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The life and work of T. J. M. Schopf (1939–1984)

Stephen Jay Gould

Museum of Comparative Zoology, Harvard University, Cambridge, Massachusetts 02138

No words of acceptance or justice exist to tell the tale. Although we paleontologists, perhaps better than all others, know how capricious, how disordered, how random our world can be, still we must rage inside when a good man dies so far before his time. In March 1984, in the line of duty (while on a field trip with students), Tom Schopf, the man who, by effort so far beyond human expectation, converted this journal from improbable dream to resounding success, died at the age of 44.

Tom Schopf was a complex, difficult, exemplary man. He personified two great virtues: dedication and vision. His commitment to science was total, almost maniacal (in the admirable sense). He could not stop thinking and doing, not for a moment. His work displayed great diversity: he developed empirical expertise with conodonts and bryozoans; he published a book on paleoceanography; he studied the potential for fossilization of modern invertebrates; he wrote widely in the history of science; above all, he tried to bring the insights of modern biology into our field, not by commenting from the ease of a library desk, but by spending required years in the arduous work of bench-top molecular biology. Tom never stopped, or when he stopped, he took his pen and wrote—long letters by hand, to me and a few other colleagues, full of heady thoughts, hopes and visions for new directions in studying life's history, and full of his love for particulars. When Tom visited a city for the first time, he headed straight for the zoo, to revel once again in nature's small pieces and to think about connections. He wrote me a postcard once from the Paris zoo: "Have now seen the Pandas at Washington, D.C., London, Madrid, and Paris. Wish they were all together to see them interact. Nice hobby—zoos."

Tom pursued his science with that rare (and admittedly ambiguous) quality that can only be called "vision." He had an overarching concept of the world's order, and he related every iota of his scientific work to furthering this view of life. While most of us are buffeted about by interest and opportunity, Tom consciously directed all his energy to developing and promulgating a radical view of life's order. We need not agree with its content (indeed, I don't), but we can only respect such honest passion for its attempt at synthesis and for its audacity; Tom Schopf was a brave man.

Tom had a sublime vision of regularity. He yearned to convert an empirical field, manifestly short of ideas to unite its fascinating particulars, into a science based on experiment, construction of null hypotheses, rigorous test, and the possibility of rejection to move forward toward an agreement that all could share. He wanted to rescue paleontology, convert it to an exciting "chancy young man's game" (a phrase from E. O. Wilson that he particularly liked), from that peculiar lethargy imposed by overly cautious empiricists who held that the record's imperfection practically debarred any generality and relegated professionals to specialization on a time, place, and taxon.

To accomplish this transformation of both thought and practice, Tom developed a vision of "species as particles" (his phrase). One could, he fervently hoped, subtract the empirical particulars that distinguish taxa and times and find an underlying statistical distribution shared by all species, whatever their manifest externalities. He hoped, in short, that paleontology could construct a set of "gas laws" that ignored the motion of individual molecules (species) for the predictable, timeless regularities of the whole. He once applied, with his characteristic mixture of bravery and almost foolish presumption, to write an autobiography in the series sponsored by the Sloan Foundation and restricted to such senior eminences as Freeman Dyson, Peter Medawar, and Lewis Thomas. He stated in his prospectus:

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"Perhaps the work I am best known for is . . . on 'particle paleontology'—the notion that, in many important ways, each species is equivalent to every other species, and that for the millions upon millions of species which have inhabited the earth, there are 'gas laws of paleontology' akin to the gas laws of chemistry. This work makes use of stochastic models in which deterministic explanations for the rise and fall of various taxa are largely discounted." As to the effect of this vision on paleontological practice, Tom continued: "Throughout all of what I have done, the main motivation has been to bring the approaches and information of biology to bear upon problems in paleontology. I have especially been concerned with bringing theory into paleontology, and in so doing, to make that field less anecdotal, more rigorous and less tied to descriptive work."

Speaking more personally and passionately to me, he wrote in December 1981: "It is now completely unacceptable to me that any species is really different from any other species. They are all out there trying like hell to do well. Sure, God helps those that help themselves. But what does God do when all species are helping themselves? The traditional view is that, under those circumstances, God is picking and choosing? But that surely is nonsense." And a year later, virtually begging me to abandon punctuated equilibrium as a throwback to an older, unacceptable determinism based on differences, and to embrace his rarefied view: "you would find in it a yet grander vision of nature." I think that we can understand from these private jottings why Tom, with his almost zealous fervor, alienated so many colleagues. But I trust that we can all also grasp the higher truth—that he did so in the service of an honest, deeply felt vision, from a passion to understand life and its history. What can be ultimately more admirable than such a deep commitment to know and to search?

The dilemma, of course, with Tom's vision of "particle paleontology" is that nature, read literally, says no and grants important individuality to times and taxa. Tom understood this perfectly well and therefore devoted much of his career to arguments, often more than a bit forced in my opinion, that apparent differences reflected biases of the record or superficial levels of perception, and that a deep, underlying regularity ticked away in the same manner for each species. Thus, in his own distinctive contribution to our joint work on random models (no. 41 in the Bibliography that follows), he argued that apparent differences in morphological rates only reflect the bias of complexity, with fast rates found in creatures with more parts to change—and that rates normalized for complexity might not vary from group to group. (He was bitterly disappointed that this argument, which he developed and which meant so much to him, was rejected by most colleagues.) And his (in my view) overly passionate campaign against punctuated equilibria (nos. 71, 73, 79, 80) rested on a conviction that morphological stasis was superficial appearance only and that, at the "real" molecular level of change, genomes churned at an extremely rapid and stochastically constant rate.

Tom's vision of underlying generality in objects unites and makes sense of all his major work. He made, in his own view, two significant contributions. He knew more practical biology than any other "paleobiologist" of our young Turk generation (now, inevitably, aging). He mastered the methodology of gel electrophoresis when it was still the forefront of evolutionary genetics and not just a routine technique. He knew how to apply it (as no one else did) to major problems of paleontological importance. In a series of seminal papers with Jim Gooch (nos. 19, 21, 22, and, esp., 27 and 34), he effectively disproved the Bretsky-Lorenz hypothesis that selectivity in mass extinctions reflected the depauperate genetic variability of creatures living in uniform environments. His usual vision of generality motivated this work as well—for he showed that species of different phyla in fundamentally different environments have adequate (and about the same) levels of genetic variability.

Tom did not live to do his major work in molecular evolution—work that required, at an age when few of us would willingly revert to the apprenticeship and absurd hours of graduate student life, a total retooling and starting from scratch. In 1981–82, he went to CalTech, apprenticed himself in a modern laboratory of molecular biology, and struggled to master the techniques of DNA sequencing. He hoped to solve that greatest of genealogical dilemmas: How are

the invertebrate phyla related cladistically, particularly those groups (like his beloved ectoprocts) that show both protostomous and deuterostomous characters? He might well have succeeded. He reasoned—I think he was right—that if classical morphology and embryology had not solved this problem in several hundred years of trying, they were not likely to succeed at all, and that the massive sample size of items in a DNA program would surely overcome convergence and reveal homology. Again his vision of generality drove him on—the genome ticks similarly for all creatures and reveals cladistic relationships thereby.

He viewed the joint work on random models of phylogeny that he instigated with Raup, Simberloff, Sepkoski, and me as his second major contribution to science (nos. 35, 41, 44, 51, 58). Tom lacked the mathematical skills that allowed the others (and, to a much lesser extent, me) to do the detailed work, but he was the source and soul of our efforts. He suggested the task, selected the participants, brought us together at Woods Hole, proselytized us to accept his controlling vision. He was frustrated that we did not embrace it wholeheartedly. (I, for one, was always more interested in using the stochastic models as null hypotheses for finding out what about the fossil record really did require the individuality of times and taxa.) He was therefore even more frustrated that he needed our skills but could not win our total allegiance. I believe that his finest and most enduring contribution (no. 37) to this work lay in his recognition that he could use another of those timeless, taxonless generalities of equilibrium ecology—the species-area curve—to calibrate the effects of coalescing all continents into Pangaea as a basic cause of the great Permian extinction. I now suspect that he claimed too much, but I think he was right in identifying the stage (theatrical, not geological) and in showing us a way to quantify its effects—a major advance on the classic, intractable problem of paleontology.

Tom was a prickly, often difficult colleague, so driven by his unconventional vision, so committed to its fundamental truth, so brave (or foolhardy) that he would sacrifice friendship and human relations to its zealous advance. I cannot psychoanalyze him; it would foolishly denigrate his genuine intellectual commitment even to try. But I do know one thing: I know that it led him to much personal misery. I often cried inside that he could not break out of it and spare himself the pain, but I and his other friends could do nothing. Tom was so committed to a unity of vision that he hopelessly conflated his sense of the factual with his belief about the ethical. He saw what he deeply believed as not only true but just, right, and moral. He could (and did) infuriate colleagues by constantly accusing them of evil as well as error. Thus, when Dave Raup suggested that bad genes as well as bad luck might have done the trilobites in, Tom felt that he had revived not only racial senescence but racism as well (in claiming that some groups aren't as good as others). And when I persisted in my (to him) curious commitment to punctuated equilibria, he accused me of the very biological determinism (in arguing for truly different rates and patterns in various taxa) that I had combatted in *The Mismeasure of Man*. I could not fight these interpretations; I could only be sad for Tom. But my love and admiration for him were never compromised (though I was, of course, often annoyed), for I hope we can all understand that a pure and burning commitment to knowledge and understanding drove him, often (sadly) in destructive ways.

This commitment and the boundless energy he could muster in its support also underlay Tom's other great professional achievement, perhaps ultimately his finest legacy. Tom made this journal of ours, almost singlehandedly, and by dint of arduous effort so far above any call of duty that it beggars description. Whoever heard of a journal that became profitable in its initial year, that immediately became the exciting centerpiece of an entire profession, that commanded the attention and universal respect not only of its immediate paleontological supporters, but of the large (and previously quite disinterested) community of evolutionary biologists? Tom did it, with a little help from his friends to be sure, but mostly by that unbeatable combination of blood, sweat, toil, and tears.

He knew that his own personality could not assure smooth sailing, so he wisely chose Ralph Johnson, one of the great gentlemen and diplomats of our profession, for the overt activity, while he did the spadework and attended both to the endless details and to forging a clear intellectual basis for a journal representing an exciting, young, true science of paleobiology.

I walked into his office one night in Woods Hole, and Tom was in the midst of writing 250 letters *by hand* to libraries that had not subscribed (he knew how much *Paleobiology* needed an institutional basis of support). When I asked why he simply didn't xerox a single letter, and save himself countless hours of backbreaking, boring, hand-cramping work, he replied that a personal note might get more attention from harried librarians. I do not know whether his ploy worked, but need I say more to illustrate his dedication? Tom and Ralph were an unbeatable combination. They triumphed, and we are here and healthy today as a result of their effort and vision.

We all judge our work, not by its reception among the multitudes, but by its impact on the very few colleagues so attuned to us and so highly respected by us that we weigh their opinions in the ultimate balance. Never before have I lost one of these precious people. I expected that Tom would be around all my life. I counted on his inspiration and his damnable vision. I was also privileged to know the deep humanity that he often hid. Now he is gone, and I feel empty and angry.

Speaking of anger in the face of death, Dylan Thomas urged us to "rage against the dying of the light." Although I grieve for Tom and will miss him deeply all the rest of my life, I do not fear for the light he kindled and nurtured for our science of life's history. It will burn brightly, at least until Nemesis returns.

Tom once sent me a postcard from Rome after visiting the Sistine Chapel: "I wish a paleontologist could equal, in 10 chapters of a book, those nine frescoes that Michelangelo used to tell the origin of life, and the one that told its demise. Maybe one should try." Tom, you didn't live to finish the book, but you wrote a few of the chapters and you built the publishing firm. We are all richer for your vision and dedication, and we thank you for sharing with us a life too brief, but so well lived.

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