working paleontologists, might help out our profession to interpret the history of life more adequately" (2002c, 775, 777–78). The theory's emphasis on morphological stasis was an "empowering switch" that "enabled paleontologists to cherish their basic data as adequate and revealing, rather than pitifully fragmentary and inevitably obfuscating." Paleontology could therefore "emerge from the intellectual sloth of debarment from theoretical insight imposed by poor data—a self-generated torpor that had confined the field to a descriptive role in documenting the actual pathways of life's history. Paleontology could now take a deserved and active place among the evolutionary sciences" (2002c, 778).

Understanding that it was among Gould and Eldredge's goals to use PE to "revolutionize" paleontology (and evolutionary biology) helps to account for much of the criticism that PE and its subsequent elaboration received, and much of Gould and Eldredge's response. As Steve frequently complained, critics variously claimed that PE wasn't true, wasn't original, or wasn't interesting, much less that it was revolutionary. This certainly must have touched a nerve in the two young would-be fire brands.

(**2**) **Steve caused a lot of his own problems.** Much of the criticism that PE received was (and is) unjustified, but some resulted from confusion sown by Steve himself. This was not, as has been claimed, because he was ducking and dodging, changing his views to fit whatever would work. It was largely because, as mentioned above, he rapidly ran PE to (and perhaps beyond) its logical extremes, and also because he used hyperbole and incendiary language, even when he should have known better.

Steve (2002c, 981–84) attributed much of the negative reception of PE to the media coverage of the 1980 Chicago Macroevolution Conference. Some of the press, he argues, connected disagreements over mechanism at the meeting to then-resurgent creationism, and "kindled the understandable wrath of orthodox Darwinians and champions of the Modern Synthesis" (2002c, 983). Yet Steve himself was responsible for at least some of the negative reception and in *Structure* he (perhaps a bit reluctantly) admits this. When he lists his and Eldredge's "own faults and failures," he says:

> critics can identify three sources of potential confusion that might legitimately be laid at our doorstep, and might have been prevented had our crystal ball been clearer . . . I did use some prose flourishes that, in a context of considerable suspicion and growing jealousy, probably fanned the flames of confusion. Although I never stated anything unclearly, and committed no logical errors that could legitimately have inspired a resulting misreading, I should have toned down my style in a few crucial places. . . . We may have sown some confusion by using partially overlapping terminology for a specific theory (punctuated equilibrium), and for the larger generality (punctuational styles of change) in which that theory lies embedded. But this taxonomic usage does stress a legitimate commonality that we wished to emphasize. (2002c, 1010–11)

Two statements in particular, made in papers in 1977 and 1980 as Gould and Eldredge were beginning to explore the wider implications of PE in earnest, came back to haunt Steve; these two statements became lightning rods and "sound bites" for critics, many of whom had never read or understood their original context.

(**a) The Synthesis is "effectively dead."** In a paper celebrating the fifth year of the journal *Paleobiology*, infamously titled "Is a new and general theory of evolution emerging?" Steve suggested that the Neodarwinian synthesis, "as a general proposition, is effectively dead" (1980c, 120). This provoked enormous criticism and a series of spirited specific rebuttals (see 2001c, 1004 for references). In *Structure*, he admits that, perhaps, he should have been a bit more circumspect:

> Given the furor provoked, I would probably tone down—but not change in content—the quotation that has come to haunt me in continual miscitation and misunderstanding by critics: "I have been reluctant to admit it—since beguiling is often forever—but if Mayr's characterization of the synthetic theory[5] is accurate, then that theory, as a general proposition, is effectively dead, despite its persistence as textbook orthodoxy" (Gould 1980, 120). (I guess I should have written the blander and more conventional "due for a major reassessment" or "now subject to critical scrutiny and revision," rather than "effectively dead."...Yes, the rhetoric was too strong (if only because I should have anticipated the emotional reaction that would then preclude careful reading of what I actually said). (2002c, 1007)

He protests, however, that

> Critics generally complete their misunderstanding of my 1980 paper [1980c] by first imagining that I proclaimed the total overthrow of Darwinism, and then supposing that I intended punctuated equilibrium as both the agent of destruction and the replacement. But punctuated equilibrium does not occupy a major, or even a prominent, place in my 1980 paper....I did speak extensively—often quite critically—about the reviled work of Richard Goldschmidt, particularly about aspects of his thought that might merit a rehearing. This material has often been confused with punctuated equilibrium by people who miss the crucial issue of scaling, and therefore regard all statements about rapidity at any

> level as necessarily unitary, and necessarily flowing from punctuated equilibrium. In fact…my interest in Goldschmidt resides in issues bearing little relationship with punctuated equilibrium, but invested instead in developmental questions that prompted my first book [1977e]….The two subjects, after all, are quite separate, and rooted in different scales of rapidity…I do strive to avoid the label of *homo unius libri*." (2002c, 1005)

Steve responds to Dennett's (1997) harsh criticism in much the same way. Dennett (1997) quoted from the infamous 1980 paper to support his claim that Steve had advocated for a "non-Darwinian saltation" as the "first step in the establishment of a new species." The passage quoted by Dennett: "Speciation is not always an extension of gradual, adaptive allelic substitution to greater effect, but may represent, as Goldschmidt argued, a different style of genetic change—rapid reorganization of the genome, perhaps non-adaptive" (Gould, 1980c, 119). Steve responds to what he calls Dennett's "pitiful" case by saying that "this quotation doesn't even refer to PE, but comes from a section of my 1980 paper on the microevolutionary mechanics of speciation" (2002c, 1009).

Yet despite his admission that his earlier rhetoric might have been a bit excessive and even confusing at times, it is striking that Steve continued even as late as 2002 to make exactly the same kinds of extreme statements. For example, he says that critics misinterpreted PE as having "something to say about evolution in general…[It doesn't,] for punctuated equilibrium only confirms all the beliefs and predictions of the Modern Synthesis" (2002c, 1000–1001). In a very narrow sense, this is correct, but both Gould and Eldredge clearly did (and Eldredge continues to; see, e.g., Eldredge 1995) think they had "something to say about evolution in general" and clearly implied that they thought what they "had to say" would, at least in part, transcend the Synthesis. Steve similarly claims that he never "made the Goldschmidtian link" (2002c: 1007), yet he so strongly implied it (in several places: 1977s; 1980v; 1982h) that only the most careful reader would have (at least initially) grasped his distinction.[6]

(**b) Marxism at his daddy's knee.** In their first major foray into exploring the wider implications of PE, Gould and Eldredge (1977; 1977c) included discussion of the cultural embeddedness of theory, contrasting the Victorian setting of Darwin's gradualism with other

possible cultural settings of punctuational styles of change. They concluded with what became one of the most-repeated Gouldisms: "It may also not be irrelevant to our personal preferences that one of us learned his Marxism, literally at his daddy's knee." (Gould and Eldredge 1977, 146). This statement too, was subject to wide citation and criticism.

In *Structure*, Steve reflected on the decades of opprobrium this line engendered by reviewing in some detail his and Eldredge's structuring of the passage:

> I do not see how any careful reader could have missed the narrowly focused intent of the last section in our 1977 paper, a discussion of the central and unexceptionable principle, embraced by all professional historians of science, that theories must reflect a surrounding social and cultural context. We began the section by trying to identify the cultural roots of gradualism in larger beliefs of Victorian society…We couldn't then assert, with any pretense to fairness or openness to self-scrutiny, that gradualism represents cultural context, while our punctuational preferences only record unvarnished empirical truth.…We therefore began by writing [p. 145] that "alternative conceptions of change have respectable pedigrees in philosophy." We then discussed the most obvious candidate in the history of Western thought: the Hegelian dialectic and its redefinition by Marx and Engels as a theory of revolutionary social change in human history.…But the argument required one further step for full disclosure. We needed to say something about why we, rather than other paleontologists at other times, had developed the concept of punctuated equilibrium. We raised this point as sociological commentary about the *origin* of ideas, not as a scientific argument for the *validity* or the same ideas.…So I mentioned a personal factor that probably predisposed me to openness towards, or at least an explicit awareness of, a punctuational alternative to conventional gradualistic models of change: "It may also not be irrelevant to our personal preferences that one of us learned his Marxism, literally at his daddy's knee."…I have often seen this statement quoted, always completely out of context, as supposed proof that I advanced punctuated equilibrium in order to foster a personal political agenda. I resent this absurd misreading. I spoke only about a fact of my intellectual ontogeny; I said nothing about my political beliefs (very different from my father's, by the way, and a private matter that I do not choose to discuss in this forum).

> I included the line within a discussion of personal and cultural reasons that might predispose certain scientists towards consideration of punctuational models. . . . In the next paragraph, I stated my own personal conclusions about the general validity of punctuational change—but critics never quote these words, and only cite my father's postcranial anatomy out of context instead: "We emphatically do not assert the 'truth' of this alternative metaphysic of punctuational change. Any attempt to support the exclusive validity of such a monistic, a priori, grandiose notion would verge on the nonsensical." (2002c, 1018)

Fair enough, but I am also reminded that Steve often said that one should look at the core of an argument, not the fine points, to get at what they really think. For example, he responded to critics of PE who suggested that the theory contained nothing new by complaining about "the frequent grousing of strict Darwinians who often say something like: 'but we know all this, and I said so right here in the footnote to page 582 of my 1967 paper . . . " (2002c, 1023). "General tenor," he said, "not occasional commentary, must be the criterion for judging a scientist's basic conceptions." (Gould and Eldredge 1993, 444). By this standard, it is I think safe to say that Steve wanted to push the comparison of PE to other punctuational ideas to the full extent possible, and he paid the price in criticism for occasionally going too far.

Steve's responses to criticism of these and other similarly inflammatory passages in his writing legitimately raise the question of how he could *not* have seen how potentially confusing such statements were. There are several possible explanations: (1) He did realize how provocative such statements would be and genuinely didn't care, and in fact intentionally intended to stimulate controversy. He did, after all, write that "iconoclasm always attracts me" (2001m, 369); (2) He made such statements unconsciously, later really did realize that he had made more than just a stylistic mistake, and "backpedaled hard" (Dennett 1995, 283–84); (3) He couldn't imagine that his readers wouldn't read carefully enough to understand the distinctions so clear in his own mind (and actually mostly there in what he wrote). I personally think it was some combination of the first and third of these. As I have already mentioned, Steve thought it was completely conventional and legitimate to rush to the logical boundaries with a new idea, test the theoretical limits, and

then pull back where needed; he said as much (albeit sometimes in fine print) and simply assumed everyone would understand.

(3) **The logic of PE (and its implications) is clear.** Anyone putting ideas into any public forum opens the doors for potential criticism. The more ideas you put out there—and the more iconoclastic and provocative they are—the more you risk being criticized. In his writing and conversation, Steve grouped criticism of him into three categories. The first was correction of empirical or objective points, which he said he not only accepted but loved. "The factual correction of error," he wrote, "may be the most sublime event in intellectual life, the ultimate sign of our necessary obedience to a larger reality and our inability to construct the world according to our desires" (1993l, 452).

The second was simple, personal nastiness, in the form of willful misrepresentation and snide remarks, which he said was deeply hurtful to him. (One of my clearest memories as a graduate student is that he advised a group of us one day, as we were discussing a paper highly critical of him: "when you go out into the world, don't engage in this kind of *ad hominem* attack.") The unfairness of much of this criticism has been cited by others (e.g., Ruse 2000; Wagner 2002). Steve attributed much of this kind of commentary to "little more than complex fallout from professional jealousy, often unrecognized and therefore especially potent" from "our most negatively inclined colleagues" (2002c, 1000).

The third was criticism that resulted from (conscious or unconscious) misunderstanding of what he had tried to say. Although some of the most severe and high-profile criticism focused around Steve's critique of adaptationism, particularly in sociobiology and evolutionary psychology (e.g., Davis 1984; Dennett 1997; Pinker 1997; Wright 1999), these issues are in my view epiphenomenal on the core of Steve's view, which is the theory of PE.

In my two decades of teaching, reading the technical literature, going to scientific meetings, and encountering professional colleagues, no single phenomenon has impressed, puzzled, and frustrated me more (aside from creationism) than the misrepresentation and misunderstanding of PE and its larger evolutionary implications. Not being a professional logician limits my ability to level a coherent technical critique of the responses I have

encountered. I can only say (in a statement that sounds so naive that I can hardly write it) that I simply cannot understand how something that appears to be so entirely logical to me can appear so otherwise to others.

This point is central to the argument and analysis of this essay. Putting empirics aside, the logical necessity of many if not most of the immediate implications of PE is compelling, if not indisputable, and this has been pointed out repeatedly by others (e.g., Hull 1980; Sober 1984; Lloyd 1988; Eldredge 1989; Vrba 1989; Lieberman 1995). The argument in its simplest form is as follows: If all or even most species in a clade are in stasis, then most evolutionary change in morphology is not occurring within species, and therefore must be occurring between species. If this is the case, trends must largely be the result of sorting among species, rather than extension and extrapolation of within-species anagenesis. This requires at least a modestly hierarchical view of the evolutionary process and an emphasis on speciation that the Modern Synthesis did not have. I don't see how it can be otherwise. PE, wrote Steve in summarizing this logic, "supplies the central argument for viewing species as effective Darwinian individuals at a relative frequency high enough to be regarded as general—thereby validating the level of species as a domain of evolutionary causality, and establishing the effectiveness and independence of macroevolution. Punctuated equilibrium makes its major contribution to evolutionary theory, not by revising microevolutionary mechanics, but by individuating species (and thereby establishing the basis for an independent theoretical domain of macroevolution . . . [This shift] ineluctably places much greater emphasis upon chance and contingency, rather than predictability by extrapolation." (2002c, 781–83) PE, Steve once said with succinctness, "leads to hierarchy, not saltationism" (1986a, 62). This argument says nothing about the validity of species selection, which I discuss further below.

Certainly the most quoted criticism of this argument—and of Gould as a scientist—came from John Maynard Smith (1995):

> Gould occupies a rather curious position, particularly on his side of the Atlantic. Because of the excellence of his essays, he has come to be seen by non-biologists as the preeminent evolutionary theorist. In contrast, the evolutionary biologists with whom I have

> discussed his work tend to see him as a man whose ideas are so confused as to be hardly worth bothering with, but as one who should not be publicly criticized because he is at least on our side against the creationists. All this would not matter, were it not that he is giving non-biologists a largely false picture of the state of evolutionary theory.

Ironically, Maynard Smith (1984) is also the originator of the much-repeated line that paleontology is once again at the "high table" of evolution, largely as a result of PE and its derivatives (see, e.g., Eldredge 1995; Ruse and Sepkoski 2008).

Other examples of the same genre of criticism appear in comments by other distinguished authors. Dan McShay (2004), for example, admits that he has "never been able to understand why species selection requires punctuated equilibrium," but worries "that this is my own obtuseness, because Steve and others seem so sure of the connection" (2004, 48). In his overview of Steve's career, Allen Orr (2002a, 137) complains that "it's hard to see what species selection has to do with punctuated equilibrium anyway." In a still more cluelessly critical vein, Mark Ridley writes in his review of *Structure*:

> According to Gould, the theory of punctuated equilibrium implies that species are individuals, not classes. But I do not see the logical connection. Evolution in general, not punctuated equilibrium in particular, is the reason species do not form classes. If anything, the relative constancy of species after their sudden origin would make them more like a class . . . again, I do not see that species selection follows from either punctuated equilibrium or the individuality of species. . . . Gould argues that punctuated equilibrium means that species are individuals and that the individuality of species enables species selection to operate. I have no problem with the three factual claims—of punctuated equilibrium, of the individuality of species, and of species selection. But I do not agree that the three are linked causally or conceptually. If they are not, Gould's system does not work. (2002, 11)

After years of attempting to rebut some of these critiques, Steve offered an analysis fully in line with his long-standing view of how science works. If smart people don't "get it," he said, then that is a sure sign that "it" is outside their conceptual worldview:

> I have long faced a paradox in trying to understand why many intelligent critics seem unable to understand or acknowledge our reiterated insistence that the radical claim of punctuated equilibrium lies not in any proposal for revised microevolutionary mechanisms . . . but rather at the level of macroevolution. . . . When smart people don't "get it," one must conclude that the argument lies outside whatever "conceptual space" they maintain for assessing novel ideas in a given area. Many evolutionists, particularly those committed to the strict Darwinism of unifocal causation at Darwin's own organismic level, or below at the genic level, have never considered the hierarchical model, and apparently maintain no conceptual space for the notion of effective selection at higher levels. (2002c, 1013)

I think Steve was correct in this critique. After all, he himself refers to hierarchy as the most difficult intellectual conundrum he ever confronted (2002c, 598). Yet I think his analysis is incomplete. It is true that most of his critics did not understand (or even try to understand) hierarchy, but they also did not grasp the other ramets of his worldview, and how they cohered into an overarching conception of nature.

III. Humanism

A. A humanistic naturalist

Although he did not say so frequently in his early work, by the end of his career Steve often identified himself with the humanities and the humanist perspective. For example, he wrote that he was "a naturalist by profession, and a humanist at heart" (2001m, 396); "I love, best of all," he said, "the sensitive and intelligent conjunction of art and nature—not the domination of one by the other" (1998x, 2). "If any overarching theme pervades this body of writing," he said about his *Natural History* essays, "I suppose that a groping effort toward the formulation of a humanistic natural history must unite the disparate" (1998x: 4). "I do love nature," he wrote, perhaps somewhat defensively, "as fiercely as anyone who has ever taken up a pen in her service. But I am even more fascinated by the complex level of analysis just above and beyond . . . that is, the history of how humans have learned to study and understand nature. I am primarily a 'humanistic naturalist' in this crucial sense. . . . That is, I am enthused by nature's constitution, but even more fascinated

by trying to grasp how an odd and excessively fragile instrument—the human mind—comes to know this world outside, and how the contingent history of the human body, personality, and society impacts the pathways to this knowledge" (1998x, 5).

Harvey Blume (2002) noted that "Gould's science and literary style owed more to art and artists than to algorithms." This humanistic interest had deep roots in his life. Steve's childhood was clearly one in which books and culture mattered a great deal. He says that he "shared the enormous benefit of a respect for learning that pervades Jewish culture, even at the poorest economic levels" (1999n, 8). These humanistic interests led him to become a double major at Antioch College in geology and philosophy, which in turn led him to the examination of two ideas that were to have major implications for his later work—uniformitarianism and form. As an undergraduate he wrote a paper on "Hume and uniformitarianism." "This work led me" he wrote, "to a more general analysis of the potential validity of catastrophic claims, and particularly to an understanding of how assumptions of gradualism had so stymied and constrained our comprehension of the earth's much richer history" (2002c, 44–45). Such thinking also clearly contributed to his predilection for "punctuated" patterns of change.

Although Steve's love of formalism and structuralism clearly had a basis in his empirical work, I think this interest was fundamentally based in his humanistic leanings. He loved D'Arcy Thompson's book *Growth and Form* (1942) and wrote his senior thesis on Thompson's theory of morphology (eventually published as 1971b). To the end of his life he remained proud of his first review article (1966c) on this subject, "written and published while I was still a graduate student" (2002c, 42). As a direct result, Steve then took up allometry for some of his first empirical and theoretical studies, fascinated by the problems of correlations of growth and the resulting structural constraints (e.g., 1968b; 1969g; 1971c and e). In 1970, he published a paper on form (1970c) that took a strongly adaptationist approach, much to his later dismay (2002c, 41). Yet most of his work on form focused on what he would come to call the formalist or structuralist perspective, or "laws of form"—the notion that growth itself, like history, was a powerful channeler of the potential directions of evolutionary change. Ultimately, this interest in form was to lead to what may well end up being one of

his most lasting and influential scientific contributions, *Ontogeny and Phylogeny* (1977e), as well as the famous Spandrels paper with Richard Lewontin (1979k). "I read the great European structuralist literatures in writing my book on *Ontogeny and Phylogeny*," he said; "I don't see how anyone could read, from Goethe and Geoffroy down through Severtzov, Remane and Riedl, without developing some appreciation for the plausibility, or at least the sheer intellectual power, of morphological explanations outside the domain of Darwinian functionalism" (2002c, 43).

This interest in structure and form of course vastly transcended its humanistic origins in Steve's thought to become a central feature of his view of the evolutionary process. Rules of structure, he wrote, "deeper than natural selection itself, guarantee that complex features must bristle with multiple possibilities—and evolution wins its required flexibility thanks to messiness, redundancy, and lack of perfect fit" (1993l, 120). Indeed, the very possibility of future evolution—what he called "evolvability"—depended in large part on the nonselective side consequences of these structural rules. (See Thomas, this volume, for further discussion.)

Beyond these specific scientific themes, Steve's interest in humanism infused everything he did with a panoramic view that virtually required him to connect science with art, literature, and history. His insistence on examining the history and social setting of ideas was not merely an antiquarian exercise but rather central to his view of how humans think. "I cannot imagine a better test case for extracting the universals of human creativity," he said, "than the study of deep similarities in intellectual procedure between the arts and the sciences" (1999c; 2001m, 51). Writing about Vladimir Nabokov, Steve said that the lepidopterist-novelist "sought to . . . illustrate the inevitably paired components of any integrated view that could merit the label of our oldest and fondest dream of fulfillment—the biblical idea of 'wisdom'" (1999c; 2001m, 51–52).

Steve's humanistic interests connected directly to his view of how science works. Because science is a human activity, examination of the human origin of ideas, particularly the personal background of the thinkers who developed them, was essential to a more adequate understanding of the ideas themselves. Some examples:

> Theory-free science makes about as much sense as value-free politics. Both terms are oxymoronic. All thinking about the natural world must be informed by theory, whether or not we articulate our preferred structure of explanation to ourselves.... Moreover, theory is always, and must be, colored by social and psychological biases of surrounding culture; we have no access to utterly objective observation or universally unambiguous logic. (1993p; 1995l, 419–20)
>
> Scientific progress depends more upon replacing theories than adding observations (and waiting until they coalesce into a proper explanation), and if all theories are bolstered by cultural biases, then any process of replacement requires an unmasking of previous structures. (1993p; 1995l, 420)
>
> Creative science is always a mixture of facts and ideas. Great thinkers are not those who can free their minds from cultural baggage and think or observe objectively (for such a thing is impossible), but people who use their milieu creatively rather than as a constraint.... Such a conception of science not only validates the study of history and the role of intellect—both subtly downgraded if objective observation is the source of all good science. It also puts science into culture and subverts the argument—advanced by creationists and other modern Yahoos, but sometimes consciously abetted by scientists—that science seeks to impose a new moral order from without. (1981e; 1987f, 103)

Also in line with his humanism was Steve's strident advocacy of interdisciplinariness, and his complaints about "the increasingly rigid and self-policed boundaries" (2001m, 29) between academic disciplines. (I was deeply disappointed when I left graduate school for my first job and discovered that the rest of academia did not share this commitment. Even now, when interdisciplinariness is more on the lips of administrators than ever, in practice it faces substantial obstacles in academic culture, and remains largely unencouraged and unrewarded.) He wrote numerous essays, as well as four books, on the connection of the arts and humanities with science (e.g., 1986m; 1992l; 1993h; 2000l). The master naturalist and traveler Alexander von Humboldt (1769–1859), said Steve,

> rightly emphasized the interaction of art and science in any deep appreciation of nature... this vision may now be even more important and relevant today.... For never before have we been surrounded with such confusion, such a drive to narrow special-

> ization, and such indifference to the striving for connection and integration that defines the best in the humanist tradition. Artists dare not hold science in contempt, and scientists work in a moral and aesthetic desert . . . without art. Yet integration becomes more difficult to achieve than ever before, as jargons divide us and anti-intellectual movements sap our strength. (2001m, 108)

It is ironic, given his profound humanistic interest, that Steve found such personal and intellectual delight in the dethroning and diminution of human status, of smashing the idols of our hubris, of passionately arguing that the world was not only not made for humans, but that it did not care for us at all. One of his favorite and most-repeated quotes was from Freud (1935) (Steve usually abbreviated it [e.g., 1995k, 325; 1996d, 17; 2001m, 217]; it is given here in full):

> Humanity has in the course of time had to endure from the hands of science two great outrages upon its naive self-love. The first was when it realized that our earth was not the center of the universe, but only a tiny speck in a world-system of a magnitude hardly conceivable; this is associated in our minds with the name of Copernicus, although Alexandrian doctrines taught something very similar. The second was when biological research robbed man of his peculiar privilege of having been specially created, and relegated him to a descent from the animal world, implying an ineradicable animal nature in him: this transvaluation has been accomplished in our own time upon the instigation of Charles Darwin, Wallace, and their predecessors, and not without the most violent opposition from their contemporaries. But man's craving for grandiosity is now suffering the third and most bitter blow from present-day psychological research which is endeavoring to prove to the ego of each one of us that he is not even master in his own house, but that he must remain content with the veriest scraps of information about what is going on unconsciously in his own mind. We psycho-analysts were neither the first nor the only ones to propose to mankind that they should look inward; but it appears to be our lot to advocate it most insistently and to support it by empirical evidence which touches every man closely.[7]

This statement, Steve said, "suggests a criterion for judging the completion of scientific revolutions—namely, pedestal-smashing

itself. Revolutions are not consummated when people accept the physical reconstruction of the universe thus implied, but when they grasp the meaning of this reconstruction for the demotion of human status in the cosmos" (1995s; 1995k, 325). This "pedestal-smashing" was an indelible and enduring element of Steve's thought and approach to intellectual life. The more disappointing to cherished human hopes an idea was, the more he liked it. In fact, he thought that an idea was truer *because* it was against our comfortable beliefs: "Most satisfying tales," he said, "are false" (1996o, 318). He did not find this attitude depressing in the least: "The deflation of hubris is blessedly positive, not cynically disabling" (2001m, 227). "The debunking of canonical legends…serves a vital scholarly purpose at the highest level of identifying and correcting some of the most serious pitfalls in human reasoning.…we like to explain pattern in terms of directionality, and causation in terms of valor. The two central and essential components of any narrative—pattern and cause—therefore fall under the biasing rubric of our mental preferences" (2000o; 2001m, 55–56).

Nature, to Steve, was one of innate unpredictability and twists and turns, not just regularity dictated by physical law (which of course he accepted). The idea that order could be created by "blind" natural selection delighted him. "How delicious," he gushed, "to contemplate that these 'benevolent' results [good organic design and harmonious ecosystems] arise only as side consequences of a mechanism operating 'below' divine superintendence, and pursuing no 'goal' but the selfish propagation of individuals—that is, organisms struggling for personal reproductive success, and nothing else" (1992s; 1995k, 341).

Although many other taxonomies would be just as fruitful, I think of Steve's humanism as falling into four broad categories: writing, human equality, religion, and the role of a public intellectual.

B. The republic of letters: Essays, books, and the status of scientific writing

Many commentators have said that Steve was as much (or more) a writer as a scientist; many critics of his science have agreed. Indeed, most of his obituaries and memorials cited his popular

writing as his signal achievement. If there was ever a scientist who demonstrated the truth of Canadian physician William Osler's observation that "In science, the credit goes to the man who convinces the world, not to the man to whom the idea first occurs" (Bean and Bean 1961, 112), it was Steve Gould. But Steve's life in letters was much more than his popularity as an essayist and best-selling "popular writer." His published legacy leaves us with a number of important unanswered questions about the nature of scientific writing. Some of these issues have already been subjected to textual analyses (e.g., Lyne and Howe 1986; Selzer 1993). Here I comment on three aspects of his writing: the connection between his popular and technical publications; the form of the essay; and his love affair with language and literature in general.

(1) **Popular vs. technical.** Steve said that he saw no distinction between his technical and popular writing, and intended his "popular" essays "for professionals and lay readers alike—an old tradition, by the way, in scientific writing from Galileo to Darwin, though effectively lost today." (1995k, xiv). He refused "to treat these essays as lesser, derivative, or dumbed-down versions of technical or scholarly writing for professional audiences." Rather, he said, he insisted on "viewing them as no different in conceptual depth (however distinct in language) from other genres of original research" (2001m, 6). Even one of Steve's harshest critics praised this feature of his writing: "he follows the admirable policy of writing at the same time for amateurs and professionals. I envy his ability to do this" (Maynard Smith, 1992).

There were strong similarities between the two kinds of work. For example, Steve noted that he had frequently presented in his "popular" essays "genuine discoveries, or at least distinctive interpretations, that would conventionally make their first appearance in a technical journal for professionals. . . . I have frequently placed into these essays original findings that I regard as more important, or even more complex, than several items that I have initially published in conventional scholarly journals" (2001m, 6–7). Also, in both popular and technical work he was "most moved by general themes," but found them "vacuous unless rooted in some interesting particular" (1987f, 10).

Yet the intellectual connection that Steve perceived between his popular and scholarly writing has a more general and provocative meaning and implication that all scientists should consider further. Steve's published work spans a continuum from Op-Ed pieces that were simply fun or completely nonscientific to peer-reviewed taxonomic monographs and dense philosophical analyses of hierarchy. He perceived no bright line between popular and nonpopular work, and his erudition and prolixity allowed him (much to the dismay of some critics) to get away with it. Most other scientists probably couldn't (or wouldn't) try to copy this model. Yet in an age of proliferating blogs, self-published books, and online databases, not to mention exploding volumes of knowledge and discovery (reviewed and nonreviewed) in all fields, the nature of "published" work is rapidly changing. As a confirmed and proud luddite, Steve mostly did not work in such a world, but he stretched the bounds of scientific literature in his own distinctive way. It is worth considering whether his hybridized style of nonpeer-reviewed but still scholarly publication (exemplified most notably by his *Natural History* magazine essays) may be a viable genre for future "scientific" work. Steve clearly wanted his colleagues to cite these pieces as primary literature. (I have done so several times, because they contain scientifically valuable insights, ideas, opinions, and the occasional genuinely new empirical discovery that are unavailable elsewhere; e.g., Allmon 2007.) Steve's "popular" essays were, however, only very infrequently cited by scientists as primary sources in the technical evolutionary literature (Ruse 1999), much to Steve's displeasure:

> I confess that I have often been frustrated by the disinclinations, and sometimes the downright refusals, of some (in my judgement) overly parochial scholars who will not cite my essays (while they happily quote my technical articles) because the content did not see its first published light of day in a traditional, peer-reviewed publication for credentialed scholars. (2001m, 6–7)

(2) The essay as scientific literature. Steve reveled in the essay as a literary form, repeatedly pointing out its venerable origins in the work of Michel de Montaigne (1533–92), whose *Essays* (1580) "defined as crucial to the genre . . . ordinary things (with

deeper messages)" (1995k, ix). He noted that the word "essay" is derived from a French word meaning literally "try" or "attempt" (2001m, 9). Each of his three hundred essays for *Natural History*, for example, were based on "a gem of a detail [which] always sought to ground a generality" (1995k, xi). And he had a rich storehouse of such gems. "I cannot forget or expunge any item that enters my head,"[8] he said, "and I can always find legitimate and unforced connections among disparate details. In this sense, I am an essay machine; cite me a generality, and I will give you six tidbits of genuine illustration" (1995k, xi–xii). Maynard Smith (1992) summarized nicely what I have frequently thought after reading a Gould essay: "they often tell me something that I ought to have known but didn't." Steve said he kept up his remarkable streak of monthly essays without a break in large part because he learned from them; they were voyages of personal discovery (as when he rediscovered a volume of Edmund Burke on his shelf as part of researching an essay on women natural history writers: "If I didn't write these monthly essays, Burke would probably have stayed on my shelf until the day I died" [1995k, 197]).

Yet Steve's essays were not in the "conventional" mode of the "natural history essay," that is more-or-less straightforward descriptive celebrations of the beauty and wonder of nature. His "personal theory about popular writing in science," he said, divided natural history essays into two modes: "Galilean [in recognition of Galileo's writing his major works in the vernacular Italian rather than the elitist Latin], for intellectual essays about nature's puzzles, and Franciscan [after St. Francis of Assisi], for lyrical pieces about nature's beauty." "I am, he said, "an unrepentent Galilean. I work in a tradition extending from the master himself, to Thomas Henry Huxley in the last century, down to J. B. S Haldane and Peter Medawar in our own. I greatly admire Franciscan lyricism, but I don't know how to write in that mode" (1994t; 1995k, 10).

This preference was not just an issue of literary style. It was connected to the fact that he "always found the theory of how evolution works more fascinating than the realized pageant of its paleontological results" (2002c, 38), and for humanistic and intellectual issues over what he called the " 'wonderment of oddity' or 'strange ways of the beaver' tradition" of essay writing (1995u; 1998x, 394). "Sorry to be so disparaging," he added parenthetically after this

revealing statement. "The stories are terrific. I just often yearn for more intellectual generality and less florid writing." "I would be an embarrassing flop in the Franciscan trade," he wrote elsewhere. "Poetic writing is the most dangerous of all genres because failures are so conspicuous, usually as the most ludicrous form of purple prose.... Cobblers should stick to their last and rationalists to their measured style" (1991a, 12–13). This style was also connected to his general lack of interest in ecology (see Allmon et al., this volume) and his long-standing critique of adaptationism and emphasis on contingency in evolution. "Nature writing in the lyrical mode," he said, "often exalts the apparent perfection and optimality of organic design. Yet... such a position plunges nature into a disabling paradox, historically speaking. If such perfection existed as a norm, you might revel and exult all the more, but for the tiny problem that nature wouldn't be here (at least in the form of complex organisms) if such optimality usually graced the products of evolution... optimality provokes wonder but provides no seeds for substantial change... Creativity in this sense demands slop and redundancy" (1990m; 1993l, 97–98).

Steve's writing style—in both his popular and technical writing—was an object of much praise and envy. Dust-jacket blurbs of his essay volumes lauded his "elegant prose," "wit and style," and "characteristically energetic, down-to-earth lucidity." Reviews cited glowingly the essays' "provocative and delightfully discursive" style (Wilford 1991) and Steve's "ability to astonish and amuse us" (Lehmann-Haupt 1980). Although he disagreed stridently with Steve's conclusions, even Richard Dawkins said he wrote well (e.g., 1990). John Updike (1985) observed that, although as Steve's career progressed he was "writing more lengthily but, my faint impression is, more felicitously." The essays were anthologized for undergraduate English classes in several colleges and universities (Palevitz 2002).

He was proud of being able to take technical scientific writing in directions it did not usually go: "for some perverse reason that I have never understood, editors of scientific journals have adopted several conventions that stifle good prose, albeit unintentionally—particularly the unrelenting passive voice required in descriptive sections, and often used throughout" (2000z, xii). In reviewing one of Steve's volumes of essays, Slobodkin (1988, 503) noted

one of the characteristics that distinguished his writing: "in most scientific prose the author strives for clarity in the dual sense of expository simplicity and in making oneself transparent so that the empirical world is visible through the text but the peculiarities of the author are invisible.... The uniqueness of Steve is that he dances between us and his subject."

Yet clearly not everyone liked Steve's style. Although one reviewer praised *The Flamingo's Smile* by saying that the standard Gould essay was "so clear that any educated person can read it and understand" (Glass 1987, 426), another expressed about the same volume a view that perhaps best epitomizes Steve's later writing and how it was received by many scientists: "Graceful these essays are not—there are too many digressions and flat-footed reiterations, too little concern for pace and rhythm and economy and polish. For all the precision of his thought and research, his syntax and language are sometimes confoundingly imprecise" (Quammen 1985).

By the end of his career, Steve's style had unarguably become more elaborate, reaching an apotheosis in his final works. *Structure*, in particular, was criticized first and foremost for its size: an "elephantine opus" (Quammen 2003, 74), and "heavy enough for a stewardess to have insisted that I store it in an overhead compartment for takeoff and landing lest it endanger the passengers" (Stearns 2002, 2339). Reviewers complained about its "almost pathological logorrhea" (Ridley 2002) and "remarkably undisciplined prose" (Orr 2002a, 133), and for undergoing almost no editing or peer review (Monastersky 2002; Ayala 2005, 113). The writing, said a reviewer, "is sometimes so verbose, convoluted, and digressive that sentences have to reread in order to understand their content" (Zimmerman 2003, 454). "Such billowing clouds of verbal flatulence," opined an even less kind commentator, "herald a new phenomenon—the literate bioterrorist—or maybe a biologically literate deconstructionist, more interested in generating complex clauses than in communicating anything." The book, he continued, "is too verbose, too densely written, too bombastic and self-referential...and too long." and "stands as a monument to good, professional editing, which it didn't receive. Gould—who famously refused to allow any modification of his unique prose—got his way at the end, and his book is the worse for it" (Barash 2002, 284).

In February 1993 I was presented for the first time with the harrowing and thrilling opportunity of formally reviewing one of Steve's papers (eventually published as 1994f), and I too noted the inflated prose with mild disapproval: "Although the discursive style is fun to read and informative," I wrote in my review, "I note the severe space limitations and resulting publication delays that currently plague this journal, and regretfully suggest that the text can be shortened by perhaps 20–30% without serious damage to its scientific content." (Most of the text was, in the end, published as originally written.) Scott Wing, former editor of the journal *Paleobiology*, said that he accepted writing from Steve that he "wouldn't tolerate from others." Steve's prose, said Wing, was like Russian dolls, "with parenthetical remarks within parenthetical remarks within parenthetical remarks" (Monastersky 2002, A18).

(**3) Literature for literature's sake.** When I was a graduate student (1982–88), Steve was just beginning to collect rare books seriously, and he kept many of them in his office where they were available to us students. Eventually, as his collection grew, there was a lock on the cabinet, and finally the rare books disappeared entirely when he moved to New York City to his famed loft lined with bookshelves (see, e.g., Stephens 1997). Most media accounts of his book collecting made him sound like just another eccentric Harvard bibliophile and did not communicate the core of his interest in antiquarian books. He did love them for themselves, but he also used them as primary sources for his research. Books, he wrote, reiterating a point I frequently heard him make in conversation, "are the wellspring and focus of our lives as scholars" (1987f, 10). Many of his essays were based on old books he purchased, in which he discovered marginalia or other ephemera, or from which he made new observations that spurred an insight (e.g., 1990o, 1993v, 1995w, 1997r, 1998s, t, 1999y, 2000p, 2000u).

He was enormously proud that his essays were based mostly on primary sources (in the original languages). This was not just an antiquarian concern; his insistence on tracking down original sources in the history of science resulted in what he viewed as several significant discoveries (e.g., 1988j, 1993s). Beyond this, he defended his obsession on the grounds of general scholarly integrity:

> Very few people, including authors willing to commit to paper, ever really read primary sources—certainly not in necessary depth and completion, and often not at all . . . yet another guarantee of authorial passivity before secondary sources, rather than active dialogue, or communion by study, with the great thinkers of our past. I stress this point primarily for a practical, even an ethical, reason. . . . When writers close themselves off to the documents of scholarship, and rely only on seeing or asking, they become conduits and sieves rather than thinkers. When, on the other hand, you study the great works of predecessors engaged in the same struggle, you enter a dialogue with human history and the rich variety of our intellectual traditions. You insert yourself, and your own organizing powers, into this history—and you become an active agent, not merely a "reporter." Then, and only then, can you become an original contributor, even a discoverer, and not only a mouthpiece. (1998x, 6)

Steve's considerable ability with languages was a point of great personal pride; "at a time when so few Americans can deal in anything but English . . . ," he said, "I can read the languages in which the main documents of evolutionary theory are written" (Monastersky 2002, A17; see also 2001c, 36).

C. They were despised and rejected: The fact of human equality

Steve clearly had a soft spot for the underdog, probably because he saw himself as one. He occasionally alluded in his essays to a childhood that included substantial abuse from his peers. For his childhood interest in dinosaurs, for example, he said he "was viewed as a nerd and misfit on that ultimate field of vocational decision—the school playground at recess. I was called 'Fossil Face'; the only other like-minded kid in the school [Richard Milner] became 'Dino' . . . The names weren't funny, and they hurt" (1995k: 222). Richard Milner recalls first meeting Steve when they were both twelve years old in the sixth grade in Queens. Milner described Gould as "a short, chubby, bright-eyed boy with a broad grin" and confirms that Steve hated his nickname, but said he accepted it "with good humor" (Milner 2002, 30). Another childhood acquaintance recalls Steve as a "chubby and somewhat awkward 14-year-old" (Mackler 2002). When I was a teaching assistant for him, I recall Steve objecting to our changing the grades of students who personally

complained to us because he said he had always been too shy to do so and thought there were many like him.

Beyond his individual experience, as a Jew (albeit a secular one), Steve viewed himself as a member of a sometimes disparaged and maligned group, and the history of discrimination, antisemitism, and immigration quotas was therefore very personal to him. His own ancestors had arrived from Hungary, Poland, and Russia during the first decade of the twentieth century, a fact that he frequently referred to in his writing. Referring to Henry H. Goddard (1866–1957), who argued for restrictions on immigration to the United States in the early twentieth century, Steve dedicated *Mismeasure of Man* (1981l, 1996j) "To the memory of Grammy and Papa Joe [his maternal grandparents], who came, struggled, and prospered, Mr. Goddard notwithstanding." He said that he wrote the book for reasons that "mixed the personal with the professional. I confess, first of all, to strong feelings on this particular issue. I grew up in a family with a tradition of participation in campaigns for social justice, and I was active, as a student, in the civil rights movement at a time of great excitement and success in the early 1960s" (1996j, 36).

Mismeasure was largely a critical success (but it also provoked enormous negative reaction; see, e.g., Jensen 1982; Carroll 1995; Rushton 1996, 1997) and Steve brought out a second edition with a new foreword (1996j), largely to respond to *The Bell Curve* (Herrnstein and Murray 1994). In a preview of the new edition, Steve made it clear what his ultimate fear about *The Bell Curve* was. The book, he said, presented an "apocalyptic vision of a society with a growing underclass permanently mired in the inevitable sloth of their low IQs. They will take over our city centers, keep having illegitimate babies (for many are too stupid to practice birth control), commit more crimes and ultimately require a kind of custodial state, more to keep them in check (and out of our high IQ neighborhoods)" (1994j).

The theme of human equality ran through many of his essays on human evolution, in which he pointed to "human equality as a contingent fact of history" (1984aa, 1997o) and the unreality of human races (1974r). These essays were also strong arguments for the a "bush" versus a "ladder" view of human evolution (see, e.g., 1976m, 1986a, 1987o, 1987p), and so a punctuational over

a gradualistic view of evolutionary change, as well as the powerful role of contingency in evolution in general. This entire line of thought was also closely connected to his critique of adaptationism, biological determinism, sociobiology, and evolutionary psychology in human biology which was also in turn connected to his critique of gene selectionism, and thereby to his thinking on hierarchical theories of evolution.

Steve was much struck by the implications the "great chain of being" (Lovejoy 1936) for both nonhumans and humans; ranking of nonhuman nature, he argued, led inevitably to ranking of humans (e.g., 1981l, 1983x, 1983y). He loved the English writer Alexander Pope (1688–1744) but shivered at one of his passages in *Essay on Man* (1734),[9] and its echoing by others, such as a now-forgotten female popularizer of conchology, Mary Roberts: "To this splendid superstructure [wrote Roberts in 1834], nothing can be added; neither can any thing be taken from it, without producing a chasm in creation, which, however imperceptible to us, would materially affect the general harmony of nature. All things were made by Him, and without him cannot any thing subsist; besides, it seems as if he designed to teach us by the admirable arrangement of his creatures, that the different gradations in society are designed by his providence, and appointed for our good" (1993r; 1995k, 196).

Summarizing his views on what he saw as the long and sorrowful legacy of human discrimination, he wrote:

> In many years of pondering over fallacious theories of biological determinism, and noting their extraordinary persistence and tendency to reemerge after presumed extirpation, I have been struck by a property that I call "surrogacy." Specific arguments raise a definite charge against a particular group—that Jews stink, that Irishmen drink, that women love mink, that Africans can't think—but each specific claim acts as a surrogate for any other. The general form of argument remains perennially the same, always permeated by identical fallacies over the centuries. Scratch the argument that women, by their biological nature, cannot be effective heads of state and you will uncover the same structure of false inference underlying someone else's claim that African Americans will never form a high percentage of the pool of Ph.D. candidates. (2001m, 352)

Steve wrote with particular passion when discussing anti-Semitism. At the end of an essay on an early interpretation of fossils that also included strong anti-Semitic statements, Steve wrote a compelling epitome of how his views of the relationship between science and nonscience connected to his views of human values:

> The improvement of knowledge cannot guarantee a corresponding growth of moral understanding and compassion—but we can never achieve a maximal spread of potential benevolence . . . without nurturing such knowledge. Thus the reinterpretation of jew stones as [fossil] sea urchin spines . . . can be correlated with a growing understanding that Jews, and all human groups, share an overwhelmingly common human nature beneath any superficiality of different skin colors or cultural traditions. And yet this advancing human knowledge cannot be directed toward its great capacity for benevolent use, and may actually (and perversely) promote increasing harm in misapplication, if we do not straighten out our moral compasses and beat all those swords. . . . into plowshares, or whatever corresponding item of the new technology might best speed the gospel of peace and prosperity through better knowledge allied with wise application rooted in basic moral decency. (2001m, 174)

D. The fullness of life: The roles and status of religion and science

Steve described himself variously as "a humanist and non-theist" (1995k, 40); "a Jewish agnostic" (1998x, 270); "a paleontologist by training, and with abiding respect for religious traditions" (2001m, 214); "not, personally, a believer or a religious man in any sense of institutional commitment or practice" (1998x, 281); and "an agnostic in the wise sense of T. H. Huxley, who coined the word in identifying such open-minded skepticism as the only rational position because, truly, one cannot know" (1999n, 8–9). That is, he clearly did not believe in a personal God or deity, but he was also closely tied to his own Jewish heritage. He relates that he "had no formal religious education," not even a bar mitzvah, because his "parents had rebelled against a previously unquestioned family background." They "retained pride in Jewish history and heritage, while abandoning all theology and religious belief." "In my current judgment," he adds parenthetically, "they rebelled too far" (1999n, 8).[10] From an early age, Steve valued the cultural role of religion,

but not its revealed or supernatural part. It is not possible, in my view, to understand Steve's views of religion and science unless one grasps his views on the source of human values and ethics, which in turn come out of this firmly Jewish and humanistic tradition.

At first glance, Steve appears to have shared completely the atheistic and materialist views of critics of religion such as Richard Dawkins and Daniel Dennett. (For example, he frequently said in lectures about the possibility of extraterrestrial life that astrobiology was similar to theology in that it was "a discipline with no subject matter.") He also repeatedly and stridently denied that science or nature (or, by implication, God) could be the source of human values or ethics, because almost *any* message can be (and has been) so derived: "answers to questions about ethical meaning cannot come from science" (1992w; 1995k, 75). "Nature simply is what she is," without any inherent moral or ethical message or signal for human life (2001m, 108–9). This philosophy was straight out of both Darwin and Enlightenment humanism: "When we stop demanding more than nature can logically provide . . . we liberate ourselves to look within" (2001m, 217–18).

> Our failure to discern a universal good does not record any lack of insight or ingenuity, but merely demonstrates that nature contains no moral messages framed in human terms. Morality is a subject for philosophers, theologians, students of the humanities, indeed for all thinking people. The answers will not be read passively from nature; they do not, and cannot, arise from the data of science. The factual state of the world does not teach us how we, with our powers for good and evil, should alter or preserve it in the most ethical manner. . . . the answer to the ancient dilemma of why such cruelty (in our terms) exists in nature can only be that there isn't any answer—and that framing the question 'in our terms' is thoroughly inappropriate in a natural world neither made for us nor ruled by us. It just plain happens. . . . If nature is nonmoral, then evolution cannot teach any ethical theory at all. (1982m; 1983d, 42–44)
>
> Once we recognize that the specification of morals and the search for a meaning in our lives cannot be resolved by scientific data in any case, then Darwin's variational mechanism will no longer seem threatening, and may even become liberating as a rationale for abandoning a chimerical search for the purpose of our lives, and the source of our ethical values, in the external workings of nature. (2001m, 248)

Yet Steve did not engage in the strident criticism of religion for which Dawkins and Dennett are well known (e.g., Dawkins 2006; Dennett 2006). Instead, he put forth what, on the surface, appeared to be a very different view, which he called "non-overlapping magisteria," or NOMA (1997n, 1999n). This view held that science and religion occupy separate but equal realms of human endeavor, or magisteria, and neither could or should make claims on the other's legitimate domain of influence. "No scientific theory, including evolution," he argued, "can pose any threat to religion, for these two great tools of human understanding operate in complementary (not contrary) fashion in their totally separate realms: science as an inquiry about the factual state of the natural world, religion as a search for spiritual meaning and ethical values" (2001m, 214).

Steve clearly saw this as a very important social issue: "People of goodwill wish to see science and religion at peace, working together to enrich our practical and ethical lives" (1999n, 4). "[T]he myth of a war between science and religion remains all too current, and continues to impede a proper bonding and conciliation between these two utterly different and powerfully important institutions of human life. How can a war exist between two vital subjects with such different appropriate turfs—science as an enterprise dedicated to discovering and explaining the factual basis of the empirical world, and religion as an examination of ethics and values?" (1994o; 1995k, 48–49). We need science to do what it does, he argued, but "We will also need—and just as much—the moral guidance and ennobling capacities of religion, the humanities, and the arts, for otherwise the dark side of our capacities will win, and humanity may perish in war and recrimination on a blighted planet" (2001m, 269).

> Science can supply information as input to a moral decision, but the ethical realm of "oughts" cannot be logically specified by the factual "is" of the natural world—the only aspect of reality that science can adjudicate. . . . I win my right to engage moral issues by my membership in *Homo sapiens*—a right vested in absolutely every human being who has ever graced this earth, and a responsibility for all who are able. If we ever grasped this deepest sense of a truly universal community—the equal worth of all as members of a single entity,

> the species *Homo sapiens*, whatever our individual misfortunes or disabilities—then Isaiah's vision could be realized, and our human wolves would dwell in peace with lambs, for "they shall not hurt nor destroy in all my holy mountain." We are freighted by heritage, both biological and cultural, granting us capacity both for infinite sweetness and unspeakable evil. What is morality but the struggle to harness the first and suppress the second? (1995k, 318)

NOMA, however, did not fare well among theologians or philosophers (see, e.g., Polkinghorn 1998; Haught 2000, 2003; Ruse 2000). The basic reason lay in Steve's definition of religion. In order to get religion to not conflict with science, said critics, Steve had to define religion in a way that excluded much of what religious people value, namely a caring God with supernatural powers. To make NOMA work, said theologian John Haught, for example, Steve had to "first reduce 'religion' to ethics" (2000, 25). Haught later elaborated on this critique: Steve could only reconcile science and religion, he said:

> by understanding religion in a way that most religious people themselves cannot countenance. Contrary to the nearly universal religious sense that religion puts us in touch with the true depths of the real, Steve denied by implication that religion can ever give us anything like reliable knowledge of *what is*. That is the job of science alone. As far as Steve was concerned, our religious ideas have nothing to do with objective reality. Scientific skeptics may appreciate religious literature, including the Bible, for its literary and poetic excellence. But they must remember that only science is equipped to give us factual knowledge. Doubters may enjoy passages of Scripture that move them aesthetically, or they may salvage from religious literature the moral insights of visionaries and prophets. . . . Still, Steve could not espouse the idea that religion in any sense gives us truth. No less than Dennett and Dawkins, when all is said and done, he too held that only science can be trusted to put us in touch with what is. At best, religion paints a coat of "value" over the otherwise valueless "facts" disclosed by science. Religion can enshroud reality with "meaning," but for Steve this meaning is not intrinsic to the universe "out there." It is our own creation. (Haught 2003, 6–7)

Some of these critics accused Steve, in his role as evolutionist laureate in the battle with creationism, of articulating NOMA in

part to make evolution more palatable to what he knew was a largely religious American general public. Perhaps this was indeed part of his motivation. Even if it was, however, this charge largely misses the main source of the view that NOMA represents: human values were, for Steve, no less real and "out there" than rocks or snails, but they could not be reduced to or directly determined *by genes*. They were for him, like so many other aspects of human consciousness, emergent (and contingent) epiphenomena of the incredible complexity of the human brain. Just because values and ethics are "our own creation," this did not for Steve make them less real.

Ultimately, and ironically, NOMA failed because it was an attempt to do what Steve consistently criticized in others: make reality match our hopes. His family background and intellectual leanings made him a nonbeliever, but his cultural heritage imbued him with a deep and heartfelt appreciation of the value of nonrevealed aspects of religion. His abiding humanism—perhaps combined with some (subliminal?) strategic spinning—compelled him to seek and find a personal reconciliation of science and religion, but the religion that he thought could coexist in such equality with science is a religion that few believers would accept (see Allmon 2009, for further discussion).

E. Intellectual adventures within ourselves: The role of the (public) intellectual

An important aspect of Steve's humanism was his self-conscious status as a scholar and (eventually a very public) intellectual (Lewontin 2008). His writing, particularly his popular essays, is filled with digressions and discursions about this topic. This self-appointed status made paleontologists both proud and embarrassed. Proud because, in many respects, Steve's conspicuous intellect brought out the best in us, encouraging us to be deeper scholars and to think about things in different ways. Sometimes, however, we were a bit reluctant to claim him because, although he was ours, he was sometimes, well, just a little much. Decades before his appearance on *The Simpsons*, for example, he was producing mixtures of admiration and dismay at our own professional meetings for his intellectual and rhetorical pyrotechnics. Two episodes in this category stand out in my memory. In 1985, in summing up

a professional short course on mollusks before a standing-room-only crowd at a major geology meeting, he discoursed at length on hyaena penises (see 1985d). In 1989, after listening to him give a major talk on the reinterpretation of the Burgess Shale (which was then so obsessing him) to a packed hall at an international meeting of evolutionary biologists, a senior colleague turned to me and said, with a combination of affection and bewilderment, "Steve is a caricature of himself."

More substantively, Steve's prolific exploration of the qualities of scholarship both in others and in himself provides a fascinating (and inspiring) case study of both the opportunities and pitfalls of such a broad and anastomosing view of the world. In an essay on Goethe as scientist, Steve quoted the German polymath in a passage that is remarkably applicable to himself. Quoting his own translation of Goethe (1831), he wrote that "a man of lively intellect feels that he exists not for the public's sake, but for his own . . . every energetic man of talent has something universal in him, causing him to cast about here and there and to select his field of activity according to his own desire" (1993l, 155). "The truly awesome intellectuals in our history," he wrote in another essay, "have not merely made discoveries; they have woven variegated, but firm, tapestries of comprehensive coverage. The tapestries have various fates: Most burn or unravel in the footsteps of time and the fires of later discovery. But their glory lies in their integrity as unified structures of great complexity and broad implication" (1993l, 125). "Good scholars," he said, "struggle to understand the world in an integral way (pedants bite off tiny bits and worry them to death). These visions of reality . . . demand our respect, for they are an intellectual's only birthright. They are often entirely wrong and always flawed in serious ways, but they must be understood honorably and not subjected to mayhem by the excision of patches" (1993l, 136).

Being such a consciously public intellectual, for Steve, also came with solemn duties, and he tried to imbue in his students a strong sense of scholarly obligation. It was incumbent on each of us, he said, to be a generalist without being a dilettante; to connect one idea with another in a world filled with dissociated information and academic over-specialization; to understand that the history of ideas matters as much as the ideas themselves;

and that you can and should be a teacher and a researcher and a communicator to the public, and in fact to be less is not to meet your obligations as a scholar. Our internal intellectual adventures were to be shared with others, and this simply came with the territory:

> Our greatest intellectual adventures often occur within ourselves—not in the restless search for new facts and new objects on the earth or in the stars, but from a need to expunge old prejudices and build new conceptual structures. No hunt can promise a sweeter reward, a more admirable goal, than the excitement of thoroughly revised understanding—the inward journey that thrills real scholars and scares the bejesus out of the rest of us. (2001m, 355)

Taking stands on important issues was also part of being an intellectual for Steve, and he took public positions and campaigned actively on at least four such issues: human equality, creationism, textbooks, and natural history museums. I have already discussed his stands on human equality. Steve's public crusading against creationism was even more famous (e.g., 1981f, 1982k, 1987i, 1987t). Philosopher Michael Ruse frequently described his experience with Steve during the 1981 Little Rock creationism trial: "For me these recollections epitomize what Stephen Jay Gould was all about: First, that he was there at all—many other prominent figures, beginning with Carl Sagan, had been too busy to take time out to go down to the South and fight the creationists. But Steve felt it was his public duty, and he never gave it another thought" (Ruse 2003). Historian of creationism Barbara Forrest similarly said of Steve that it was remarkable that a "person as important in science as he was thought it was worthwhile to get involved" in fighting the creationists. "He lent his reputation to get the attention in the media," she continued. "He did what I wish more scientists would do" (Palevitz 2002).

Steve waged a similar, if less spectacular campaign against copying in textbooks (1988j, 1990m; see also O'Keefe 2002, xv). He called biology textbooks "the most impenetrable and permanent of all quasi-scientific literatures" (2001m, 310). Copying by textbook authors was not only damaging because it led to persistence of errors. It also clearly offended him and his pride in using

primary sources. (He co-authored a textbook himself [1981c], but I heard him say several times that he didn't like the experience and would never write another.)

One campaign even closer to my own heart was Steve's championing of natural history museums. He admitted to "ambivalence . . . about the *Jurassic Park* phenomenon, and about dinomania in general" because it threatens to corrupt natural history museums with the promise of greater popularity and accompanying financial stability. "As a symbol of our dilemma," he observed:

> consider the plight of natural history museums in the light of commercial dinomania. In the past decade, nearly every major or minor natural history museum has succumbed (not always unwisely) to two great commercial temptations: to sell a plethora of scientifically worthless and often frivolous, or even degrading, dinosaur products by the bushel in their gift shops; and to mount, at high and separate admission charges, special exhibits of colorful robotic dinosaurs that move and growl but (so far as I have ever been able to judge) teach nothing of scientific value about these animals.[11] If you ask my colleagues in museum administration why they have permitted such incursions into their precious and limited spaces, they will reply that these robotic displays bring large crowds into the museum, mostly of people who otherwise never come. These folks can then be led or cajoled into viewing the regular exhibits, and the museum's primary mission of science education receives a giant boost. I cannot fault the logic of this argument, but I fear that my colleagues are expressing a wish or a hope, not an actual result, and not even an outcome actively pursued in most museums. (1995k, 235)

Steve consistently made an eloquent plea for natural history museums to do what they do best: to present and interpret authentic objects of nature. "It is our job," he said, speaking of natural historians and museum people:

> to stay whole, not to be swallowed in compromise, not to execute a pact of silence, or endorsement, for proffered payoff. The issue is more structural than ethical: we are small, though our ideas are powerful. If we merge without maintaining our distinctness, we are lost. . . . Our task is hopeless if museums, in following their essences and respecting authenticity, condemn themselves to marginality,

> insolvency, and empty corridors. But fortunately, this need not and should not be our fate. We have an absolutely wonderful product to flog—real objects of nature. . . . Luckily—and I do not pretend to understand why—authenticity stirs the human soul. The appeal is cerebral and entirely conceptual, not at all visual. Casts and replicas are now sufficiently indistinguishable from the originals that no one but the most seasoned expert can possibly tell the difference.

Our success," he concluded, in words that warm the heart of every natural history museum director, "cannot be guaranteed, but we do have one powerful advantage, if we cleave to our essence as guardians of authenticity" (1995k, 234, 236–67).

IV. History

A. Why study history?

My profession, Steve said, referring to paleontology, "embodies one theme even more inclusive than evolution—the nature and meaning of history" (1985z, 18). History, he said, "must not be dismissed as a humanistic frill upon the adamantine solidity of 'real' science, but must be embraced as the coordinating context for any broad view of the logic and reasoning behind a subject so close to the bone of human concern as the science of life's nature and structure" (2002c, 46).

Whether it was from the human side, or from that accidental encounter with the *T. rex* when he was five, Steve was clearly interested in history from a very early point in his life. This led him to what he called his "first two scientific commitments"—paleontology and evolution. He was, however, not just taken by history as narrative but as a fundamental process, and not just by the history of life but also the history of human thought about that life. All of these together comprised a single but multistranded web of connections throughout his thinking.

Steve's interest in history was at least threefold. First was the history of science. More than almost anyone else in paleontology and evolutionary biology, he was fascinated by the history of these fields, and this interest was clearly assuming a larger proportion of his attention at the end of his life. While many practicing scientists turn to the history of their field late in their careers, Steve viewed

the history of the discipline as an essential part of being an active practitioner within it, and he imbued his students with this view as well. I have frequently been struck since leaving graduate school by what an unusual view this is. Many, if not most, biologists and geologists know scarcely more about the history of their field than is contained in the obligatory first chapters of textbooks, and largely view the history of the field as a quaint antiquarian exercise. Steve, in contrast, saw it as central to good scholarship. Analysis of superseded world views, he argued, helps us to grasp the significance of the theories and ideas we now put forth. Examining the history of science, he said, allows us to see that smart people have struggled with issues that we might now think are solved. Historiography is thus an essential part of doing science today: "To unravel the archaeology of human knowledge, we must treat former systems of belief as valuable intellectual 'fossils,' offering insight about the human past, and providing precious access to a wider range of human theorizing only partly realized today" (2001m, 168).

Second, Steve was a relentless advocate for the intellectual value of the historical—as distinguished from the experimental—sciences. He argued that practitioners of fields such as paleontology, historical geology, evolutionary biology, and cosmology should never see themselves as pursuing less rigorous questions than students of more ahistorical fields such as physics or chemistry (e.g., 1986a, 1989d, 1994g, 1999b, 2001b).

> Historical science is not worse, more restricted, or less capable of achieving firm conclusions because experiment, prediction, and subsumption under invariant laws of nature do not represent its usual working methods. The sciences of history use a different mode of explanation, rooted in the comparative and observational richness of our data. We cannot see a past event directly, but science is usually based on inference, not unvarnished observation (you don't see electrons, gravity, or black holes either). (1989d, 279)
>
> The firm requirement for all science . . . lies in secure testability, not direct observation. . . . History's richness drives us to different methods of testing, but testability is our criterion as well. . . . We search for repeated pattern, shown by evidence so abundant and so diverse that no other coordinating interpretation could stand, even though any item, taken separately, would not provide conclusive proof. (1989d, 282)

> The common epithet linking historical explanation with stamp collecting represents the classic arrogance of a field [physics] that does not understand the historian's attention to comparison among detailed particulars, all different.... The historical scientist focuses on detailed particulars... because their coordination and comparison permits us, by consilience of induction, to explain the past with as much confidence (if the evidence is good) as Luie Alvarez could ever muster for his asteroid by chemical measurement,... We shall never be able to appreciate the full range and meaning of science until we shatter the stereotype of ordering [different scientific fields] by status and understand the different forms of historical explanation as activities equal in merit to anything done by physics or chemistry. (1989d, 281)
>
> The "lesser" status of historical science may be rejected on two grounds. First, it is not true that standard techniques of controlled experimentation, predictability, and repeatability cannot be applied to complex histories.... Nature... presents us with experiments aplenty, imperfectly controlled compared with the best laboratory standards, but having other virtues (temporal extent, for example) no t attainable with human designs. Second... [h]istory... is knowable in principle... testable, and different. We do not attempt to predict the future.... But we can postdict about the past—and do so all the time in historical science's most common use of repeatability.... Finally, history's richness drives us to different methods of testing, but testing (via postdiction) is our method as well. [Following Darwin, we look for a "concilience of inductions":]... types of evidence so numerous and so diverse that no other coordinating interpretation could stand—even though any item, taken separately, could not provide conclusive proof—must be the criterion for evolutionary inferrence. (1986a, 64–65)

This notion of the separate but equal status of historical science was put into practice in Steve's very successful course on the history of life at Harvard, which he taught for more than twenty years (see 1984g, and Ross, this volume).

Third, he was a tireless advocate for the importance of history itself as an essential element of the evolutionary process. History in his view was less the stately unfolding of a preordained or predictable course of events than a mostly unpredictable series of events that constrain (both positively and negatively) subsequent conditions and potential. All evolutionary biologists are taught that

evolution is Markovian, with each step depending on the previous one, but Steve internalized and then promulgated this notion to an extraordinary degree. History was for him virtually a thing, a force, like gravity. "History matters," he was fond of saying. By this he meant that history as sequence of events bestows on its products an inescapable (but largely unpredictable) legacy. It was this flow and power of history as a process that perhaps led him to focus more on how evolution works than on the specific organisms it produces (the "theory rather than the pageant" [2002c, 38]).

This interest in the importance of history in evolution was closely tied to Steve's critique of adaptationism and to his emphasis on imperfection and exaptation as sources of raw material for "evolvability." If natural selection was all-powerful, he argued, it would build whatever phenotype was required in an optimal way for local circumstances, and history would *not* matter. This, he held, was exactly what the extreme Darwinian selectionist position posited: "The most common denial of history made by self-styled Darwinian evolutionists resides in claims for optimality—conventionally for the mechanics of morphology, more recently for behavior and ecology" (1986a, 66). His interest in history was also the explanation for his admiration of French paleontologist Louis Dollo (1857–1931), famous for "Dollo's Law" of irreversibility in evolution. "Irreversibility" Steve said, was a profound "signature of history" (1993l, 92). He called Dollo one of his intellectual heroes, and maintained an active interest in his ideas throughout his career (e.g., 1970e, 1994e).

B. Ladders and bushes: The critique of progress

The "most fundamental question in palaeontology," Steve said, is "does the history of life have an intrinsic direction (toward greater morphological complexity, increased diversity, etc.)?" (1976c, 231; see also 1977b); that is, is there *progress*? It is hard to pick just one theme that Steve thought was more important than any other, but if one must choose, it would have to be the issue of progress in evolution. His view was unmistakable: "Progress is a noxious, culturally embedded, untestable, nonoperational, intractable idea that must be replaced if we wish to understand the patterns of history" (1988g, 319; see also 1996d).

Steve's critique of progress united many strands of his thought. The morphological stasis of PE implied a more limited role for conventional natural selection than the Modern Synthesis had suggested; combined with his interest in structuralism, as already discussed, this led to the critique of adaptationism (Gould and Lewontin 1979; 1979k), which implied that progress, in the sense of general improvement, was even less likely than Darwin thought. PE also implied that evolutionary trends are driven less by selection-driven anagenesis and more by sorting among species, leading to a view of evolution as more of a directionless "bush" than a unidirectional "ladder." Progress, furthermore, was (and still is) a deeply held Western cultural value, a source of personal and national purpose, meaning, and comfort. As such, it was in the crosshairs of Steve's intense distaste for any view that smacked of seeing in nature what makes us feel good: "ladders are culturally comforting fictions, and copious branching is the true stuff of evolution" (1993l, 67).

> If the purely adaptationist vision were valid, we might gain the comfort of seeing ourselves, and all other creatures, as quintessentially "right," at least for our local environments of natural selection. But evolution is the science of history and its influence. We come to our local environments with the baggage of eons; we are not machines newly constructed for our current realities. (1993l, 369)

The theme and phrasing of "ladders vs. bushes" were common in Steve's writing. "Many of my essays," he said, "stress this theme of mentally liberating bushes versus constraining ladders because I believe that no other misconception so skews public understanding of evolution" (2001m, 324). "Humans are not the end result of predictable evolutionary progress, but rather a fortuitous cosmic afterthought, a tiny little twig on the enormously arborescent bush of life, which, if replanted from seed, would almost surely not grow this twig again, or perhaps any twig with any property that we would care to call consciousness" (1995s; 1995k, 327).

Progress is also a problem for analyses in the history of science, Steve argued, since it implies "whig" and "presentist" views of the past. "Models of inevitable progress," he said, "whether for the

panorama of life or the history of ideas, are the enemy of sympathetic understanding, for they excoriate the past merely for being old (and therefore both primitive and benighted)" (1993l, 186).

C. History and hierarchy

By his own account, Steve realized in 1972 that PE implied a hierarchical view of evolution, but he and Eldredge didn't quite know what to make of it. As they worked through the implications of PE and ran them out to their logical conclusions, hierarchy came to dominate both of their thinking (e.g., 1982f, 1982g; Eldredge 1989, 1995, 1999). As was the case with so many other strands of his thought discussed here, Steve's passion for hierarchy clearly had both empirical and theoretical roots. Stasis obviously implies it, but hierarchy also likely appealed to Steve theoretically because it was yet another way in which history could really substantively matter in evolution. If all evolution is reducible to natural selection acting on individuals to optimize them for their present environment, then history is little more than a parade of perfection and strict reductive determinism. If, however, a variety of discrete processes act at different hierarchical levels (above and below the level of the individual), which are themselves produced by the historical sequences of evolutionary change, then history—and historical science—are essential elements of a full understanding of evolution. Hierarchy also appeared to offer the best opportunity for an independent macroevolutionary theory, based in paleontology, thereby fulfilling the ambitions of those two young Columbia graduate students.

Steve also found particularly fertile fodder for uniting these disparate strands in looking below the level of the individual, at the meaning of the growing tide of information from molecular genetics (see Dorit, this volume):

> The collapse of the doctrine of one gene for one protein, and one direction of causal flow from basic codes to elaborate totality, marks the failure of reductionism for the complex system that we call biology—and for two major reasons. First, the key ingredient for evolving greater complexity is not more genes, but more combinations and interactions generated by fewer units of code—and many of these interactions (as emergent properties, to use the technical

> jargon) must be explained at the level of their appearance, for they cannot be predicted from the separate underlying parts alone. So organisms must be explained as organisms, and not as a summation of genes. Second, the unique contingencies of history, not the laws of physics, set many properties of complex biological systems. Our thirty thousand genes make up only one percent or so of our total genome. The rest . . . originated more as accidents of history than as predictable necessities of physical laws. Moreover, these noncoding regions, disrespectfully called "junk DNA," also build a pool of potential for future use that, more than any other factor, may establish any lineage's capacity for further evolutionary increase in complexity. (2001m, 227)

D. Replaying the tape: The role of contingency

Durant (2002, 391) commented that Steve's first bout with cancer (1982) was "surely enough to persuade anyone of the importance of contingency in life," but Steve's interest in contingency clearly goes back much farther. At some point early in his career, Steve relates that his "general love of history in the broadest sense spilled over into my empirical work as I began to explore the role of history's greatest theoretical theme in my empirical work as well—contingency," which he defined as "the tendency of complex systems with substantial stochastic components, and intricate nonlinear interactions among components, to be unpredictable in principle from full knowledge of antecedent conditions, but fully explainable after time's actual unfoldings" (2002c, 47). Steve credited his graduate advisor Norman Newell's interest in sudden and catastrophic causes of mass extinction during the 1960s with stimulating his enthusiasm for the unpredictable effects of abrupt change (1998f).

An extremely important contributor to Steve's embrace of contingency was certainly what he frequently called the "MBL studies" (because much of the work was done at the Marine Biological Lab at Woods Hole, Massachusetts; see also Bambach, this volume). In the early 1970s a group that included David Raup, Thomas Schopf, Daniel Simberloff, Jack Sepkoski, and Steve worked on trying to specify how ordered phyletic patterns, as Steve wrote, "heretofore confidently attributed to selection for little reason beyond the visual appearance of order itself, could plausibly be

generated within purely random systems" (2002c, 27). These studies obviously affected Steve profoundly, leaving him "humbled by the insight that our brains seek pattern, while our cultures favor particular kinds of stories for explaining these patterns—thus imposing a powerful bias for ascribing conventional deterministic causes, particularly adaptationist scenarios in our Darwinian traditions, to patterns well within the range of expected outcomes in purely stochastic systems" (2002c, 43).

Contingency, said Steve, "embraces one of the deepest and grandest issues that we can fruitfully engage in science—the nature and status of history in comparison with the more conventional style of explanation by predictable and repeated occurrence under timeless and invariable laws of nature" (2001b, 195). It also became for him the epitome of the general effect of history on evolution. Around it he was eventually to integrate his critiques of progress, adaptationism, gradualism, predictability, and biological determinism, as well as his interests in evo-devo, hierarchy, constraint, unpredictability, and the dashing of the fondest of conventional human hopes. He acknowledged that all evolutionists accepted some role for chance; the difference was (as he often said) in relative frequency: "I envision," he said, "that almost every interesting event of life's history falls into the realm of contingency" (1989d, 290). "[M]any aspects of even the broadest patterning of life's history," he maintained, such as "why and when do multicellular organisms arise, why and when do mammals eventually inherit the environments of large terrestrial vertebrates from dinosaurs—fall largely (or at least importantly) into the domain of contingent explanation" (2001b, 197). He could wax especially lyrical about this perspective: "Contingency is rich and fascinating; it embodies an exquisite tension between the power of individuals to modify history and the intelligible limits set by laws of nature. The details of individual and species' lives are not mere frills, without power to shape the large-scale course of events, but particulars that can alter entire futures, profoundly and forever" (1993l, 77). "We tend to look at history," he said in a 1988 interview:

> as though it were a series of predictable optimal states, and that's where most of our problems come from. The real message of history is that you have this kind of massive contingency where everything

> that exists now is totally unpredictable. . . . I think that's the most important lesson in history, and I think it would help us understand why we live in a world where a lot of things don't make sense . . . that troubles us deeply, because our cultural biases lead us to think that things don't make sense. Maybe if we understood how history really works, we would realize from these massive ill-fittings that a lot of things really don't make sense. You don't have to try to explain everything that's troubling as though it really was good when it arose as a Darwinian adaptation. It's an adaptationist assumption that if we do anything, it must have its evolutionary source in something that was once right or appropriate. But it doesn't have to. (Batten 1988)

Contingency also became a focal point for the integration (by Steve and others) into paleontology one of the major events in late twentieth century geology—the increasing acceptance of nongradual, nonuniformitarian change (e.g., Kauffman 1987; Albritton 1989; Ager 1993). Steve was both observer and participant in the growing development and popularity of these ideas (e.g., 1965b, 1967d, 1975t, 1984h); he both reflected and helped to create the *Zeitgeist*. "This issue of uniformitarian vs. catastrophic change," he wrote later, "stands as one of the grand questions of science, for the debate pervades so many disciplines and bears so strongly upon some of the most profound puzzles of our lives" such as the nature of causality and the nature of change (1995k, 164). Although many developments contributed to this intellectual seachange, the single most important event was surely the Alvarez extraterrestrial impact theory for the end-Cretaceous extinction (Alvarez et al. 1980), and Steve was one of the first paleontologists to embrace this idea (e.g., 1984c, 1984x, 1985c, 1985j, 1986a, 1987x, 1989g).

It is difficult now to recall or understand what a boiling cauldron of scientific activity the early and mid-1980s were for much of paleontology (see, e.g., Glen 1994). The Alvarez hypothesis was greeted with great skepticism by most of the paleontological community and was hotly debated, in print and at professional meetings. It stimulated a burst of empirical work on the Cretaceous-Tertiary as well as other mass extinction events. Right on its heels was the 26 million year periodicity hypothesis (Raup and Sepkoski 1984), which in turn generated more controversy and a huge array of

other hypotheses about possible extraterrestrial causes of mass extinction (see Raup 1986). The early 1980s was also of course the time when punctuated equilibrium and its implications were being debated—in the wake of the 1980 Chicago Macroevolution meeting, Stanley's book on the same topic (1979), and Gould's highly provocative papers (1980b, 1980c, 1982f, 1982g). It was in this context that Steve assembled what can only be referred to as his own grand synthesis—of punctuation, mass extinction, contingency, and hierarchy.

The idea apparently began to develop in his mind at the 1983 annual meeting of the Geological Society of America meeting in Indianapolis, when he heard Adolph Seilacher present his "vendobiont" theory for the Ediacara fossils (Seilacher 1984, 1989). On November 9, just a few days after attending the meeting, Steve wrote an excited and revealing letter to Luis and Walter Alvarez, David Jablonski, David Raup, Seilacher, and Jack Sepkoski:

> Dear Luis, Walter, Dave, Dave, Dolf, and Jack,
>
> It all came together for me in Indianapolis, as this rather hastily written Natural History column (to appear next February [1984r]) will testify—and I want to thank all you gentlemen for the insights.
>
> I used to think (long ago and with my strong internalist, Platonist, D'Arcy Thompsonian biases) that mass extinction was just a whiz-bang phenomenology with no lasting importance (besides delaying things for a while each time) for patterns in the history of life. Then, I think, I just ignored it for a while, since I was so caught up in punctuated equilibrium as an unorthodox theory for pattern in normal times. Now you have all helped me to realize that it truly is a separate process, and a cardinal shaping force for patterns in life's history (Dave Raup, at least, will remember my old argument that "vectors" in life's history (or their non-existence) has always been the fundamental question of paleontology.). And we do not have a general theory for it as yet. All this taken together must constitute the chief excitement of paleobiology for the near future at least. I have hardly begun to consider all the implications, but I do think that we finally have the basis to grope for a general theory of pattern by considering unorthodox processes both for normal times (if punctuated equilibrium has any lasting meaning it will be here) and for mass extinctions. Their interaction must be

> the dominant generator of pattern. I'll bet that most of microevolutionary thought for Darwinian transformation of local populations won't be outstandingly relevant. What do you think?
>
> Sincerely,
>
> [signed] Steve

The eventual result of this epiphany was "The paradox of the first tier: An agenda for paleobiology" published in the tenth anniversary issue of *Paleobiology* (1985f). This paper was, in my view, the most bold, coherent, logical, elegant, extreme, and overreaching technical paper Steve ever wrote.[12] It pulled together a huge array of ideas and hypotheses, some well founded and others at the time only tenuous, into a single overarching hierarchical view of evolution, from the ecology of natural selection in local populations, to the effects of periodic mass extinctions separated by tens to hundreds of million years. Closely behind this impressive hierarchical edifice came Steve's provocative, controversial, and influential explication of the reinterpretation of the Burgess Shale (1985x, 1986q, 1989d, 1990q, 1991i, 1992k, 1992m, 1993k), which claimed that contingency had played a, if not *the*, dominant role in sorting out the survivors of the Cambrian explosion from the less-fortunate "weird wonders" so beautifully preserved in this extraordinary fossil deposit. Altogether, this work solidified "a worldview that celebrates quick and unpredictable changes in a fossil record featuring lineages construed as largely independent historical entities." Steve added tellingly that he found "such a world stunning and fascinating in its chaotic complexity and historical genesis;" he said he would "happily trade the comforts of the older view for the joys of contemplating and struggling with such multifarious intrigue" (1995k, 103).

Steve also frequently wrote about the role of contingency in intellectual history, for example for typewriter keyboard arrangements (1987k) or scientific illustrations (1999x), and he noted approvingly (2001b) the increasing interest in contingency as an important factor among scholars of human history (e.g., McPherson 1988). The recently successful "what if" volumes of popular history (e.g., Cowley 1999) similarly make use of a perspective sometimes called the "counterfactual" to explore the possibly large implica-

tions of small events that might have happened differently in the past. Such arguments are not advocating total nondeterminism or "randomness" as causes of history; they are simply making the case that Steve made repeatedly: that unpredictable, unique, historical events, *by their very nature*, will exert a much stronger effect on the ultimate course of future history than most Western historians (and scientists) had previously acknowledged, and that historians (and paleontologists and evolutionary biologists) ignore such events at their peril.

E. The critique of determinism

In the minds of many biologists, Steve Gould was mostly seen, and is now mostly remembered, for his strident criticism of sociobiology, and its descendants evolutionary psychology and human evolutionary ecology (e.g., 1969c, 1974f, 1974s, 1974t, 1976n, 1978g, 1978h, 1979v, 1980x, 1983e, 1983m, 1984m, 1984w, 1994j, 1995n; see also Kitcher, this volume). I am neither qualified nor inclined to analyze the substantive details of either side of what was frequently a nasty debate. Here I wish only to point out that Steve's critique of sociobiology was, like almost every other facet of his intellectual life, closely and logically connected to multiple other interests and themes. As I have already discussed, he clearly had personal political and social views that were at odds with those of some advocates of sociobiology, and the effects of this disagreement cannot be discounted. He also, however, disagreed with sociobiology's focus on adaptative explanations for aspects of human behavior for the same reason he critiqued all such applications of "the adaptationist program"—because in his view they took too little account of the nonadaptive, historically contingent features of organisms that he thought were the crucial stuff of much evolutionary change. He disagreed with the application of hereditarian interpretations of discrete measures of human intelligence (such as IQ) for the same reason, because he thought that many features of organisms, including much of what we call human consciousness and intelligence are emergent characteristics of the highly complex human brain that evolution, probably mainly by natural selection, had built for other reasons.

V. Ever since Steve: Assessing a legacy

Steve Gould thought and wrote more than most practicing scientists about what controls the ultimate historical fates of a particular scientist's intellectual legacy. In one of his own favorite essays, for example, he describes the career and afterlife of paleontologist Nathaniel Southgate Shaler, who in the late nineteenth century was "by far, Harvard's most popular professor," and who, thirty years after his death, "at the Harvard tercentenary of 1936 . . . was named twelfth among the fifty people most important to the history of Harvard" (1988q; 1991a, 313). Yet today, he is virtually unknown. "Why has he faded," Steve asks, "and what does his eclipse teach us about the power and permanence of human thought?" (1988q; 1991a, 318). Steve provided no unambiguous answer, except to note that, unlike Shaler, who more or less stuck to his mentor Louis Agassiz's views of divine direction in the history of life, his friend and fellow Harvard professor William James—"one of America's great gifts to the history of human thought"—"questioned Agassiz from day one . . . probed and wondered, reached and struggled every day of his life" (1988q; 1991a, 318–19). Steve's clear implication is that iconoclasts will ultimately prevail.

As is true elsewhere in this essay, I cannot make a thorough analysis or a confident judgment. I will only note three aspects of analysis of the Gouldian legacy that future historians (and scientists) might want to keep in mind.

Changes

As Stearns (2002, 2339) nicely puts it, Steve "deserves quite a bit more credit than his severest critics would grant (zero)" but less than Steve himself would award himself "(a great deal indeed)." It is a fact of the current state of evolutionary biology that Steve left a significant legacy of substantively changed views. These changes (for which he can take at least partial credit) include at least the following:

(**1**) **Stasis.** Although it remains difficult to put a firm number on its frequency, it is clear that morphological stasis is widespread in the fossil record, at least in many groups of benthic marine macroinvertebrates, and perhaps in many other groups as well, and may well be predominant in many clades under most circumstances.

This was not predicted by the Modern Synthesis and was almost wholly unknown or unappreciated prior to 1972. It is simply not, in my experience, true that, as Orr put it recently: "By the nineties, most evolutionary biologists had simply stopped paying attention to punctuated equilibrium. . . . Punctuated equilibrium was down, if not out" (2002a, 136). On the contrary, it has, at least in part, become integrated into the evolutionary canon (e.g., Price 1996, 367–74; Freeman and Herron 1998, 475–81; Futuyma 1998, 689–94; Stearns and Hoekstra 2005, 433–34).

(**2**) **Evo-devo.** *Ontogeny and Phylogeny* was prescient and influential in its emphasis on the developmental basis for evolutionary change. Stearns (2002) notes that structuralism was largely lacking from the Modern Synthesis and says that Steve was correct to emphasize its importance. In doing so, says Stearns, Steve "did play an important role in preparing the anglophone community to receive the results [of molecular developmental genetics; "hoxology" as Steve called it], to know why they were important, and to place them in the context of historically significant questions." But, argues Stearns, the continental Europeans didn't need any such preparation, and therefore, if Steve "had never existed, I suspect that the field of evo-devo would have been in approximately the state today that it actually finds itself in," although the history of evo-devo that we experienced was "more interesting and colorful . . . because of him even if we could have gotten there without him" (2002, 2343).

(**3**) **The Softening of the Synthesis.** Adaptation and natural selection are still at the core of modern evolutionary biology. As Orr recently begins a technical paper: "Evolutionary biologists are nearly unanimous in thinking that adaptation by natural selection explains most phenotypic evolution within species as well as most morphological, physiological, and behavioral differences between species" (2002b, 1317). It is also, however, widely (if not always loudly) acknowledged that a substantially greater diversity of views about evolutionary processes is acceptable today compared to a generation ago, and this is in part clearly due to Steve's influence. Orr himself acknowledges that "Gould's attacks on adaptationism may have been extreme, but fanciful Just So stories are now, thankfully, rarer" (Orr 2002a, 138). Similarly, as Stearns (2002) eloquently puts it:

> [t]he complacency and rigidity of evolutionary biology in the 1960s were real. The consistency of evolutionary phenomena with population genetics was incorrectly extended to a general belief that population genetics was sufficient to account for evolution. This gave population genetics a privileged position as the standard against which evolutionary thought should be measured, and it created an atmosphere in which important evolutionary phenomena not directly tied to genetic mechanisms were often defined away or ignored, to the great frustration of those interested in them. . . . Steve's greatest contribution was his effectiveness in shattering the complacency of the field and broadening the range of respected discourse. . . . He was a real leader in opening our minds to important things that had been missed, and he did our field a great service in reminding the public that there is more to biology than molecular biology and that there are interesting unanswered problems whose solutions will not require DNA sequences. (Stearns 2002, 2345)

Species selection

While the abundant evidence for stasis provides ample empirical confirmation of at least a core of PE, the continuing paucity of evidence for species selection, after more than a quarter century of searching, appears to me to be a serious problem, one that Steve did not adequately acknowledge and in fact in *Structure* rhetorically obfuscates. (I do not intend to discuss the details of species selection here, but only to comment on the style of Steve's argument.) The logic of species sorting as a result of PE is clear, as is the meaning of emergent characters (and/or emergent fitness; see 2001c, 658–59). Yet, as Steve says, "accepting a common logic but challenging the empirical importance of legitimate phenomena [is] a good substrate for productive debate in science" (2001c, 646). As several commentators (e.g., Erwin 2004) and Steve himself have noted, species selection simply does not have many empirical examples. Steve, however, thought that this argument is "unfair," and noted (correctly) that "a few excellent (and elegant) cases have been well documented, so this process cannot rank as a distant plausibility waiting for an improbable verification, as some critics have charged" (2001c, 709). The fact, he argues, that "well-documented cases of species

selection do not permeate the literature" is because "[w]e have barely begun to acknowledge (much less to define or operationalize) this process, and we have still not entirely agreed upon the criteria for recognition" (2001c, 710).

Perhaps this is true, but in fact there are by my count *exactly three* well-documented and widely accepted examples of species selection, and all are more than twenty years old (Vrba 1980; Hansen 1980, 1982; Jablonski 1987). Many other hierarchy-imbued Gould students have been out there working with our respective empirical baliwicks and have yet to identify even additional possible examples. It is true that we did not (until *Structure*) have an explicit cookbook of criteria for searching for such examples, and this may have had some dampening effect, but I (as a fairly sympathetic observer) still find the paucity of evidence to be at least strongly suggestive evidence of paucity.

What is most interesting in my present context is how Steve treats this situation in his lengthy discussion of species selection in *Structure*. Ironically, he gives us a roadmap for analyzing such a situation in his own critique of Dawkins's gene selection: "When the logic of an argument requires that the empirical world operate in a certain manner, and nature then refuses to cooperate, unwavering supporters often try to maintain their advocacy by employing the tactic of conjectural 'as if,'" or ceteris paribus (2001c, 628). This is exactly what Steve does for species selection, relying on a highly detailed (and, as far as I am concerned, completely reasonable) theoretic analysis of the logic of higher order selection to say that it simply *should* be out there. When it comes to discussing the empirical record, however, he frankly stretches our credulity when he uses rhetoric more suitable for a much larger dataset: "our best examples of species selection," he says, "work through differential rates of speciation rather than varying propensities for extinction" (2002c, 649–50), making it sound like there are enough to really make such a distinction. Similarly, in referring to the widely cited example of different larval strategies in Cenozoic volutid gastropods (Hansen 1980, 1982), Steve calls it "a classic example, much discussed in the literature," when in fact it is arguably no more than the best of a tiny number of examples.

It is a further irony that he makes these rhetorical special pleas, because elsewhere he made just the reverse argument when

discussing the occurrence of directional trends in the fossil record. A "case or two in the fossil record does not establish a pattern," he says. "Directional trends produced by wedging do occur, but they scarcely cry for recognition from every quarry and hillslope. The overwhelming majority of paleontological trends tell no obvious story of conquest in competition" (1993l, 304). Similarly, Steve wrote that we must "treasure our exceptions…But we must also be aware that single cases are fragile, and that sturdy facts are pervasive patterns in nature, not individual peculiarities. Most 'classic stories' in science are wrong" (1981u, 384). If we were to apply the same logic to species selection it would not come out looking good. My own view in the end is perhaps most similar to that of Flannery: "While I suspect that the concept of species selection is destined not to survive, at least in its present form, this is such a strongly contested field of biology that I would certainly not lay money—even at short odds—against Steve's eventual triumph" (2002, 53).

Stimulation

Even for many areas in which Steve's substantive conclusions have not stood up well in light of subsequent data or theory, many evolutionary biologists acknowledge that his ideas were enormously productive in stimulating research. Steve frequently argued that it was OK to be wrong for the right reason or right for the wrong reasons (e.g., 1996p; 1998x, 155; 1997m; 1998x, 323). He noted that errors could be useful "prods" to clarification and discovery, and quoted with approval the economist Vilfredo Pareto who said: "Give me a fruitful error any time, full of seeds, bursting with its own corrections. You can keep your sterile truth for yourself" (2002c, 614). (My own favorite version of the same view—which I once again learned as an undergraduate but came to appreciate only under Steve's influence—is from Mexican muralist Jose Clemente Orozco and is inscribed on the wall of Dartmouth's Baker Library: "Errors and exaggerations do not matter. What matters is boldness in thinking…in having the temerity to proclaim what one believes to be true without fear of the consequences. If one were to await the possession of the absolute truth, one must be either a fool or a mute.")

Other commentators have praised Steve for the fertility (if not the correctness) of his views:

> There's no question he's been one of the most influential and visible paleontologists, and indeed evolutionary biologists, in the last 50 years... Steve has provided an overarching vision and this astonishing ability to move among disciplines and integrate these ideas into producing a coherent picture. (David Jablonski, quoted in Monastersky 2002, A17)
>
> Most researchers... recognize that the concept [PE] has been invaluable in encouraging paleontologists to examine the fossil record with a rigor and attention to detail that previously was largely lacking. (Flannery 2002, 52)
>
> Key parts of punctuated equilibrium may be wrong, but paleontological data are, largely due to Steve, richer than ever. Species selection may not make sudden sense of the fossil record, but a reinvigorated paleontology sits at evolutionary biology's high table. (Orr 2002a, 138)
>
> I think the Modern Synthetic dogma is wrong. Steve did play some role in making us question the dogma. (H. Allen Orr, quoted in Monastersky 2002, A18)

VI. An End of a Beginning of an Appreciation and Farewell

In these days of seeking "balance" between "work and life" or "career and family," I just as often hear that many people willingly choose one over the other. Those who choose work sometimes say that it is what feeds them and makes them feel alive. Those who choose to devote more time to family sometimes say that their accomplishment and investment are in a secure and fulfilling marriage, and/or successful, healthy children. The academic equivalent of this balancing exercise is the struggle in which most faculty engage, to both advise students and pursue their own work. It was not until I had graduate students of my own that I came to appreciate the "great asymmetry" (cf. Gould 1998a) of this struggle: a student will (usually) have only one major doctoral advisor during their career, while one advisor will (usually) have many doctoral students during theirs. The significance and attention given by the advisor to the student is therefore almost always less than that given by the student to the advisor. Analogies between advisor-student and parent-child are, I realize, tenuous and probably dangerous as well. Yet I cannot help but reflect that, during a career that spanned less than forty years,

Steve Gould accomplished more scholarly productivity than most people could do in four lifetimes, and at the same time "raised" and sent out into the world at least thirty doctoral students, the majority of whom are still academically active and productive today. He did not choose; he found balance; he did it all.

In an interview not long before his death, Steve Gould was asked about his long-term wishes for *Structure* (2002c). He replied that "the biggest hope that any author would have if he put so much of a lifetime into something of this size is that it would be seen as a way station in the development of evolutionary theory that was useful and helped to focus things. Directed some energy. Got some things right, formulated something in a comprehensive and useful way" (Monastersky 2002, A17). Despite its flaws, *Structure* certainly does all of this. More important, Steve's career output does all this and much more. Thus, if we are to judge an academician's life's works as both the knowledge increased and the intellectual offspring produced, Steve Gould will share the legacy he predicted for one of his own heroes, Lavoisier: "His works, of course, will live—and he needs no more" (1998s; 2000k, 113).

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