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The THIRD CULTURE

by John Brockman

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STEPHEN JAY GOULD: There is no progress in evolution. The fact of evolutionary change through time doesn't represent progress as we know it. Progress is not inevitable. Much of evolution is downward in terms of morphological complexity, rather than upward. We're not marching toward some greater thing.

The actual history of life is awfully damn curious in the light of our usual expectation that there's some predictable drive toward a generally increasing complexity in time. If that's so, life certainly took its time about it: five-sixths of the history of life is the story of single-celled creatures only.

I would like to propose that the modal complexity of life has never changed and it never will, that right from the beginning of life's history it has been what it is; and that our view of complexity is shaped by our warped decision to focus on only one small aspect of life's history; and that the small bit of the history of life that we can legitimately see as involved in progress arises for an odd structural reason and has nothing to do with any predictable drive toward it.

I'm working on an incubus of a project on the structure of evolutionary theory, an attempt to show what has to be altered and expanded from the strict Darwinian model to make a more adequate evolutionary theory.

Basically, there are three themes. The first is the hierarchical theory of natural selection—selection operating on so many levels, both above and below. Richard Dawkins, who still wishes to explain virtually everything at the level of genic selection, is right about one thing; gene selection does operate. He's wrong in saying that it's *the* source of evolution; it's *a* source. I don't know what the relative strength and

power of the levels are—it depends on the particular problem—but gene selection is not the dominant one, by any means. It certainly happens; it may be responsible for the increase in the number of multiple copies of some kinds of DNA within evolutionary lineages, for example; it's responsible for some things.

The second theme is the extent to which strict adaptationism has to be compromised by considering the developmental and genetic restraints at work upon organisms, when you start considering the organism as a figure that pushes back against the force of natural selection. The best way to explain it is metaphorically. Under really strict Darwinism (Darwin is not a strict Darwinian), a population is like a billiard ball: you get a lot of variability, but the variability is random, in all directions. Natural selection is like a pool cue. Natural selection hits the ball, and the ball goes wherever selection pushes it. It's an externalist, functionalist, adaptationist theory. In the nineteenth century, Francis Galton, Darwin's cousin, developed an interesting metaphor: he said an organism is a polyhedron; it rests on one of the facets, one of the surfaces of a polyhedron. You may still need the pool cue of natural selection to hit it—it doesn't move unless there is a pushing force—but it's a polyhedron, meaning that an internal constitution shapes its form and the pathways of change are limited. There are certain pathways that are more probable, and there are certain ones that aren't accessible, even though they might be adaptively advantageous. It really behooves us to study the influence of these structural constraints upon Darwinian and functional adaptation; these are very different views.

The third theme is the extent to which a crucial argument in Darwinism—namely, that you can look at what's happening to pigeons on a generational scale and extrapolate that into the immensity of geological time—really doesn't work, that when you enter geological time there are a whole set of other processes and principles, like what happens in mass extinctions, that make the extrapolationist model not universal.

I'm attempting to marry those three themes—hierarchical selection, internal constraint, and the immensity of geological time—into a more adequate general view of evolutionary theory.

I should say that geological time is in there because it's so essential to strict Darwinian theory that you be able to use the strategy of bio-uniformitarian extrapolation; in other words, that you be able to

see what happens in local populations, and then render the much larger-scale events that occur through millions of years to much larger effect by accumulation of these small changes through time. If, in the introduction of the perspective of millions of years, new causes enter that couldn't ever be understood by studying what happens to pigeons and populations for the moment, then you couldn't use the Darwinian research strategy. That's why Darwin himself was so afraid of mass extinction and tried to deny the phenomenon. The geological stage is really a critique of the uniformitarian, or extrapolationist, aspect of Darwinian thinking.

Richard Lewontin is my population-genetics colleague at Harvard, probably the most brilliant man I've ever had the pleasure of working with. We teach a "Basics of Evolution" course together. In 1978, there was a symposium on adaptation held by the Royal Society of London. It was a very pro-adaptationist symposium; that's the British hang-up, after all. I think John Maynard Smith was one of the organizers. Dick was invited to present a contrary view, because—particularly after the publication in 1975 of E.O. Wilson's *Sociobiology*, which is so strongly adaptationist—Dick had been quite vocal in his doubts about the adaptational parts.

Clearly, there's a lot of adaptation in nature. Nobody denies that the hand works really well, and the foot works well, and I don't know any way to build well-adapted structures except by natural selection. I don't have any quarrel with that, and I don't think any serious biologists do. But adaptationism is the hard-line view—which has been so characteristic of English natural history since Darwin—that effectively every structure in nature (there are exceptions of course) needs to be explained as the result of the operation of natural selection; that if we're not absolutely optimal bodies—because clearly we're not—we're at least maximized by natural selection.

Darwinian biologists will use it as the strategy of first choice. If you see a structure in a flower or in a mole, and you don't know what it's for, the first thing you assume is that it was built by natural selection for something, and your job is to figure out why it's there—the "why" being "What is it good for?"—because once you know what it's good for, then you know why natural selection made it. Although this is a technique that often works, it's inadequate in so many cases that it just doesn't suffice as a general strategy, the main problem being

that many structures are built for other reasons that have nothing to do with natural selection. For example, they can arise as side consequences of other features that might have adaptive benefit. Having been built for other purposes, they may then prove useful; they can be coopted secondarily for utility. The bird's wing did not evolve for flight. If you want to know why it's there, seeing a bird fly isn't going to help you, because 5 percent of a wing doesn't fly. It must have been built originally for some other function.

Take the human brain. Most of what the human brain does is useful in a sense—that is, we make do with it—but the brain is also an enormously complex computer, and most of its modes of working don't have to be direct results of natural selection for its specific attainments. Natural selection didn't build our brains to write or to read, that's for sure, because we didn't do those things for so long.

Anyway, the Royal Society asked Dick to write a piece for the 1978 symposium. I had developed my own doubts about adaptationism, for a host of reasons. Part of it came from working on random models of phylogeny with Dave Raup and Tom Schopf and Dan Simberloff in the early seventies and coming to realize how much of an apparent pattern could be produced within random systems. Part of it came from writing my first book, *Ontogeny and Phylogeny*, in 1977, and coming in contact with the great German and French continental literature on structural, or nonadaptational, biology. That's the continental tradition as much as adaptationism is the English tradition. I also had been unhappy with the overuse of adaptation in sociobiological literature, so I had a whole variety of reasons to agree with Dick on those subjects.

Dick was going to be the one nonadaptationist speaker at the symposium. In fairness, he was going to give the last speech, and it was certainly given prominent coverage; the English are nothing if not fair.

Dick doesn't like to fly, and he had no particular desire to go there, and since we had pretty consonant views and I wanted to go to England anyway, we decided to write a joint paper. In fact I wrote virtually all of it. He was very busy, and I would be giving the paper anyway. The paper is a general critique of full-scale adaptationism, or panadaptationism. It's not an attempt to trash Darwinian natural selection, which obviously happens; it's an attempt to argue that

adaptationism, or the notion that Darwinian selection is effectively responsible for everything in the form of organisms, just will not work.

One of the main reasons I'm proud of that paper is that I do believe in interdisciplinary perspectives and—as an essayist, particularly—the use of examples from other fields. The paper succeeded because I used a fairly arresting strategy of argument, by beginning with an architectural example.

The paper is called "The Spandrels of San Marco and the Panglossian Paradigm; A Critique of the Adaptationist Program." I began by talking about the spandrels under the domes of the cathedral of San Marco. I had been in Venice a few months before, and I had stood under the dome in San Marco, and I had worked out this argument for myself, and it was very enlightening to me. It helped me to see what's wrong with the adaptationist paradigm.

Here's the situation: You decide to build a church by mounting a circular dome on four rounded arches that meet at right angles. I'll accept that as an analog of adaptation; that's an engineering design that works. But once you do that, you have four tapering triangular spaces where any two arches meet at right angles. The spaces are called spandrels—or pendentives, but the more general architectural term is spandrels. They're spaces left over.

No one can claim that the spandrels under the dome are adaptations for anything. I suppose it's a good idea to put some plaster there—otherwise the rainwater is going to come in—but the fact that they're tapering triangular spaces is a side consequence of the adaptive decision to mount the dome on four arches. It's space left over. It's a side consequence; it isn't an adaptation in itself.

When I looked at these spandrels, I realized that every set of spandrels—there are six in San Marco—had a very sensible iconography linked with the dome. Under the main dome, for example, there are four evangelists in the spandrels. Four spandrels, four evangelists. Under each of the four evangelists is one of the four biblical rivers—the Tigris, the Euphrates, the Nile, and the Indus—and they are personified as a man, and the man holds an amphora, a water jar, and he pours water onto a single flower in the tapering triangular space below. It's a beautiful design. But no one would argue that the spandrels exist to house the evangelists. The spandrels are nonadaptive, side

consequences. Since they are there anyway, you might as well fill them with useful and sensible structures.

Many biologists would say, "Well, of course, that's right. We know there are spandrels, or bits and pieces, left over, but they're just nooks and crannies, funny little corners; they don't have any importance." But that's not true; the fact that something is secondary in its origin doesn't mean it's unimportant in its consequences. Those are entirely separate subjects.

Spandrels often turn out to be more important, in terms of the consequences in history of a structure, than the actual immediate reasons for their having been there in the first place. For example, the dome of San Marco is radially symmetrical; there is no reason to ornament the dome in four-part symmetry for structural reasons, yet every dome but one in San Marco is ornamentally structured in four-part symmetry, in harmony with the spandrels below. The spandrels are not just nooks and crannies; they actually determine the iconographic program of the dome itself. Just as with the human brain: most of what the brain does are probably spandrels—that is, the brain got big by natural selection for a small set of reasons having to do with what is good about brains on the African savannas. But by virtue of that computational power, the brain can do thousands of things that have nothing to do with why natural selection made it big in the first place, and those are its spandrels.

Because I began this paper with an architectural example, no one would confute it, because it wasn't a threat to their conventional thinking. If I'd started with an organic example, it would have raised the hackles of all the people trying to be strict Darwinians.

Arthur Cain was the summer-up of the whole session. There's a line in Durrell's *Alexandria Quartet* where the narrator, Pursewarden, says that he's a Protestant in the only meaningful sense of the term—that he likes to protest. Well, moderators are supposed to be moderate, I suppose. Arthur Cain was not. He devoted his entire summary of the conference to a vitriolic attack on this paper, essentially saying that Dick and I knew that adaptation was true because we had to, because it obviously is true. Arthur said that we had attacked it because, although we knew it to be true, we so disliked the political implications of sociobiology, which is based on it, that we abrogated our credentials as scientists.

That was so off the wall that it was just amazing. When I got up to give my re-reply, the second coordinator of the conference was standing in front of the podium—which had the motto of the Royal Society, "Nullius in verba," on it—and I asked him to step aside. He was annoyed: why was I asking him to move, that was not fair. But he later realized why I had. Now, I'm stupid about certain things that scientists are supposed to be good at. I'm not particularly quantitative; I'm numerate but not innovative. I'm not a great experimentalist. But I pride myself on having immersed myself in Western culture and having learned some languages, and knowing certain aspects of humanism that many scientists don't take up.

I asked him to step aside, and I said that I thought Arthur had been entirely wrong, that he'd completely misunderstood the motives of my talk, and that I was doing nothing but trying to uphold the motto of the Royal Society, which had sponsored this meeting. The reason that was an effective strategy was that I knew that most people, most members, didn't know what the motto "Nullius in verba" meant. It looks like it means "Words do not matter" or "Do not pay any attention to words," since *nullius* means "nothing" and *verba* is "word." So most people think it means that words mean nothing and you have to do the experiment.

But *nullius* is genitive singular; it can't mean that. It means "of nothing" or "of no one." I knew what the motto meant. I knew that it was a fragment of a statement from Horace—a famous quotation from a poem, in which he says, "I am not bound to swear allegiance to the dogmas of any master." *Nullius addictus jurare in verba magister.* It's "Nullius in verba," or "In the words of no (master)." It's just a fragment from a larger line.

"That's all I'm doing," I said. "I'm saying that we are not bound to swear allegiance to the dogmas of any master; I'm here to present an alternative viewpoint that's consistent with your own society. How can you castigate me?"

The paper gets a lot of citations, but I don't know how many of its citations mean that it was actually used. In the game of citation analysis, you know that there are a certain number of citations that are, in a sense, honorary; that is, people will write a paper in which they want to support an adaptationist's perspective, and they feel that in fairness they have to cite at least one thing to show they know there's an opposing literature. The spandrels paper is the classic one,

so they cite it. Whether or not they actually take it seriously I don't know. But it's become the standard source of a broader view of the causes of evolutionary form.

The paper provides a context for my current views on constraint—the importance of geometric and historical constraint, as opposed to a strictly adaptationist view of the world. The "exaptation" argument arises very much out of the spandrels principle, and I wish I'd developed the word when I wrote the paper. There's a problem—most Darwinians don't acknowledge it, since it doesn't work out as a problem for them—because "adaptation," as the word is used, has two distinctly different meanings. It's the process whereby a structure is designed by natural selection for a use, but often the word is also used for the structure itself. I have my foot here. It works well. Is it an adaptation, simply because it works well? Strict Darwinians don't have a problem using the same word both for the structure that works well and for the process that gets you there, because they think that the process is the only way you can get the working structure.

Under the spandrel principle, you can have a structure that is fit, that works well, that is apt, but was not built by natural selection for its current utility. It may not have been built by natural selection at all. The spandrels are architectural by-products. They were not built by natural selection, but they are used in a wonderful way—to house the evangelists. But you can't say they were adapted to house evangelists; they weren't. That's why Elisabeth Vrba and I developed the term "exaptation." Elisabeth is a paleontologist at Yale University, who has collaborated with both Niles Eldredge and me, and who did the most interesting work on punctuated equilibrium.

Exaptations are useful structures by virtue of having been coopted—that's the "ex-apt"—they're apt because of what they are for other reasons. They were not built by natural selection for their current role. Strict Darwinians cannot deny the principle. Their usual response is to say that it's minor, just a gloss, exaptations are rare, they're just nooks and crannies, they're not important. But in the spandrels argument it's essential that they are important. Just because something arises as a side consequence doesn't condemn it to secondary status.

Arthur Cain brought up the subject of political implications. In a sense, I brought it on myself, but I'll defend how it happened. Niles

Eldredge and I wrote the first punctuated-equilibrium paper in 1972. I wrote a follow-up in 1977, in which I tried to analyze some of the theory's social and psychological sources, because they're in every theory of gradualism, and I had tried to argue that gradualism is a quintessential notion of Victorian liberalism. I thought it would be so ridiculous and—to use a biblical term—vainglorious to claim that gradualism, at least in part, was not a truth of nature but recorded a social context, and then to argue that "punctuated equilibrium is true; it's just a fact of nature." There obviously had to be a social context for punctuated equilibrium, too. I thought it only fair to write about what might have been some of the sources of punctuated equilibrium, and since there's a long tradition in Hegelian and Marxist thought for punctuational theories of change, it was clearly not irrelevant that I had been brought up by a Marxist father. I'd learned about these things.

That's not the reason the punctuated-equilibrium theory exists—if only because Niles developed most of the ideas, and he didn't have any such background. But it is relevant that I, rather than someone else, thought of it, in that my own background is probably a relevant fact. It was necessary for me to say that; it would have been absurd to claim that gradualism is politically influenced but punctuated equilibrium is a fact of nature. People seize upon that one statement.

Historians of science make a distinction between what they call context of justification and context of discovery, and it's fair enough. There's a logic of justification, which is independent of the political and social views of the people who develop the ideas. But if you want to ask why certain people develop ideas rather than other people, and why they develop them in this decade rather than that decade, then for those questions, which are about context of discovery rather than context of justification, surely the personal side is very relevant; it has to be explored and understood. But it has very little bearing on whether the idea is right or not. The fact that I learned Marxism from my father may have predisposed me toward being friendly to the kind of ideas that culminated in punctuated equilibrium; it has absolutely nothing to do with whether punctuated equilibrium is true or not, which is an independent question that has to be validated in nature.

Within a profession, certain issues can become very big which, if seen from the outside, might not seem so. For instance, in evolutionary theory, on the outside the only issue might be whether evolution is true or not. That's the big one! On the inside, of course, everyone knows that evolution is true; the issue is how it occurs. The main difference between Richard Dawkins and myself has to do with the agency of natural selection, and its power, and the degrees of adaptation that it produces. Within the field, these questions define the essence of Darwinism; outside the field, they might seem smallish. It is just a question of perception.

Richard wants natural selection to be effectively all-powerful, at least when you are dealing with the phenotypes—the forms of organisms. He wants the locus of that selection to be genes. I maintain that natural selection works on a hierarchy of levels simultaneously, of which genes are one and organisms are another, and that you also have higher units, such as populations and species, at which selection is very effective, and the end result is not always, by any means, adaptation—particularly when you see the process unfolding in millions of years of geological time.

No matter how effective adaptive change might be in the moment, when you start translating that and any other process into millions of years, it doesn't work out that the history of life is under adaptative control, because you have to get through these largely random and highly contingent mass-extinction events, as well as new species arising by punctuated equilibrium. Long-term success in clades is the function of speciation rate, which has very little to do with the morphologies that are built by natural selection. So Richard's and my whole views of evolutionary mechanics are very different, but to the outsider, who may only be concerned with whether evolution happens or not, we probably seem to be pretty similar, because we are both evolutionists.

I would call Richard's approach hyper-Darwinism. The brilliance of Darwin's argument, and the radical nature of it, lies in changing the focus of explanation. Before Darwin, people thought that organisms were well-designed because the highest-order force was doing it directly. There was a benevolent, creative God who made it that way. The brilliance of Darwin is that he beat the level of explanation down to organisms, saying that organisms are well-designed as a side consequence of their struggle for individual reproductive success. It is a deliciously radical argument. Instead of an all-wise, benevolent, purposeful God, what you have are organisms struggling for personal ad-

vantage—which seems to be the moral opposite, except that there is no morality in nature—and as a side consequence you get good design of organisms.

Richard has taken that posture of trying to beat the level of explanation down, and has carried it to its ultimate extreme: it's not even the organisms that are struggling, it's only the genes. The organisms are "vehicles." That's his pejorative word; most of the profession calls them "interactors," which is less pejorative. The only active agents in Richard's worldview are genes. He's wrong. If you read the British philosopher Helena Cronin's book *The Ant and the Peacock*, she argues that the whole profession has been transformed by this idea. Whatever my personal point of view might be, her claim is sociologically wrong in a purely factual or Gallup Poll sense. Not many people take this view seriously. A lot of people like it as a metaphor for explanation. But I think that very few people in the profession take it seriously, because it's logically and empirically wrong, as many people, both philosophers and biologists have shown—from Elliott Sober to Richard Lewontin to Peter Godfrey Smith.

Richard is basically wrong, because organisms are doing the struggling out there. If organisms could be described as the additive accumulation of what their genes do, then you could say that organisms are representing the genes, but they're not. Organisms have hosts of emergent characteristics. In other words, genes interact in a nonlinear way. It is the interaction that defines the organism, and if those interactions, in a technical sense, are nonadditive—that is, if you can't just say that it's this percent of this gene plus that percent of that gene—then you cannot reduce the interaction to the gene. This is a technical philosophical point. As soon as you have emergent characteristics due to nonadditive interaction among lower-level entities, then you can't reduce to the lower-level entities, because the nonadditive features have emerged. These features don't exist until you get into the higher level. His argument is wrong. It's not just a question of being inadequate. It's wrong.

Admittedly—again, in a sociological sense—it's enormously appealing. When you realize what Darwin did, which was to break down the explanation from the benevolent God to the struggling organism, the notion that you might break the explanation down further, to the struggling gene, has a certain reductionist appeal. But if you surveyed the profession, although not all of them would necessarily agree with

me about hierarchical selection, most would say that Darwin was right and selection is primarily on organisms, which has always been the traditional view.

Gene selectionism was never a paradigm that attracted large numbers. What did happen was that the generation before Dawkins, culminating in 1959, had a form of very strict Darwinian adaptationism, a more classic, organism-centered Darwinian approach that wasn't by any means totally wrong but was much too restrictive. It did become a ruling view within evolutionary theory, and to some extent we're still fighting it, in talking about large-scale, macroevolutionary changes as not being fully extrapolatable out of the adaptive struggles of organisms and populations.

I might be on the periphery of orthodoxy, but I certainly think natural selection is an enormously powerful force. Darwin's canonical form of it—that is, selection operating on individual bodies via the struggle for reproductive success—just isn't capable, by extrapolation, of explaining all major patterning forces in the history of life. Whereas it's vital for strict Darwinism that you do accept such a view. You'll always have a little bit here and there for other things, to be sure, but unless you can argue that Darwinian selection on bodies is, by extrapolation, the cause of evolutionary trends and of the major patterns of waxing and waning of groups through time, then you don't have a fully Darwinian explanation for life's history.

I see Dawkins in a dual sense. On the one hand, he's the best living explainer of the essence of what Darwinism is all about. That part's very good. He's a kind of old-fashioned, nineteenth-century, almost atheistic scientific rationalist. The other side is the strict Darwinian zealot, who's convinced that everything out there is adaptive and is all a function of genes struggling. That's just plain wrong, for a whole variety of complex reasons. There's gene-level selection, but there's also organism-level and species-level. Those are his two sides: the professional true believer, on the one hand, and the excellent explainer of a worldview, on the other.

I'd question Richard on the issue of gene-level selection and why he thinks that the issue of organized adaptive complexity is the only thing that matters. I'm actually fairly Darwinian when it comes to the issue of so-called organized adaptive complexity, but there's so much more to the world out there. Why does he think that adaptation in that sense is responsible for interpreting everything in the history of

life? Why does he insist on trying to render large-scale paleontological patterns as though they were just grandiose Darwinian competitions? They aren't. He has this blinkered view in which the classic Darwinian question of adaptation is somehow becoming coextensive with all of evolutionary theory.

Richard and I are the two people who write about evolution best. He writes about microevolutionary theory, in a way I disagree with. I focus on the pattern of life's history and its relationship to evolutionary theory. I treat the fossil record and write about macroevolutionary theory, which he doesn't like. He writes on the nature of adaptation and on evolutionary theory in its traditional small-scale immediacy, and I write about the large-scale history of life.

Whether or not Darwin would be a Darwinist today, in the way the word is used, is so hard to say, because you have to make inferences about his mental flexibility. Given the set of ideas that he himself promulgated, I think he would, because his tendency in argument was always to try and stretch natural selection on bodies to cover cases. He was willing to allow a few very circumscribed exceptions, like his invocation of group selection for the evolution of human moral behavior—an important exception, to be sure, because we care about human moral behavior. But he circumscribed it in such a way that it could apply to no other species, because he invoked a group-selection mechanism that could work only in highly cognitive species that are sensitive to the "praise and blame of their fellows"—those are his words—and we're the only such species. So therefore he set up the exception in such a way as to marginalize it; it's an important one, because it's about us and we care about us, but it's not important in the full realm of nature.

On the other hand, if you want to speculate psychologically, Darwin was an enormously flexible, brilliant, and radical thinker, so I suspect that when he learned about asteroidal impact and mass extinction and maybe even punctuated equilibrium, he would be open. I doubt that he expected that a hundred years after his death things would be exactly as he had left them.

STUART KAUFFMAN: Steve is extremely bright, inventive. He thoroughly understands paleontology; he thoroughly understands evolutionary biology. He has performed an enormous service in getting people to think about punctuated equilibrium, because you see the

process of stasis/sudden change, which is a puzzle. It's the cessation of change for long periods of time. Since you always have mutations, why don't things continue changing? You either have to say that the particular form is highly adapted, optimal, and exists in a stable environment, or you have to be very puzzled. Steve has been enormously important in that sense.

Talking with Steve, or listening to him give a talk, is a bit like playing tennis with someone who's better than you are. It makes you play a better game than you can play. For years, Steve has wanted to find, in effect, what accounts for the order in biology, without having to appeal to selection to explain everything—that is, to the evolutionary "just-so stories." You can come up with some cockamamie account about why anything you look at was formed in evolution because it was useful for something. There is no way of checking such things. We're natural allies, because I'm trying to find sources of that natural order without appealing to selection, and yet we all know that selection is important.

MARVIN MINSKY: What I love about Stephen Gould is his ability both to research and to explain the possible evolutionary pathways that might have led to what we see in particular cases. His explanations and hypotheses are constructed from the most diverse kinds of evidence, by combining both general principles and particular details from many different fields. It's a wonder to see so many aspects synthesized at all—and perhaps more of a wonder to see them described with such beauty and clarity.

NILES ELDREDGE: Steve and I are like brothers, and when we get together we mostly like to talk about the things we disagree on, but of course the rest of the world is hard pressed to see how we differ on anything at all. Yet we do. That, to us, is the most interesting stuff. When we first wrote the punctuated-equilibrium papers, I thought it was more about mode and Steve thought it was more about tempo, using the two phrases from George G. Simpson's *Tempo and Mode in Evolution*. We had a different take on what it all meant. I think to some degree we probably still do.

Steve is prodigious. I never met somebody who was so smart who worked so hard. He is a marvelous scholar. I have never found anybody who could grasp the essence of an issue so quickly, either. He