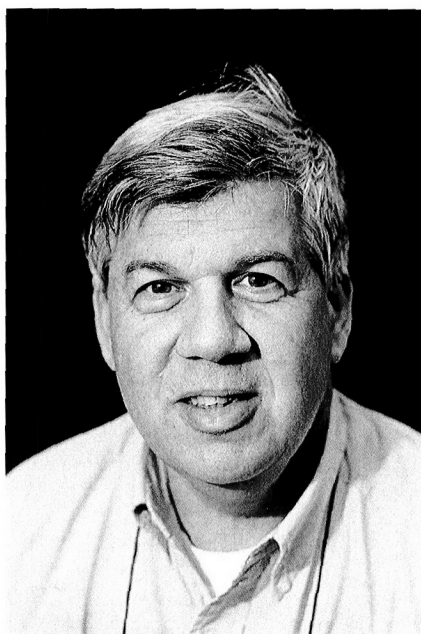


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PRESENTATION OF THE PALEONTOLOGICAL SOCIETY MEDAL TO STEPHEN JAY GOULD

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When Steve first asked me to deliver his citation on this occasion, we both thought we would be here together. That he is not makes this a truly sad occasion. But if Steve's passing robs us of the opportunity for simple out and out celebration, awarding the Paleontological Society's Medal posthumously to Steve today is instead an open invitation to us all to reflect on just how powerful and important a figure Steve was in the intellectual world of evolutionary biology, historical geology, history and philosophy of science—and especially for us today, paleontology. And in this light, we can recover the mood of celebration. For Steve never lost his identity as first and foremost a paleontologist—as a champion of the simple idea that the fossil record has much to tell us not just about what happened in life's long history up to this point—but *how* it happened as well. Steve was the prime mover and shaker in establishing without a shadow of doubt the relevance of paleontology to the modern scientific enterprise.

Steve, of course, has left behind a prodigious amount of pure scientific work—especially, but not exclusively, in the realm of theory. No one—not even Stephen Jay Gould—can look for the unanimous agreement of colleagues, nor on the lasting acceptance of each and every finding and conclusion as the collective research endeavor continues apace. But it must be said that, on this score—the degree of acceptance of data, analysis and pure ideas among one's colleagues—and, I think, also the probability of lasting impact, Steve did very well! Damn well, in fact!

We all know what the highlights are: A vision of contingency in evolution—most graphically encapsulated in mass extinctions, which run rough shod over the specific adaptations of species, to the point where entire clades are annihilated, and to the point

where evolution molds what's left over to produce the succeeding biota. What a healthy alternative to the ultradarwinian, hyper-deterministic adaptationist view of so much of the evolutionary biology that has held sway (as Steve himself might have said) throughout Steve's long, but too-short, career.

Then there's Steve's commitment to seeing the biological world as hierarchically structured, so that individuals, demes, species and perhaps even higher taxa can be seen to have their own particular internal processes, rules of organization, functions—and thus be, separately, targets of selection and perhaps other sorts of evolutionary processes. What a breath of fresh air, a vision that literally expands evolutionary theory away from the competing vision that collapses everything down to the selfish gene.

Then, too, Steve's unending commitment to wedding paleontology, comparative anatomy and developmental biology, using the early results of molecular biology, led him to speak about the possibility that relatively small changes in the genetic regulatory apparatus might cascade into relatively large-scale changes in adult morphology (a theme that took awhile to attract much interest, but one which lies close to the heart of modern evo-devo thinking)—and an area, not coincidentally, close to the hearts of many of today's younger paleontologists.

And there is, of course, good old punctuated equilibria, with its core blend of stasis and speciation theory, and so much more that has arisen as a fall-out from that initial statement. We wrote that paper in Tom Schopf's 1972 *Models in Paleobiology* book initially for the eyes of paleontologists only—and very much in the spirit that Tom (who, I have to say, absolutely hated the paper!) had in mind: of trying to get paleontologists to think in theoretical terms, to see the larger implications of their work, and

to try, as well, to communicate these thoughts and perspectives to a wider scientific circle.

But, much as it might be altogether fitting and proper to spend this entire citation oration on cataloguing Steve's more notable scientific achievements, in a sense I think that would be doing only partial justice to the man, what he stood for, and what he in fact achieved in his professional lifetime. Maybe it's because he had such a tremendous impact on me as a student two years behind him in the quest for a Ph.D in geology, I cannot help thinking that, however much Steve contributed to particular revolutions of scientific theory (and he was dedicated to that quest!), it was the *behavioral* revolution in fundamental approaches to the fossil record and its interpretation that may well prove to be his most far-reaching and lasting contribution. I am speaking here of the revolution that Steve fomented in the switch from paleontology's passive role as dutiful documenter of what-came-before-what in the history of life, to its much more exciting, active, even downright New York feisty, role in challenging the idea that all we need to know about evolution comes from *Drosophila* experiments and the mathematical formulations of population genetics.

This was a revolution started by George Gaylord Simpson in the forties, but pretty much aborted by the time the Darwinian centennial rolled around in 1959. In many ways, I think Steve's most lasting contribution will prove to be the resurrection, articulation and consolidation of that revolution: the establishment of paleontology as a legitimate, important player in the high stakes game played out at the High Table of evolutionary theory. Every one in this room has benefited from this revolution.

Let me give you a few signposts in the early history of Steve's development into a professional with the capacity not only to think, but to excite, to change not only how we think about evolution, but how we actually think of ourselves as paleontologists at the Millennium. Who knew, as Steve might himself have said, that this early trajectory would take him so far! (Indeed, just as an aside, I have to tell you, that when he handed me a copy of his just-published *The Structure of Evolutionary Theory*, in a motel in Washington last March, his room strewn with scans of his brain tumors, he had the presence of mind to write a note to me on the flyleaf that said, in part: "My God, what a ride it has been. . ."—Steve really loved that ride!!).

Steve showed up at Columbia as an entering graduate student in paleontology in the Department of Geology in the Fall of 1963. By sheer coincidence, there were at least a half dozen other new graduate students who came to study stratigraphy and both invertebrate and vertebrate paleontology—so there was a core group of eager young minds raring to go. And I have been forever grateful that they let me, a mere junior in college, tag along. Early on, I went with Steve, Bud Rollins, Bob Morris and the others on a memorable field trip to the Calvert Cliffs. Steve revealed the passion for field collecting that simply must be there if you are ever really going to be a paleontologist—and revealed as well an incredibly fine-tuned eye for the morphologically unusual and significant: many of you know that *Turritella plebeia* (if that's still its name!) is a very abundant high-spined gastropod in the St. Mary's and facies of the other formations in the Miocene of Maryland. Of the thousands of specimens we soon stopped picking up in our search for the rarer species, Steve found two specimens that were abnormally coiled—in fact, showed a rudimentary form of the uncoiling that is the hallmark of vermetid gastropods that Steve had already begun to study in some of his earliest research. This wasn't mere luck, of course—not by any means: it was the aware mind, thus the informed eye, that enabled Steve to spot these specimens—and a sure sign that Steve was always thinking, even while most of the rest of us were probably wondering about the next beer. It was a harbinger, as well, of

Steve's already developing interest in the relation between growth and form, about ontogeny and phylogeny.

Columbia's faculty back then was superb: Marshall Kay was still holding down the stratigraphy fort, while John Imbrie was our invertebrate paleontologist in residence; the American Museum crowd back then were also considered full-blown members of Columbia's Department—and that meant Norman Newell, Roger Batten, Ned Colbert, Bobb Schaeffer and Malcolm McKenna were also our teachers—quite a heady roster, all in all. All this—with some mighty tough courses and seminars—but still it didn't seem enough to this new bunch. Steve was the ringleader in organizing a seminar in macroevolution—held completely independently of the faculty (though I am sure they would have been tolerated, perhaps even welcomed, at these sessions). My notes from these sessions still make interesting reading.

Evolution, as far as paleontological research was concerned still by the mid-1960s, was pretty much in the hands of vertebrate paleontologists and the single-specimen-by-single-specimen comparative anatomical approach; vertebrate paleontologists tended to be in zoology, not geology, departments. What was it about the times that started to produce a new breed of evolutionarily minded invertebrate paleontologists—not just at Columbia, but also at Yale, Chicago and elsewhere? It's tough to say—but the advent of computing and multivariate analytic techniques, coupled with absorption of the importance of within-species variation, I think played a role in the awakening of the potential for evolutionary analysis proffered by the vast riches of a dense invertebrate fossil record. No problem getting sample sizes here!

Then, too, at Columbia, we had Tom Waller already hard at work on Tertiary pecten evolution—setting a fine example for all of us, Steve included. And we had Norman Newell. Norman, who had already written one of the very few twentieth-century evolutionary analyses of an evolving invertebrate lineage preserved in the fossil record. Norman, who already had set the standards of community ecology and paleoecology through his team's efforts in the West Texas Permian, and then recent environments and communities in the Bahamas. Norman, who was already talking about crises in the history of life—which Steve and the rest of us initially failed to see the importance of. We truly did not see how far ahead of his time Norman really was; we even muttered that perhaps Norman's take on evolution was not fully modern! (How wrong we were!)—but this was an aggressive learning style, and Steve and the rest of us were picking up much more from Norman than we realized at the time.

Steve jumped at the chance to write a paper for *Biological Reviews* on allometry. The invitation came from his earliest papers on the subject, written with his undergraduate mentor John White of Antioch College. This was the first of many such projects which Steve correctly saw as a chance to make a contribution by synthesizing and adding his own innovative take to a subject, rather than merely producing a term-paperish review limited to what others had already said. These sorts of projects honed Steve's interest in theory per se, and also brought out his fantastic blend of historical analysis melded with an acute grasp of the problem as it exists at the moment. It also got him lots of attention early—and of course he didn't mind that one bit.

I did ask him, though, how he could possibly feel comfortable in effect taking a year away from his thesis research to write his allometry review. That's when he said to me that the time to write theoretical work is now, while we are young. Put it off until you are sixty and your views won't be so fresh—and who knows, maybe you'll never get around to it at all. That had an enormous impact on me—and, at least by example, on all the rest of us.

But then came Bernie Kummel's call to Norman Newell—Bernie at Harvard, charged with replacing the soon-to-depart Harry Whittington; Bernie was one of Norman's earlier students (I think

Al Fischer was the first). Norman told Bernie that he had just the guy for him—and the die was cast. Steve rushed back to his thesis work, polished it off quick, and shot up to Harvard in the Fall of 1967. I should also briefly mention that Steve's thesis was a brilliant study of microevolution in fossil and recent *Poecilozonites* land snails—restricted as they were to a narrow little tract of Bermudan real estate, Steve saw that he had a grasp of the entire spatiotemporal history of this endemic clade—a powerful realization that, in a characteristically apt trope, Steve called an “evolutionary microcosm.”

What better place for an ambitious young paleontologist to get his first job than at Harvard? (Those were the days, after all, before Harvard and other institutions got their reputations as places that routinely refused to grant tenure to callow assistant professors!). At some point early on in Steve's Harvard career, I went up to take a look at—and pillage—the phacopid trilobites that were part of the prodigious collections Harry had had to leave behind when he vacated the niche that Steve filled. My wife and I were staying with Steve and Debbie—and after a good dinner and conversation that brought back the already good old days of graduate student life, in due course we toddled off to bed. Around 11:00 p.m., though, the hall light went back on, and the clickety-clack of Steve's typewriter broke our nascent slumber. I think he was writing a review of a just-published tome of Lyell's correspondence.

So he was a workaholic. In fact, of all the smart people I've known, I've never met anyone who was as smart and who worked so hard, as Stephen Jay Gould. He was that way throughout his life. He told me—and I believe him—that he wrote primarily for his own satisfaction. I think he felt, as I and many others do, that you cannot really understand a problem until you are able to articulate it: talk about it, and still even more, write about it. In fact, in writing, you find out right away what you do not understand—so you are forced to complete the job on the spot.

But that wasn't the whole story by any means with Steve and his writing. Steve knew that he was beginning to have a wide, as well as deep, impact—first in paleontology and evolutionary biology, but soon thereafter in the history of science—and in issues of broader, more general concern within

science, and ultimately within society. I think his *Natural History* columns (and I am proud to say I got him that gig) are not as fully appreciated for what they were as they should be. We all in this room tend to think in fairly rigid terms about what constitutes a truly “scientific” paper—as opposed to a column that can be read by a greater spectrum of sentient human beings. To think that Steve's columns were just ways for him to write for the general public is to miss the true worth that nearly everyone of them held: Steve was showing us every month that smart people can understand even the most arcane and intricate details of paleontology, genetics and development—not to mention the history of science. But he was doing far more even than that: for Steve was doing real, usually rather tough, intellectual work—including real scientific work—each time he wrote a column. You can just feel him grappling with issues—a difficult, messy and often frustrating pursuit, month after month, hidden from view only by the relaxed, confident prose style that he insisted on using, in more or less identical form, from everything from his columns on the one hand, to papers published in the *Journal of Paleontology* on the other.

More than anyone else in my lifetime as a paleontologist, Steve taught us to take ourselves and our profession seriously. And he got in everyone's face outside the profession who continued to insist that paleontologists should shut up and just get on with the business of describing fossils. Dinosaurs and hominid fossils are important icons, and Steve used them—especially *Tyrannosaurus*—very effectively. And yes, certainly, Steve left behind a prodigious legacy of intellectual accomplishment and scientific work. He helped transform the very way the entire profession of paleontology does its work.

But in the end it was his fearless, persistent and brilliant exposition of the sheer relevance of paleontology to grappling with the deepest scientific issues for which we should be eternally grateful to Steve.

Madame President, I am both sad and extremely proud to present the life and work of Stephen Jay Gould to you for presentation of the 2002 Paleontological Society Medal.