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Abstract Review of *Evolution and Mormonism: A Quest for Understanding* (2001), by Trent D. Stephens and D. Jeffrey Meldrum.

OF SCIENCE, SCRIPTURE, AND SURPRISE

Duane Boyce

Review of Trent D. Stephens and D. Jeffrey Meldrum. *Evolution and Mormonism: A Quest for Understanding*. Salt Lake City: Signature Books, 2001. xxii + 238 pp. \$19.95.

In *Evolution and Mormonism*, Trent Stephens and Jeffrey Meldrum make a passing remark about the nature of scientific practice. In the course of discussing one common scientific conclusion, they say that any rebuttal must provide a reasonable alternative explanation for the same data. Such alternative hypotheses are encouraged, they tell us, and add that, speaking of science generally, “there is no conspiracy . . . to suppress reasonable hypotheses” (p. 114).

I wish they had said more about this. Stephens and Meldrum are accomplished scientists,¹ but as it stands this description of scientific attitude and practice says too little. It captures well enough the *ideal* of scientific practice but not enough of the day-to-day reality. In actual practice science is not as tidy as the statement suggests.

I express appreciation to Cody Carter, Scott Ritter, and Terry Warner, who responded helpfully to various sections of this paper in earlier drafts. They are not responsible, of course, for the use I have made of their suggestions or for the end product: my conclusions, my point of view, or any errors that remain. For example, Ritter (at least) surely disagrees with my prediction at the end of the paper.

1. Their treatment of DNA and the Book of Mormon, for example, is one of my favorite papers on the subject. See D. Jeffrey Meldrum and Trent D. Stephens, “Who Are the Children of Lehi?” in *Journal of Book of Mormon Studies* 12/1 (2003): 38–51; see also the compilation of Maxwell Institute articles on this subject in Daniel C. Peterson, ed., *The Book of Mormon and DNA Research* (Provo, UT: Neal A. Maxwell Institute, 2008).

This is not an insignificant matter. To the extent that we know only the ideal, and assume that actual scientific practice is a perfect reflection of that ideal, we will be naïve about scientific disciplines and about the steady stream of intellectual conclusions we encounter, in one form of media or another, that flow from them. Such naïveté can cause us to become sloppy and gullible in our thinking and slip into a too-easy acceptance of whatever intellectual pronouncements happen to come our way. It is then all too easy to actually become dogmatic and to adopt a level of certainty that is out of all proportion to our actual acquaintance with the evidence and with the internal logic of the claim in question.

Nowhere is this better illustrated than in two recent biographies of Joseph Smith by Richard D. Anderson and Dan Vogel, respectively.² Both authors rely heavily on the Freudian psychoanalytic tradition to justify their biographical speculations about the activities and motivations of the Prophet. Unfortunately, though worshipped by many for nearly half a century, for sound intellectual reasons Freud's psychoanalytic theory is passé today as a scientific program, and it has been for decades. Thus, although Anderson and Vogel accept the theory unquestioningly, they could hardly be more mistaken.³ Their naïveté

2. See Richard D. Anderson, *Inside the Mind of Joseph Smith: Psychobiography and the Book of Mormon* (Salt Lake City: Signature, 1999); and Dan Vogel, *Joseph Smith: The Making of a Prophet* (Salt Lake City: Signature, 2004).

3. Latter-day Saint authors have done a good job of debunking these books and of identifying weaknesses in psychoanalytic theory along the way. See Michael D. Jibson, "Korihor Speaks, or the Misinterpretation of Dreams," *FARMS Review of Books* 14/1 (2002): 223–60; and Andrew H. Hedges and W. Dawson Hedges, "No, Dan, That's Still Not History," *FARMS Review* 17/1 (2005). (Jibson's treatment of psychoanalytic theory is the more complete of the two.) Karl Popper famously complained about the empirical emptiness of psychoanalytic theory, observing that it could be used to explain anything and yet yielded few if any testable predictions—a conclusion that over time came to be widely shared. (One cannot help but suppose that authors who want to discredit the claims of the Prophet Joseph Smith find psychoanalytic theory congenial, despite its scientific vacuousness, precisely *because* they can use its web of concepts to explain anything—and to explain it in the way they want—without any risk of having their claims disconfirmed, however wildly implausible they may be in light of actual historical evidence.) Furthermore, the deepest level of Freud's theory—his attempt, through postulation of various mechanisms and compartments of the mind, to account for (1) his patients' apparently strategic resistance to his therapeutic interventions and

on the matter has betrayed them, leading both into a sorely misplaced confidence with regard to a subject of the greatest importance.

But this is only one example. Misplacement of confidence can happen anytime and practically anywhere. Scientific advance is not a smooth linear process, much less a short one. All along the way (especially in less mature disciplines, but to some degree in all) there are surprises: new discoveries, failed tests, altered hypotheses, failed interpretations, rival explanations, revised assumptions, exposed prejudices, and so forth. At any point in this journey (again, especially in less mature disciplines, but to some degree in all), particularly *between* surprises, many well-informed people may think finality has been reached on a particular scientific matter—and yet be shown, by the next surprise along the way, to be mistaken. And some of this occurs not simply because scientific advance in itself requires hard, laborious work over time—which it does—but also because straightforward, objective empiricism can sometimes experience difficulty, at least in the short term, in overcoming the prevailing scientific expectations and cultural assumptions of the time.⁴

A further question. That is why I wish Stephens and Meldrum had decided to say more about this topic than they do. The question they have in mind goes something like this: “Does science encourage new hypotheses, or is there instead a conspiracy to suppress such hypotheses?” The answer they give is the answer of the prescriptive ideal: “Of course science encourages new hypotheses, and of course there is no conspiracy of suppression.” That’s all fair enough. I only wish they

(2) their apparent unawareness of this strategic resistance—is conceptually problematic if not impossible. See for example, Jean Paul Sartre, *Being and Nothingness* (New York: Washington Square Press, 1966), esp. 86–96; Herbert Fingarette, *Self-Deception* (New York: Basic Books, 1969); and C. Terry Warner, “Anger and Similar Delusions,” in Rom Harré, ed., *The Social Construction of Emotion* (Oxford: Blackwell, 1986), 135–66.

4. It is true, of course, that hypothesis generation and hypothesis testing lie at the heart of the empirical disciplines. It is also true that careful publication of empirical findings enables lab experiments to be repeated and tested by other labs; one scientist’s data can be scrutinized and corrected by others. It’s for this reason that we can justifiably think of science as constantly improving. Through the process of hypothesizing, testing, interpreting, publishing, retesting, reinterpreting, and so forth, science eventually corrects itself. But it is also true that there is a lot of meaning packed into that word *eventually*. Showing just a bit of that meaning is one of the purposes of this paper.

had asked a further question, one that would have rounded out their treatment of scientific attitudes and practice. That question is, “Are there any features of scientific investigation that, by themselves, and without any conscious conspiracy, exert a suppressive influence on the generation and acceptance of new hypotheses?”

This question is important because the answer is yes. Of course there are features of scientific investigation that limit and suppress new hypotheses. A primary purpose of this paper is to illustrate this—to show at least at a certain level of detail the types of difficulties that can occur in the scientific process and thus to show why we must have a more nuanced view of scientific attitudes and practice than just a statement of the ideal. I assume that Stephens and Meldrum agree with this view and that what I present in this paper is similar to what they would have said had they decided to take up this part of the topic. I have no doubt that they are as interested as I am in avoiding a naïve reading of science.

Of course, one implication of the complex, bumpy, and surprising nature of scientific inquiry is that we are guaranteed to be mistaken in our beliefs, to one degree or another, more often than we might like to suppose and perhaps even on topics where our confidence is the highest. I think that’s unavoidable.

Naïve vs. Nuanced Views of the Scriptures

Of course, if it is important to avoid a naïve reading of science, it is equally important to avoid a naïve reading of the gospel. Beyond core fundamentals (which can be known with complete certainty through the Spirit), the scriptures also touch on many subjects that can be read in different ways by different students, all of them devoted to the gospel; because revealed knowledge on such topics is so incomplete, they simply elude final interpretation. If we do not understand this, we can naïvely accept and then, come what may, hold on to whatever point of view we encountered *first* on some topic. This too is naïve, and a mistake.⁵

5. To see how we can be in error, even on scriptural topics, consider the new light that meticulous gospel scholarship has shed on Latter-day Saint understanding of Book of Mormon culture and its likely geographical locations. That has certainly been a sur-

Because our knowledge is so incomplete, I believe we will encounter an endless panorama of surprises—and on a host of scriptural topics—once we pass through the veil. We will then fully appreciate just how fragmentary our understanding has been.

Tentativeness and Humility

So in both scientific and gospel scholarship, recognizing the difficulty of comprehension in both, there is reason for a lingering tentativeness and humility about many of the beliefs we hold at any one time. No matter how much we think we know, it's probably best to live with the expectation that, despite our best efforts, in both arenas, we will turn out to be wrong on a host of matters. Many of these may be small, of course, but some of them perhaps pretty large. We should live in anticipation of surprise. The alternative, it seems to me, is to risk intellectual and even spiritual gullibility, and likely an attitude of naïve dogmatism that will turn out in the end to have been misguided and false, not to mention psychologically costly, all along the way.⁶

prise—exploding more than a century's worth of assumptions common among the general membership of the church (which includes me). For just a few examples of the burgeoning literature on Book of Mormon geography, see John L. Sorenson, *An Ancient American Setting for the Book of Mormon* (Salt Lake City: Deseret Book, 1985) and *Mormon's Map* (Provo, UT: FARMS, 2000); John E. Clark, "A Key for Evaluating Nephite Geographies," *Review of Books on the Book of Mormon* 1/1 (1989): 20–70 and "Searching for Book of Mormon Lands in Middle America," *FARMS Review* 16/2 (2004): 1–54. Perhaps all of this shouldn't have been such a surprise—see Matthew Roper, "Limited Geography and the Book of Mormon: Historical Antecedents and Early Interpretations," *FARMS Review* 16/2 (2004): 225–75—but it has been.

6. As I take a look at science, I should say at the outset that I do not approach this topic from a radical perspective. This is an important proviso because in the last twenty-five years many studies of the nature of science—usually written by nonscientists (though not necessarily scientifically illiterate nonscientists)—have arisen under headings such as "postmodernism," "science studies," and "the sociology of knowledge," all attempting to undermine simplistic notions of scientific practice and objectivity. While it is valuable to study science in order to do this, a fair amount of this literature claims far too much about the relativism of science, and some of it is just silly. These movements have often overinterpreted some of the themes found in Karl Popper, Thomas Kuhn, and W. V. Quine—very important scholarly figures and intellectual heroes of mine—and have tended to claim too much based on them (and indeed have overlooked some of their revisions). It is true, as I will try to illustrate, that scientific practice is not as strictly straightforward, uncluttered, and objective as simple descriptions sometimes imply. But to acknowledge

So I will first try to show why the description of scientific practice given by Stephens and Meldrum—indeed, why any such generalization—says too little. I will then look at their treatment of one scriptural passage and then at a scriptural matter that, unfortunately, they do not treat at all. Finally, I will illustrate why we should be enthusiastically open to surprise in scriptural as well as in strictly intellectual matters.

Of Science

My approach in looking at science will be to draw on behind-the-scenes reports of one who was himself a notable scientist: Stephen Jay Gould, prominent paleontologist and professor of zoology at Harvard and recently deceased at a comparatively young age.

that scientific progress is a complicated affair and that scientific conclusions at any given time are fallible is not to say that science is *arbitrary*, as some seem to believe.

Kuhn was one of my teachers, and I didn't see at the time the forms of relativism that have been imputed to him, though it is true that he modified some of his views over the years. Indeed, he spent the last couple of decades of his life denying the strong forms of relativism that were imputed to him by, I think, both critics and acolytes. See, for example, his remarks in "Paradigms of Scientific Revolution," in G. Borradori, *The American Philosopher: Conversations with Quine, Davidson, Putnam, Nozick, Danto, Rorty, Cavell, MacIntyre, and Kuhn* (Chicago: University of Chicago Press, 1994), 153–67; Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1970), 205–7; and Frederick Suppe, *The Structure of Scientific Theories*, 2nd ed. (Urbana: University of Illinois Press, 1977), 508.

In general, my view of science studies is best captured in Philip Kitcher, "A Plea for Science Studies," in Noretta Koertge, ed., *A House Built on Sand: Exposing Postmodernist Myths about Science* (Oxford: Oxford University Press, 1998), 32–56, which can be summed up as "Do it, but do it *well*"—a more controversial view than it may seem. On the philosophy of science in general, the best summary of twentieth-century efforts up to the postmodern period is Suppe, *The Structure of Scientific Theories* (cited above). Good sources that address developments since then are Hugh G. Gauch Jr., *Scientific Method in Practice* (Cambridge: Cambridge University Press, 2003); and Christopher Hitchcock, *Contemporary Debates in Philosophy of Science* (Oxford: Blackwell, 2004). To get a sense of the so-called "science wars" that ultimately developed around the appearance of "science studies" and the like, see Koertge, *A House Built on Sand* (cited above); Harry Collins and Trevor Pinch, *The Golem: What You Should Know about Science*, 2nd ed. (Cambridge: Cambridge University Press, 1998); Paul R. Gross and Norman Levitt, *Higher Superstition: The Academic Left and Its Quarrels with Science* (Baltimore: Johns Hopkins University Press, 1994); Andrew Ross, ed., *Science Wars* (Durham, NC: Duke University Press, 1996); and Keith Parsons, *The Science Wars: Debating Scientific Knowledge and Technology* (Amherst, NY: Prometheus, 2003).

Gould is convenient for three reasons. The first is that as a practicing scientist he furnishes more of an inside perspective than many authors who write about science are able to provide. The second is that Gould shares his experience and perspective without reservation. Written in a personal voice, his 2002 tome (1433 pages in all), *The Structure of Evolutionary Theory*,⁷ is heavily drenched in historical and behind-the-scenes personal examples of the scientific process at work. Reading Gould we can see some (at least in the way he describes them) of the inner workings of investigation, explanation, and even publication in at least one branch of science. The third reason Gould is handy is that, by happy coincidence, he writes in the same field as Stephens and Meldrum. I think this correspondence of topics, though not necessary, is at least useful for an essay in response to their book.

Gould is a controversial figure, of course. On one hand he is accused of attributing too much distinctiveness to what must be his most famous writings on evolutionary theory (“punctuated equilibrium,” to be discussed momentarily), and on the other hand he is resented for becoming the poster child for those who wish to debunk mainstream evolutionary thought.⁸ I believe these are the primary reasons for his controversial

7. Stephen Jay Gould, *The Structure of Evolutionary Theory* (Cambridge, MA: Harvard, 2002). In-text citations of page numbers from this point until note 24 refer to this book; subsequent page citations refer to the Stephens and Meldrum book.

8. For example, because Gould challenges some features of mainstream Darwinian theory, some writers have used his work to cast doubt on evolutionary theory altogether—to Gould’s utter dismay (see, for example, his “Evolution as Fact and Theory,” in *Hen’s Teeth and Horse’s Toes* [New York: W. W. Norton, 1994], 253–62, and *The Structure of Evolutionary Theory*, 986–90). This has caused chagrin among other scholars in the field, who have thought that Gould too easily invites just this kind of misuse. As to some of Gould’s departures from mainstream views, Richard Lewontin and Richard Levins (both Harvard colleagues of Gould’s) remarked at the time of his death that “the task of the biologist, insofar as it is to provide explanations, is to come up with a reasonable story of why any particular feature of a species was favored by natural selection. If, when the human species lost most of its body hair in evolving from its ape-like ancestor, it still held on to eyebrows, then eyebrows must be good things. A great emphasis of [Gould’s] scientific writing was to reject this simplistic Panglossian adaptationism, and to go back to the variety of fundamental biological processes in the search for the causes of evolutionary change. He argued that evolution was a result of random as well as selective forces and that characteristics may be the physical byproducts of selection for other traits. He also argued strongly for the historical contingency of evolutionary change. Something may be

status, and I have no interest in either. I will simply draw attention to three facets of scientific investigation (among many that appear in his book) that will help bring a bit of nuance, and thus soberness, to our understanding of the scientific process.

Although, for the reasons mentioned, I draw my examples from Gould's writings on evolutionary theory, I use them merely as examples of what can happen in scientific investigation generally. My concern throughout is not with evolutionary theory per se, but with the broader issue raised by Stephens and Meldrum of all scientific investigation, whatever the discipline. My only comment about evolution will be a modest prediction at the end.

I. Publication Bias and Distortion of the Scientific Record

Gould and colleague Niles Eldredge developed a modification of evolutionary theory in the early 1970s that they called "punctuated equilibrium." The concept arose from data in the fossil record that paleontologists had long observed: (1) when species appear in the geological record, they do so suddenly (geologically speaking), without evidence of gradual evolutionary development beforehand; and (2) most species appearing in the fossil record do not exhibit observable change over time—they look about the same when they disappear from the record as they did when they first arrived.⁹ The term *punctuated* captures the idea of this sudden appearance; *equilibrium*,

selected for some reason at one time and then for an entirely different reason at another time, so that the end product is the result of the whole history of an evolutionary line, and cannot be accounted for by its present adaptive significance." They also explain that "Gould went even further in his emphasis on the importance of major irregular events in the history of life. He placed great importance on sudden mass extinction of species after collisions of large comets with the Earth and the subsequent repopulation of the living world from a restricted pool of surviving species." Richard C. Lewontin and Richard Levins, "Stephen Jay Gould—What Does It Mean to Be a Radical?" *Monthly Review* 54/6, <http://monthlyreview.org/1102lewontin.htm> (accessed 24 October 2008).

9. Gould summarizes evidence of this consensus among paleontologists on pages 745–55 of *The Structure of Evolutionary Theory*. It is important to note that Gould limits the observation to species, not to higher taxonomic groups. He emphasizes that "transitional forms are generally lacking at the species level, but they are abundant between larger groups." Gould, "Evolution as Fact and Theory," 253–62.

or *stasis*, captures the idea of the evident morphological stability of species over time.¹⁰

According to Gould, these observations of sudden appearance followed by stasis were thought by paleontologists in general to depart from standard Darwinian expectations. Paleontologists, he tells us, attributed to conventional Darwinism the view that (1) all evolutionary change is necessarily gradual over whole populations and that (2) natural selection operates exclusively at the level of the individual organism. But if these claims are true, the fossil record ought to show literally countless examples of gradual species development through changes in individual organisms. Since it doesn't show this, paleontologists felt forced, on what they took to be sound Darwinian grounds, to see the contrary fossil evidence as simply an artifact of an imperfect geological record—either the expected transitional intermediates

10. Building on the work of others, Gould and Eldredge explained this observation by hypothesizing that species can divide to form new species by the geographical isolation and subsequent genetic differentiation of one population of a species from the parent population, and that when this occurs the new formation is analogous to the birth of new individual organisms. Such new species face the forces of natural selection just as individual organisms face them, resulting over time in the selection of some species and the elimination of others. As recently as his last book, *The Structure of Evolutionary Theory* (cited in note 7), Gould repeatedly contrasts this view of the fossil record and this mechanism for evolutionary change with “Darwin” or “classical Darwinism.” He attributes to classical Darwinism the view that the accumulation of large-scale macroevolutionary changes occurs only through gradual transformation of the whole species as a single population (“anagenesis”) and that the kind of “branching” or “splitting” described by Gould and Eldredge (and others before them) plays no important role in forming such macroevolutionary modifications. Herein lies controversy because critics have claimed that this is a mischaracterization of Darwin, arguing that every tenet of punctuated equilibrium was anticipated (at least) by Darwin himself and so could hardly be considered “non-Darwinian,” as Gould claims. See, for just one example, Wesley R. Elsberry, “Punctuated Equilibria,” <http://www.talkorigins.org/faqs/punc-eq.html> (accessed 28 October 2008). But Gould is not without answer. He argues (e.g., *The Structure of Evolutionary Theory*, 147–49) that such criticisms are misguided because they rely on selective quotations from Darwin and ignore the overall tenor of his work as well as its central *logic*—both of which support gradualism as the “central conviction residing both within and behind all Darwin’s thought” (p. 148). He points out that Darwin said “something about nearly everything” and therefore that, if taken out of context and divorced from everything else said by Darwin and entailed by the central logic of his work, “a Darwinian statement can be found to support almost any position, even the most un-Darwinian” (p. 148; see also pp. 784–822 and 990–1022, where Gould addresses a wide range of criticisms of his work).

(which, again, should be countless) for one reason or another simply failed to fossilize, or scientists just hadn't been looking long enough to be able to discover them.

Omissions from the Scientific Record

As a result of this view of the matter, Gould tells us, paleontologists came to consider every instance of stasis in the fossil record as a departure from Darwinian expectations and therefore as “just another failure to document evolution” that “certainly did not represent anything worth publishing” (p. 760). As Gould tellingly observes,

Paleontology therefore fell into a literally absurd vicious circle. No one ventured to document or quantify—indeed, hardly anyone even bothered to mention or publish at all—the most common pattern in the fossil record: the stasis of most morphospecies throughout their geological duration. (pp. 759–60)

According to Gould, the canonical response of paleontologists to this state of affairs was “a quite unconscious conspiracy of silence”—they didn't publish what they knew; after all, “few scientists write papers about failure to document a desired result” (p. 759), in this case, evidence of gradual evolutionary change in species. As Gould asks rhetorically to demonstrate a practical reality about scientific choices, “How many scientists will devote a large chunk of a limited career to documenting a phenomenon that they view as a cardinal restriction recording a poverty of available information?” (p. 761). The answer, of course, is “not many.” And he adds that many samples of species have never been documented in detail because “their apparent stasis seems ‘boring’ to students of evolution” (p. 763). “As a consequence,” he tells us, “most nonpaleontologists never learned about the predominance of stasis, and simply assumed that gradualism must prevail, as illustrated by the exceedingly few cases that became textbook ‘classics’” (p. 760).¹¹

11. As such textbook classics, Gould mentions the coiling of the oyster *Gryphaea* and the increasing body size of horses and adds that “nearly all these ‘classics’ have since been disproved, thus providing another testimony for the temporary triumph of hope and expectation over evidence” (p. 760).

So scientists in fields other than paleontology never learned the truth about the fossil record, at least to the extent that it was known among paleontologists, *because paleontologists were not publishing works that would tell them*. Defined a priori as nonevidence of what they were all looking for (i.e., patterns indicating gradual species development), there was nothing about stasis in the geological life of a given species that made it worth publishing. Thus, based only on textbook orthodoxy, which was all that was available to them as nonspecialists, for a century and more many scientists mistakenly believed and propounded the view that the fossil record, taken at face value, substantiated claims of gradualism and of exclusively organismic (versus, for example, species-level) evolution.

When the paleontological reality finally became known (or as Gould says, when “this fundamental phenomenon finally emerged from the closet”), nonpaleontologists were “often astounded and incredulous” (p. 760). For example, most evolutionary biologists, we are told, had not expected this result because little in the way of published research or discussion by paleontologists had *led* them to expect it.

Gould aptly illustrates the grip this vicious circle had on even the most elite of scientists:

To cite a personal incident that engraved this paradox upon my consciousness early in my career, John Imbrie served as one of my Ph.D. advisors at Columbia University. This distinguished paleoclimatologist began his career as an evolutionary paleontologist. He accepted the canonical equation of evolution with gradualism, but conjectured that our documentary failures had arisen from the subtlety of gradual change, and the consequent need for statistical analysis in a field still dominated by an “old-fashioned” style of verbal description. He schooled himself in quantitative methods and applied this apparatus, then so exciting and novel, to the classic sequence of Devonian brachiopods from the Michigan Basin—where rates of sedimentation had been sufficiently slow and continuous to record any hypothetical gradualism. He studied more than 30 species in this novel and rigorous way—and found

that all but one had remained stable throughout the interval, while the single exception exhibited an ambiguous pattern. But Imbrie did not publish a triumphant paper documenting the important phenomenon of stasis. Instead, he just became disappointed at such “negative” results after so much effort. He buried his data in a technical taxonomic monograph that no working biologist would ever encounter (and that made no evolutionary claims at all)—and eventually left the profession for something more “productive.” (p. 760)

Borrowing from the medical and social sciences, Gould describes this phenomenon as “publication bias”—a bias by which “prejudices arising from hope, cultural expectation, or the definitions of a particular scientific theory dictate that only certain kinds of data will be viewed as worthy of publication, or even of documentation at all” (p. 763). This was precisely the case with paleontologists’ nonreporting of stasis as the overwhelmingly dominant pattern in the fossil record, a bias that resulted in serious omission of empirical observations from the scientific literature on the topic.

Distortion of the Scientific Record

But omission of an important and widespread empirical observation was only part of the problem. In a historical field such as paleontology, Gould frequently reminds us (e.g., pp. 147, 761, 803, 854–74), the central matter is one of relative frequencies, that is, how often a given pattern occurs in comparison to other possible patterns. For example, how often does stasis appear in the fossil record as opposed to cases of gradualism? When relative frequencies of this sort are of such importance, publication bias is particularly problematic. To omit data about any one pattern by failing to publish it not only excludes that evidence from the scientific record (one kind of problem, serious in its own right), but it also automatically distorts any conclusions we want to draw about the relative frequencies of patterns that do appear in the literature. The published patterns will always appear dominant simply because contrary patterns have not even been published—or published only rarely—and this because “no other style of evolution

attracted study” in the first place (p. 855). According to Gould, this is precisely what occurred: traditional paleontologists “eliminated examples of abrupt appearance and stasis from the documentation of evolution”—and this meant that “the relative distributions of evolutionary rates [how often evolution appears to have occurred abruptly, geologically speaking, vs. how often it appears to have occurred more gradually] would therefore emerge only from cases of gradualism” (p. 761)—a case of sampling bias as serious as any I know.

But the problem worsens, for it is not just that the pattern of stasis was excluded from documentation or that this omission inherently distorted conclusions drawn from the patterns that *were* documented in the literature. The difficulty was deeper yet because, as Gould reports, “gradualism occurs too rarely to generate enough cases for calculating a distribution of [abrupt vs. gradual] rates” (pp. 761–62). In other words, all other problems aside, evidence of gradual development still does not appear frequently enough in the fossil record to permit the calculation of comparative frequency rates—and this in a discipline, Gould tells us, whose standard methodological procedure *just is* the calculation of frequency rates. As a result,

paleontological studies of evolution therefore became warped in a lamentable way that precluded any proper use of the fossil record. . . . Instead, paleontologists worked by the false method of exemplification: validation by a “textbook case” or two, provided that the chosen instances be sufficiently persuasive. And even here, at this utterly minimal level of documentation, the method failed. A few examples did enter the literature—where they replicated by endless republication in the time-honored fashion of textbook copying. But, in a final irony, almost all these famous exemplars turned out to be false on rigorous restudy. (pp. 761–62)

Gould then cites literature that demonstrates stasis in cases that had long been widely used as textbook examples of gradual development. He includes the oyster *Gryphaea*, all documented species of

fossil horses, and the antlers of the “Irish Elk” (pp. 761–62). Gould further laments the historical reality in the discipline:

Traditional paleontology therefore placed itself into a straight-jacket that made the practice of science effectively impossible: only a tiny percentage of cases [those showing gradualism] passed muster for study at all, while the stories generated for this minuscule minority rested so precariously upon hope for finding a rare phenomenon . . . that even these textbook exemplars collapsed upon restudy with proper quantitative procedures. (p. 763)

Then, in one of his many personal asides, Gould reports: “As Hallam said to me many years ago, after he had disproved the classical story of gradualism in *Gryphaea*: more than 100 other species of mollusks, many with records as rich as *Gryphaea*’s, occur in the same Liassic rocks, yet no one ever documented the stratigraphic history of even a single one in any study of evolution, for all demonstrate stasis. Scientists picked out the only species that seemed to illustrate gradualism, and even this case failed” (p. 763). Little wonder that Gould calls such publication bias—paleontology’s prime example of which was the nonreporting of stasis in the fossil record—“an insidious phenomenon in science that simply has not been recognized for the serious and distorting results perpetrated under its aegis” (p. 763).

II. Authority and Stigma

To illustrate another untidy element found in scientific practice, we begin with the finding in 1980 (by L. W. Alvarez et al.) of evidence that a catastrophic event had led to (at least) one case of mass extinction in the earth’s history. Gould describes as clearly “evidence-driven” Alvarez’s hypothesis of extraterrestrial impact as the cause of this catastrophe, and yet Gould reports that perhaps only he and one other invertebrate paleontologist of their generation “reacted with initial warmth to the impact hypothesis” (p. 1307). Despite the empirical case for the conclusion, Gould informs us that it was met with “rejec-

tion and outright disdain from nearly all established professional students of the fossil record” (p. 1307).

Lyell and Cuvier

Why would this be? If the evidence seemed immediately plausible and elegant to Gould and at least one other paleontologist, why not to all? To understand at least part of the reason for this resistance, at least as Gould sees the matter, we must consider two important historical figures, the French naturalist and zoologist Georges Cuvier (1769–1832) and the famous British geologist Charles Lyell (1797–1875).

Cuvier is a significant figure because, contrary to the received opinion of his time, he believed that most fossils were remains of species that had become extinct. Further, he believed they had become extinct through catastrophic events that had occurred over the course of the earth’s history, resulting in a succession of one fauna after another in the fossil record. This conclusion was reached by a specific scientific method. Gould says that Cuvier and other catastrophists

preached a radical empirical literalism: interpret what you see as a true and accurate record of actual events, and interpolate nothing. If horizontal strata overlie a sequence of broken and tilted beds, then a catastrophe must have terminated one world and initiated another, as the geological discontinuity implies. If fauna disappeared at such a boundary, and younger beds contain fossils of different creatures, then a mass extinction must have eradicated the older fauna. The catastrophists advocated directionalism as a primary theme for the earth’s history, and empirical literalism as a fundamental approach to science. (p. 485)

Whatever the evidence for it might be, catastrophism is not congenial to classical Darwinian theory. Darwin postulated the central role of competition among organisms in crowded environments to explain the progressive nature of natural selection. This allowed him, in Gould’s words, to “validate the central belief of his surrounding culture, the concept of progress, as a primary signal of life’s history” (p. 480). But this Darwinian commitment to orderly progress in

evolutionary outcomes was undermined by the possibility of large-scale events, such as catastrophes, that would have occurred randomly and produced random effects among existing populations. Inimical to the Darwinian vision of (primarily) orderly adaptation and progressive evolutionary development, the possibility of such mass extinctions and other large-scale environmental phenomena introduce, in Gould's words, "a powerfully confusing and potentially confuting new actor: the tumbling, whimsical wheel of fortune rather than the slow and steady wedge of progress" (p. 480).

Enter Charles Lyell, whose well-known argument for and commitment to a uniformitarian view of geological history was, unlike Cuvier's catastrophist theory, highly congenial to Darwin's preferred account of evolutionary mechanisms and of progressive development, and it was a position to which Darwin naturally gravitated. But he was hardly alone in this. Lyell's multivolume work on the principles of geology, published over the course of three years, was impressive to all, and his work became the rage in intellectual circles; indeed, his contribution stands as a major influence not only in geology but in the history of science generally.

But according to Gould, there's more to the story than the triumph of Lyell's arguments and evidence over Cuvier's (or anyone else's). He reports that Lyell's ascendancy ultimately owed as much to rhetorical flair (a "tricky rhetorical argument," he calls it, p. 482) as to any strength in documentation. Gould recounts:

In his most clever, and devastatingly effective, trope of rhetoric, Lyell argued that the substantive claims of "uniformity" must be valid because the basic practice of science requires that we accept a set of methodological assumptions bearing the same name despite their truly different status ("uniformity of law and process"). In so doing, Lyell managed to elevate a testable claim about gradualism to the status of a received *a priori* doctrine vital to the successful practice of science itself. This subtle conflation has exerted a profound, and largely negative, influence upon geology ever since, often serving to limit and stifle hypotheses about rates of processes,

and to bring derision upon those who advocated even local catastrophes. (p. 482)

As Gould informs us, the combination of Lyell's status and his utter rejection of catastrophism ultimately "placed catastrophism beyond the pale of scientific respectability." Catastrophists came to be seen as simplistic theological dogmatists who argued from biblical accounts of the creation and of the earth's history rather than from scientific evidence—this despite the fact that "anyone who knew Cuvier, Elie de Beaumont, or d'Orbigny recognized their mental power, their scientific integrity, and the considerable empirical support enjoyed by their systems. But when these men died, Lyell's characterization persisted, and 'catastrophism' became equated with anti-science and dogmatic theological reaction" (p. 482).

What's most interesting, and informative, is that the mischaracterization of Cuvier and the other catastrophists that began with the initial writings by Lyell more than 150 years ago has persisted to the present. Gould reports:

How ironic . . . that modern textbook cardboard should misidentify Lyell as an empiricist who, by laborious fieldwork and close attention to objective information, drove the dogmatists of catastrophism out of science. To the contrary, the catastrophists were the empirical literalists of their time! Lyell and Darwin opposed catastrophism by probing "behind appearance" to interpret, rather than simply to record, the data of geology. . . . Proper procedure in geology, Lyell asserted, requires that we interpolate into a systematically impoverished record the unpreserved events implied by our best theoretical understanding. Lyell and Darwin worked by interpretation and interpolation; the catastrophists preached empirical literalism! (pp. 485–86)

Gould hastens to add that he is not saying that Cuvier and the other catastrophists were right in methodology and Lyell and Darwin wrong. But, he says, he knows "no greater irony in the history of science than the inverted

posthumous reputations awarded to Lyell and the catastrophists for their supposed positions on ‘objectivism’ in science” (p. 486).

But there’s more to the story than mere irony. Gould further informs us that Darwin’s defense of Lyell succeeded “in directing more than a century of research away from any consideration of truly catastrophic mass extinction, and towards a virtually unchallenged effort to spread the deaths over sufficient time to warrant an ordinary gradualistic explanation in conventional Darwinian terms” (p. 1303). And he adds that uniformitarian presuppositions clamped “a tight and efficient lid upon any consideration of empirically legitimate and conceptually plausible catastrophic scenarios. Merely to suggest such a thing (as even so prominent a scientist as Schindewolf . . . discovered) was to commit an almost risible apostasy” (p. 1303).¹²

The stigma attaching both to any consideration of catastrophic events over earth’s history and to the early adherents of such considerations has endured. Gould remarks: “The arms of misreason extend across generations. When primary documents disappear from sight, textbook pap can clone itself, and resulting legends then beget further fantasy with little hope for correction within an established system of belief” (pp. 482–83). And he shares this aside:

The great works of Cuvier and other catastrophists have always remained on library shelves, and have been much valued by historians and collectors. But never doubt the power of false characterization to ban effective consideration of the readily available. A scientist beyond the pale becomes an object of ridicule without being read—and the force of silence should never

12. Gould notes with lament the extension of this theoretical commitment even into popular culture: “So strongly entrenched did this prejudice [toward uniformitarian assumptions and gradual change] remain, even spilling over into popular culture as well, that a few years after Alvarez et al. (1980) published their plausible, and by then increasingly well affirmed, scenario of extraterrestrial impact as a catastrophic trigger for the Cretaceous-Tertiary event, the *New York Times* [2 April 1985] even ridiculed the idea in their editorial pages,” saying that “astronomers should leave to astrologers the task of seeking the cause of earthly events in the stars” (p. 1303). Gould castigates the *Times* for its foray into such technical matters, matters about which newspapers in general demonstrate little competence, much less sophistication.

be underestimated. To cite just one personal anecdote about Cuvier and his *Discours préliminaire*: The stereotyped Cuvier stands accused in most textbooks for arguing that catastrophes wipe all life off the face of the earth, and that God then creates new biotas from scratch. But Cuvier never advanced such a claim. No doubt, when pressed, he would have accepted some new creation to replenish a depleted world. But he attributed much local faunal change across stratigraphic boundaries to migration from previously isolated areas following geographic alterations that accompany episodes of rapid geological change (citing, as a potential example, the migration of Asian mammals to Australia should a land bridge ever connect these continents). Cuvier didn't hide this argument; he presents his viewpoint prominently in Section 30 of the canonical Jameson translation (1818, pp. 128–29). Yet, at least a half dozen times in my professional life, colleagues ranging from graduate students to senior professors have approached me with excitement, thinking that they had just made an important and original discovery: “Hey, look at this. Cuvier didn't believe in complete replacement by new creation . . .” “Yes,” I reply, “page 128; the passage has always been there.” (pp. 482–83)

Thus, although one of the greatest intellects of the nineteenth century and an empiricist of the first order, Cuvier became stigmatized (through the influence of Lyell, who was also a great intellect) as theologically dogmatic and *anti*-empiricist. According to Gould's account, Cuvier and his scientific conclusions, and even anything approaching his conclusions, were excluded from respectable consideration by the intellectual community for generations.

Alvarez

This brings us back to our starting point: Alvarez and the resistance of the paleontological community to his hypothesis of mass extinction through catastrophic impact. From the historical perspective provided by Gould, it appears that the paleontological community resisted Alvarez's conclusion primarily because of the century-and-a-half-long

momentum of scientific bias against *any* conclusion of that kind. The evidence, Gould tells us (and which he reviews), was elegant and plausible, and “bursting with seeds of testability,” it “should have caught the attention and intrigue of all scientists from the start” (p. 1307).

But the anti-catastrophic biases of Lyellian and Darwinian traditions ran so deep, and the knee-jerk fear and disdain of paleontologists therefore stood so high, that even this welcome novelty of operationality did not allay rejection and outright disdain from nearly all established professional students of the fossil record (whereas other relevant subdisciplines with other traditions, planetary scientists and students of the physics and engineering of impacts, for example, reacted in markedly more mixed or positive ways). (p. 1307)

So on Gould’s account the resistance to Alvarez was due not to overwhelming countervailing evidence or arguments, but instead to “biases,” “knee-jerk fear,” and “disdain,” the histories of which can all be traced back more than 150 years to the Lyellian repudiation of catastrophism in general. Tying the historical pieces together, Gould remarks that “no one can comprehend the emotional vigor of the debate engendered by Alvarez’s proposal for catastrophic mass extinction by extraterrestrial impact without understanding the historical legacy of Lyell’s successful and tricky rhetorical argument against catastrophism” (p. 482).

It should be noted that, according to Gould, the Alvarez finding does not imply “a general theory of mass extinction” (p. 1311). However, it does validate the assertion that “catastrophism contained important elements of validity from the start” (p. 483). For Gould it is unfortunate that these elements could not be appreciated by the scientific community given the tenor and the theoretical commitments of the times, and that they have had a difficult time being appreciated even recently. Of his own attempts to remedy the situation, Gould reports:

I have, in my own writings, tried to summarize the theoretical importance of readmitting truly catastrophic scenarios of mass extinction back into scientific respectability (after 150

years of successful Lyellian anathematization) by stating an emerging consensus about four crucial and general features of such events, each strongly negative (and, in their ensemble, probably fatal) for the key extrapolationist premise needed to maintain a claim of exclusivity for a strictly Darwinian theory of evolutionary process . . . : mass extinctions are *more frequent, more rapid, more intense, and more different in their effects* than paleontologists had suspected, and than Lyellian geology and Darwinian biology could permit. (pp. 1312–13)

The stigma attaching to Cuvier and catastrophism affected not only the reputations of individual scientists and their work, but also what other scientists found worthy of investigation in their own work, as well as what they found worthy of respect—whatever the evidence—in the work of others. And it was an effect that was still being felt 150 years after its origins.¹³

III. Authority and the Bandwagon Effect

The negative effects of authority in a scientific discipline are not restricted to the stigmatizing of particular scientists or particular theories. Sometimes they are more subtle, though no less powerful and constricting in their effects.

“Hardening” and the Modern Synthesis

One example is Gould’s description of what happened to evolutionary theory in roughly the final half of the last century and extending

13. John Sorenson notes the effects of professional stigma in ignoring evidence relevant to the Book of Mormon. For example, he identifies “dogmatically opinionated experts” as the impediment to serious consideration of possible transoceanic voyages in ancient times—voyages like those described in the Book of Mormon. The resistance by such experts has occurred despite abundant evidence and has decisively affected general academic attitudes on the topic. See John L. Sorenson, “Ancient Voyages Across the Ocean to America: From ‘Impossible’ to ‘Certain,’” *Journal of Book of Mormon Studies* 14/1 (2005): 4–17. He also describes the chilling, and costly, effects of academic authority and intimidation on efforts to decipher the Maya hieroglyphics and other aspects of Mayan civilization. See John L. Sorenson, “Viva Zapato! Hurray for the Shoe!” review of “Does the Shoe Fit? A Critique of the Limited Tehuantepec Geography,” by Deanne G. Matheny, *Review of Books on the Book of Mormon* 6/1 (1994): 297–361.

to the present. Gould's story begins with what is known as "the modern synthesis" (variously called "the synthetic theory" or "the modern synthetic theory"), beginning in the 1930s and having as a major early milestone the publication in 1942 of Julian Huxley's book *Evolution: The Modern Synthesis*. In Gould's view, "Huxley obviously felt that the morphology of evolutionary consensus could best be described as a synthesis—that is, a gathering together of previously disparate elements around a central core" (p. 503). This modern synthesis is traditionally thought of as consisting of two phases: (1) the synthesis of Darwin with Mendelian genetics and (2) a growing similarity in the contributions made by different disciplines to evolutionary theory.

But Gould also describes the history of the modern synthesis as including a dimension of intellectual "hardening," specifically hardening around natural selection as the exclusive agent of evolutionary change. Whereas in the first phase of the synthesis scholars "tended to agnosticism about relative frequencies among the legitimate phenomena, notably on the issue of drift (and other random phenomena) vs. selection" (p. 505), later, Gould notes, "the leading synthesists promoted natural selection, first to a commanding frequency and then to virtual exclusivity as an agent of evolutionary change. This consensus hardened to an orthodoxy, often accompanied by strong and largely rhetorical dismissal of dissenting views—a position that reached its acme in the Darwinian centennial celebrations of 1959" (p. 505). This position "eventually narrowed to a restrictive faith in what Weismann had called the 'all-sufficiency' of natural selection, with the accompanying requirement that phenotypes be analyzed as problems in adaptation" to the virtual exclusion of any other factors. "This hardening extended beyond overconfidence in adaptation to a more general, and sometimes rather smug, feeling that truth had now been discovered, and that a full account of evolution only required some mopping up and adumbration of details" (p. 505). Gould reports that "confidence in adaptation grew so great that many symposiasts [at the 1959 celebratory conference] presented their arguments in a 'can't fail' manner, by delimiting a set of supposedly inclusive outcomes, each validating adaptation for any conceivable result" (p. 572).

Gould reports that although in the beginning the synthesis was pluralistic and exploratory in character, it “had hardened by 1959 into a set of core commitments that, at least among epigones and acolytes, had become formulaic and almost catechistic, if not outright dogmatic” (p. 570).¹⁴ Illustrating just how much an assumed feature of the intellectual landscape this dogma had become, Gould tells how even such a monumental figure as Theodore Dobzhansky failed to understand central elements regarding population genetics of the synthesis and happily accepted them “on faith” (p. 520).

Dobzhansky’s willingness to accept an incomprehensible literature, and the later acquiescence of so many leaders from other subdisciplines (largely via Dobzhansky’s “translation”) testify to a powerful shared culture among evolutionists—a set of assumptions accepted without fundamental questioning or perceived need to grasp the underlying mechanics. Such a sense of community can lead to exhilarating, active science (but largely in the accumulative mode, as examples cascade to illustrate accepted principles). As a downside, however, remaining difficulties, puzzles, anomalies, unresolved corners, and bits of illogic may retreat to the sidelines—rarely disputed and largely forgotten (or, by the next generation, never learned). This situation may sow seeds of an orthodoxy that can then become sufficiently set and unchallenged to verge on dogma—as happened in many circles, at least among large numbers of epigones, at the acme of the Synthesis in the late 1950’s and 1960’s. (p. 520)

14. Here is Gould’s description of the synthetic catechism, including its implications for paleontology: “The Synthetic approach to macroevolution can be encapsulated in a few dicta: view life as stately unfolding under adaptive control; depict trends as accumulative and anagenetic within lineages according to the extrapolationist model [i.e., extrapolating principles of microevolutionary change to explain all macroevolutionary change]; downplay or ignore the macroevolutionary calculus of birth and death of species. These propositions leave little role for the actual archives of life’s history—the fossil record—beyond the *documentation* of change. The *causes* of change must be ascertained elsewhere, and entirely by neontologists (my profession’s term for the folks who study modern organisms). Thus the Synthesis held paleontology at arm’s length” (p. 564).

One is reminded of the influential Hungarian philosopher of science Imre Lakatos. Working from the London School of Economics concurrently with Karl Popper, Lakatos argued (among other things) that, at some point in the development of any significant “research programme” (i.e., a set of related theories that share a similar “hard core”), the basic assumptions of the program ultimately come to be rendered irrefutable simply by force of the consensus of the participants. Once this point is reached those assumptions are thereafter protected by what Lakatos calls the “negative heuristic” of the program—a conceptual momentum that directs research away from any further investigation of those central assumptions. Simply put, no one ever questions them anymore. This conclusion has been overblown by some, but its central reality is evident in this report from Gould.¹⁵

Explaining that the hardening he describes “cannot be explained as simply and empirically driven” (p. 542), Gould says further:

The culture of science trains us to believe that such major shifts of emphasis record improvements in knowledge won by empirical research and discovery. I do not deny that observation did play a significant role, at least in illustrating, with some elegant examples, the power of adaptation. . . . But empirical discovery cannot supply the entire (or even, I think, the major) reason for adaptationist hardening, for each favorable case can be matched by a failure (often hedged or unacknowledged), and no adequate assessment of an overall relative frequency has ever been achieved—to this day. Thus, any judgment, in either direction, must represent the fashionable imposition of a few well-documented cases upon an unstudied plethora. (pp. 541–42)

In an interesting demonstration of shared psychology—and lost memory—in an intellectual movement, Gould shares at length (pp. 520–41) how Dobzhansky, Mayr, and Simpson, all of whom were major leaders in the hardening movement, changed their minds over time in the

15. See Imre Lakatos, *The Methodology of Scientific Research Programmes: Philosophical Papers, Volume 1* (Cambridge: Cambridge University Press, 1980).

direction of adaptationism and away from their earlier openness to other possible mechanisms of evolutionary change, and yet later claimed that they had not changed their minds at all—even though this was clearly belied by what they had written in their earlier years. Gould adduces this as an interesting feature of the general intellectual hardening: these prominent scientists had obviously changed their minds but could not remember doing so.

Although Gould considers the possibility that “some complex mixture of empirical and sociological themes may explain the adaptationist hardening of the synthesis” (p. 543), he draws particular attention to one dimension:

The community of evolutionary biologists is sufficiently small, and sufficiently stratified—a few lead and many follow, as in most human activities—that we need not necessarily invoke some deep and general scientific or societal trend to explain a change in opinion by a substantial community of evolutionists in different nations. A reassessment by a few key people, bound in close contact and mutual influence, might trigger a general response. (p. 543)

Gould believes this occurred with the three leading exponents of hardening in America—Dobzhansky, Simpson, and Mayr. They worked together as colleagues in what Gould calls a “New York Mafia” centered at Columbia University and the American Museum of Natural History. He says:

The validation of selection as a nearly exclusive mechanism of evolutionary change, as embodied in the adaptationist program[,] received strong verbal approbation, and elegant illustration in a few cases, but won orthodox status largely as a bandwagon effect prompted by the urgings of a few central figures, notably Mayr and Dobzhansky, and the subsequent acquiescence of most professionals to the assertion of such leading figures, and not to the data of convincing demonstrations. (p. 586)

In other words, a few top scientists dramatically influenced what other scientists believed, not because of convincing research data, but because these leaders were suitably situated and sufficiently elite that others acquiesced to their pronouncements. As Gould observes, “a few lead, and many follow” (p. 543). Thus can the specter of orthodoxy arise even in scientific disciplines.¹⁶

Paleontology’s intimidation. The role of authority and the bandwagon effect influenced not only the general hardening of evolutionary theory; according to Gould, it also dramatically influenced paleontologists’ behavior for decades. We have already seen in Gould’s account that paleontologists long believed that stasis in the geological record amounted to nonevidence of what they were searching for: what they took to be the Darwinian expectation of gradual evolutionary change. Intimately acquainted with the primary evidence as they were, however, paleontologists could not be entirely content to consider the overwhelming observation of sudden appearance of species and their subsequent stasis as failure to find anything worthwhile and to constantly attribute such failure to the regrettable imperfection of the geological record. They were at least partly nagged by the suspicion that their primary data—the actual fossil record—amounted to evidence of *something*. So why didn’t they say so? Gould tells us:

Paleontologists have always recognized the long-term stability of most species, but we had become more than a bit ashamed by this strong and literal signal, for the dominant theory of our scientific culture told us to look for the opposite result of gradualism as the primary empirical expression of every biologist’s favorite subject—evolution itself. (p. 749)

16. Adding a further element to the calculus of authority and bandwagon effects, Gould points out that “founders tend to be brilliant and subtle, and to keep all major difficulties constantly in mind, while epigones generally promulgate the faith and disregard, or never learn, the problems, exceptions, and nuances” (p. 543). In other words, the bandwagon effect would be smaller or at least of shorter duration if acolytes were as smart as the masters. One must wonder about this, however, since Gould has already demonstrated that the major personalities behind the hardening of the modern synthesis became hardened without even realizing it. Where was this special capacity for maintaining awareness of problems, exceptions, and nuances among *them*?

And:

Darwinian evolution became the great intellectual novelty of the later 19th century, and paleontology held the archives of life's history. Darwin proclaimed insensibly gradual transition as the canonical expectation for evolution's expression in the fossil record. . . . Thus, paleontologists could be good Darwinians and still acknowledge the primary fact of their profession—but only at the price of sheepishness or embarrassment. No one can take great comfort when the primary observation of their discipline becomes an artifact of limited evidence rather than an expression of nature's ways. Thus, once gradualism emerged as the expected pattern for documenting evolution—with an evident implication that the fossil record's dominant signal of stasis and abrupt replacement can only be a sign of evidentiary poverty—paleontologists became cowed or puzzled, and even less likely to showcase their primary datum. (p. 750)

Ashamed, sheepish, embarrassed, cowed. Whatever else they may be, these are clearly not the adjectives of one who finds science, in practice, routinely encouraging “alternative hypotheses.”

The Inevitability of Textbook Authority

One more element of the role of authority in scientific practice is worth mentioning. It is best illustrated by Thomas Kuhn, the immensely influential historian of science. In his course at MIT on the nature of scientific knowledge, Kuhn routinely posed the following question to students early in the term: “What reasons do you have for believing in the Copernican view of the solar system rather than the Ptolemaic? All of you accept the Copernican system, of course. The question is, Why?”

The question, of course, seems too easy because everyone knows that the Copernican view of planetary motion—including its central heliocentric postulate—is true and that the ancient geocentric view is false. That's grade school stuff. One would therefore expect these future scientists, certainly among the best math and science students

in the country, to dispatch the question with ease. But they didn't. Against every item of evidence they advanced to prove the Copernican system correct, Kuhn would demonstrate that such evidence was not compelling, let alone decisive, and that it did not in fact justify acceptance of the Copernican system over the Ptolemaic. He would demonstrate this repeatedly until the class finally fell silent. Kuhn would then explain that the proper evidence for the Copernican system does exist but that its technicality renders it unfamiliar to most, including those studying science.¹⁷

Kuhn conducted this exercise because he wanted to demonstrate how fully even scholars rely on the authority of other scholars and textbooks in accepting and believing what they do. Even the best, he was eager to show, hold comparatively few beliefs rooted in direct familiarity with the scientific evidence, and the evidentiary gaps that remain are filled by largely unexamined assumptions based on the authority of others.

Of course, a little reflection reveals how unavoidable this reality is; with all there is to know it could not be otherwise. But it is still instructive to be explicit about this point. After all, for generations nonpaleontologists accepted a false version of the geological record—that fossil discoveries generally displayed gradual organismic change over geological time—because that was the version presented in textbooks. Unfortunately, as we have been informed by Gould, the classic examples cited in the textbooks have been roundly discredited. Similarly, for 150 years scientists have believed Lyell's demonstrably false anathematization of Cuvier and of catastrophism in general, rendering the vast majority of paleontologists thoroughly resistant to strong evidence even of local catastrophic events in the world's history, all because Lyell's authoritative mischaracterization had been preserved and replicated in generations of science textbooks.¹⁸

17. As a member of one such class I observed this discussion firsthand. I assume the give-and-take experience was similar in both previous and subsequent classes.

18. See pages 576–84 for further examples of what Gould considers to be simplistic and misleading, and even distressing, treatments of evolutionary themes in introductory textbooks.

Again, scholars have no choice but to learn much of what they know from the authority of textbooks. No one can be a specialist in very many things, much less in everything. But it is worth keeping in mind that this is the reality. It is also worth keeping in mind that textbook versions have been known not only to present mere caricatures of the state of knowledge in a particular field, but also, according to Gould, to be demonstrably false.¹⁹

Of Science and Surprise

From Gould's detailed accounts of scientific episodes and movements, we have seen up close some of the complexities of scientific investigation—examples of (unintentional) omissions and distortions in scientific publications, the role of scientific authority in unfairly stigmatizing individuals and theories over generations, and the role of bandwagon effects and textbook orthodoxy in influencing scientific opinion.

This might seem surprising. If science is marked by its empiricism—by its dedication to observing, predicting, and testing, as well as to theorizing—shouldn't it be able to detect any dead-end avenues of investigation rather quickly and get back on track? Isn't there something called a "crucial experiment" or a "crucial test"?

Unfortunately, the matter is seldom so simple. It's true that science is eventually self-correcting, but there is a lot of meaning packed into the word *eventually*, and the process is far from smooth or quick. Theories are complicated networks of interrelated observations,

19. Regarding the authority of textbooks, Codell Carter of the Brigham Young University philosophy department tells the story of a professor who taught anatomy for twenty years at the University of Adelaide (Australia). Although using an English textbook that gave instructions on dissecting an English frog, the students actually dissected a similar but anatomically distinct Australian variety. In all those years of teaching, however, only a dozen students ever noted the differences between the frogs they were dissecting and the frog that appeared in the textbook. What's most fascinating, however, is that even in those rare cases the students invariably concluded that the problem was not with the textbook, *but with the frog*. See Lester S. King, *Medical Thinking: A Historical Preface* (Princeton, NJ: Princeton University Press, 1984). But as Carter asks, How could the frog be wrong?

hypotheses, assumptions, meanings, inferences, implications, and presuppositions. Testing any part of such a complex network yields multiple possibilities for interpretation. For example, although from the beginning Newtonian theory was highly successful in accounting for planetary motion, it was eventually discovered in the nineteenth century that it actually *failed* to account for the orbit of the planet Uranus. But in spite of this negative evidence, scientists didn't reject the Newtonian explanation of planetary motion. Instead, they considered this predictive failure a research problem, a puzzle to be explained, not a failure of the Newtonian explanation itself. In fact, they accounted for the anomaly *within* Newtonian mechanics by postulating the existence of another planet of the necessary mass and orbit to account for the observed motion of Uranus. When the planet Neptune was discovered, in direct corroboration of this hypothesis, the wisdom of such an approach was dramatically confirmed and provided further evidence for the power of Newtonian theory.

The story does not end there, for there was a similar difficulty with the orbit of Mercury. On the heels of the success with Uranus, it was naturally thought that the anomaly of Mercury could be explained in the same way. Thus, the existence of yet another planet was hypothesized and given the name Vulcan. No such planet was ever discovered, however, and Mercury's orbit was never explained within Newtonian theory—and yet this was never considered a falsifying event for the Newtonian account of planetary motion, but only an anomaly that would eventually be explained *by* the Newtonian account.²⁰

20. An accurate prediction of Mercury's orbit awaited the appearance of Einstein's general relativity theory, and only then was that orbit considered a direct counterinstance to the Newtonian account. Classic discussions of this incident and its meaning for scientific practice are found in Hilary Putnam, "The 'Corroboration' of Theories," in *Mathematics, Matter and Method: Philosophical Papers*, 2nd ed. (Cambridge: Cambridge University Press, 1979), 1:250–69; Stephen Toulmin and J. Goodfield, *The Fabric of the Heavens* (New York: Harper and Row, 1961); Carl G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, NJ: Prentice-Hall, 1966). See also Harold I. Brown, *Perception, Theory and Commitment: The New Philosophy of Science* (Chicago: University of Chicago Press, 1979). Incidents like this convinced Lakatos, among others, that no theory is ever abandoned except in exchange for another, which makes the formulation of alternative explanations as important as negative observations in the demise of existing theories. See, for example, I. Lakatos, "Falsification and the Methodology of Scientific Programmes,"

This scientific reality—resistance to falsification even by disconfirming evidence—is inevitable. This is because any individual scientific hypothesis is inherently part of a larger whole, surrounded by a complex network of related theoretical statements and assumptions (A), all of which, though implicit, are always tested along with any particular hypothesis (H). Thus, as introductions to the philosophy of science typically point out, any test situation assumes something like this form:

If H and A are all true, then so is O (an observation predicted by H).

(As our tests show) O is not true.

Therefore, H and A cannot all be true.

To recur to the example of Uranus and Mercury, we might put the matter like this:

If the Newtonian explanation of planetary motion and all the related theoretical statements and assumptions that surround it are all true, then Uranus will exhibit orbital pattern x and Mercury will exhibit orbital pattern y .

(As our tests show) Uranus and Mercury do not exhibit orbital patterns x and y , respectively.

Therefore, the Newtonian explanation of planetary motion and the related theoretical statements and assumptions that surround it cannot all be true.

By the logical principle of *modus tollens*, this is a valid conclusion, but it does not tell us which of the statements of the theory must be rejected—the hypothesis about planetary motion itself or one or more of the myriad assumptions and theoretical statements that are conceptually related to it. We are free to look anywhere in the system for the feature that requires modification by this experimental result.

in *Criticism and the Growth of Knowledge*, ed. I. Lakatos and A. Musgrave (Cambridge: Cambridge University Press, 1970), 91–196. Lightman and Gingerich develop a similar theme and apply it to five episodes in the history of science. See Alan Lightman and Owen Gingerich, “When Do Anomalies Begin?” *Science* 255 (7 February 1992): 690–95.

In the cases of Uranus and Mercury, for example, scientists simply rejected an implicit assumption they had made—the assumption that, just because they had not yet been observed, there were no planets nearby that were affecting the orbits of Uranus and Mercury in the required way.

Holism. The view that no statement can ever be tested in isolation because of its embeddedness in a larger theoretical structure is called “holism.” First advanced by Pierre Duhem (1861–1916), its most forceful recent proponent (in his own version) has been W. V. Quine. Often called the Duhem-Quine thesis, it is the basis for Quine’s argument that at least in principle even the laws of mathematics are revisable in the wake of experimental results that call for a modification of theory. According to Quine, the reason the laws of mathematics are not modified when modification is called for—and other elements of the theory are revised instead—is that we want to make whatever changes will cause the least amount of disruption to the theoretical system as a whole, other things equal. We follow, Quine says picturesquely, the “maxim of minimum mutilation.” The idea is to revise the theory in the smallest way possible and to keep the theory as simple as possible, but all in the service of maximizing the theory’s future predictive success. What is not required is that any *particular* part of the theory (including the specific hypothesis under examination) be retracted in the face of experimental failure.²¹

Thus hopes about falsification and crucial tests to the contrary, it is rarely a simple matter to reject a theoretical statement, even in the

21. See generally W. V. Quine, *The Pursuit of Truth* (Cambridge, MA: Harvard University Press, 1990). Here Quine has moderated the extreme holism of his early writings (a holism that has been exploited by some in the interest of postmodern analyses). An early critic of the early view was Adolf Grunbaum. See, for example, his “The Falsifiability of Theories: Total or Partial? A Contemporary Evaluation of the Duhem-Quine Thesis,” *Synthese* 14 (1962): 17–34. Larry Laudan reviews, less technically, much of Grunbaum’s argument against any strong version of holism; see his *Science and Relativism* (Chicago: University of Chicago Press, 1990). One attempt to show that we can, despite the holistic thesis, determine which part of a theory to amend in view of experimental failure is Y. Balashov, “Duhem, Quine, and the Multiplicity of Scientific Tests,” *Philosophy of Science* 61/4 (1994): 608–28. A more recent argument for the nonnecessity of mathematical truths is Robert Nozick’s. See his *Invariances: The Structure of the Objective World* (Cambridge, MA: Harvard University Press, 2001).

face of evidence unanimously regarded as negative. It can be difficult to know exactly when to consider a negative result a mere puzzle—something to investigate or wonder about—and when to consider it an actual failure of the statement or hypothesis in question. Because these are matters of judgment, and not merely of evidence, scientists can be expected to, and do, disagree about the most promising explanation and about the most fruitful direction for further study.

Given this reality, it is hard to fault any theorist or set of theorists who, in hindsight, seem to have overlooked data that should have modified their views. The meaning of such data for the theoretical statement in question is generally ambiguous rather than obvious, and this must be kept in mind.

Worldviews. As a final point, though, it is worth remarking that sometimes it's not just that the meaning of the data is ambiguous; the problem is that some possible meanings cannot be seen at all. Gould speaks, for example, of the "broad worldviews" that scientists develop about their subjects. Such worldviews are not just "passive summaries," he tells us, of what is generally accepted; they also "serve as active definers of permissible subjects for study, and modes for their examination." Although such intellectual contexts can foster and guide fruitful work, Gould says, the downside is that "ever so often in the history of science, such worldviews direct and constrain research by actively defining out of existence, or simply placing outside the realm of conceptualization, a large set of interesting subjects and approaches." This is serious enough, but it gets even worse. As Gould explains, the subjects that are thus excluded from consideration often include "the very classes of data best suited to act as potential refutations" of the very worldview that has excluded them. "Such self-referential affirmations," Gould adds, "are not promoted cynically, or (for the most part) even consciously, but they do, nonetheless, operate as strong impediments to scientific change" (p. 1309).

Worldviews, though inescapable, are therefore double-edged swords. On one hand, they integrate a wide range of observations and beliefs and guide productive research in the directions suggested by such. But on the other hand, worldviews simultaneously preclude

research in other directions because, from within a given worldview, there are always possibilities that cannot even be *seen*, much less appreciated and investigated. And this includes the very classes of data that have the best chance of *disconfirming* the worldview itself. In this way, without conscious calculation, worldviews can tend to be insular and, by conceiving a world in which much possible counterevidence cannot even be conceived, self-confirming. Such worldviews are like spectacles that not only impose a certain view of the world on us but also simultaneously prevent us from seeing the degree to which what we see is determined by the spectacles themselves. As Gould says (using the term *theory* synonymously with *worldview*): “A theory often compels us to see the world in its light and support. Yet we think we see objectively and therefore interpret each new datum as an independent confirmation of our theory.” As a result, “although our theory may be wrong, we cannot confute it” (p. 761). Thus, to the degree that we hold scientific worldviews of one sort or another (and I think in mortality it is impossible not to), we are their intellectual captives as well as their intellectual beneficiaries. The combination is inescapable.

Surprise

This brief treatment is far from an exhaustive list of the complexities—indeed, the nagging discontents—that inhere in scientific investigation, but they suffice to draw at least this modest conclusion: Any simple assurance about the nature of scientific attitudes and practice—for example, the generalization that alternative hypotheses are encouraged as a matter of course in scientific inquiry and that reasonable hypotheses are not suppressed—is limited to a description of the *ideal* in scientific investigation. In matters of actual practice, such a description significantly underdescribes the reality. If actual scientific practice were as tidy as the ideal, Gould would find little use for many of the expressions he employs, such as “sheepish,” “embarrassed,” “cowed,” “risible apostasy,” “catechism,” “shared cultural assumptions,” “dogma,” “knee-jerk fear,” “bias,” “tricky rhetorical argument,” “canonical,” “intellectual hardening,” “triumph of hope and expectation over evidence,” “fantasy,” “unconscious conspiracy,” “absurd vicious circle,” “a few lead and many follow,” “insidious,”

“warped,” “derision,” “anathematization,” “textbook pap,” the “power of false characterization,” “arms of misreason,” “prejudice,” “disdain,” “rhetorical dismissal of dissenting views,” “restrictive faith,” “straight-jacket,” “object of ridicule without being read,” “bandwagon,” “orthodoxy,” “stigma,” and “acquiescence.” He also would not talk of “difficulties, puzzles, anomalies, unresolved corners, and bits of illogic” that are “rarely disputed and largely forgotten,” or of investigative failures that are “often hedged or unacknowledged.” He would not speak of epigones who “generally promulgate the faith and disregard, or never learn, the problems, exceptions, and nuances” of a scientific theory. Nor would he be able to identify presuppositions that have imposed a “tight and efficient lid” on alternative explanations for scientific data, or talk of “assumptions accepted without fundamental questioning.” He would not speak of worldviews that “operate as strong impediments to scientific change,” and he would not identify “profound” and “negative” influences that (note it well) “limit and stifle hypotheses.”²²

If we do not appreciate how such descriptions can apply to one level or another of scientific practice—descriptions that depart significantly from the prescriptive ideal of scientific inquiry—we appreciate too little. The risk of such naïveté is that it can lead to carelessness in our thinking and, especially in fields where we are not expert, to a credulous acceptance of whatever intellectual pronouncements we happen to encounter. This in turn can lead to a degree of certainty that is not remotely matched by our understanding or by our familiarity with the relevant evidence, both pro and con.

22. Because of his controversial status, some may wish to see Gould as simply having an ax to grind, and thus they will be inclined to dismiss his historical examples and characterizations out of hand. I think that is unfair on two counts. First, if Gould simply has an ax to grind, that should be easy enough to show. Thus, someone who wants to make this claim should actually demonstrate it, point by point, rather than simply assert it. That would require saying as much on these topics as Gould himself says, but such a discussion would certainly be welcome. But second, a demonstration of this sort would be largely irrelevant to this paper in any case. My concern is not with evolutionary theory per se, but with scientific investigation generally. The points Gould makes could be illustrated from any number of fields. I use Gould simply because he is handy, for the reasons I mentioned earlier.

That scientific study is complex and sometimes messy is not a new observation, of course. Specialists in the philosophy of science have long identified subtle complicating dimensions of scientific study (both human factors and others, like holism, that are more strictly academic in nature), and to one degree or another such dimensions play a role in scientific investigation whatever the field. Gould's area of study is just one example.²³ The role such factors play will vary widely among scientific disciplines, of course, depending partly on a field's maturity, the degree of difficulty it faces in obtaining precise data, the nature and extent of the research funding it enjoys, and so forth; but these complexities play at least some role in every field. My own discipline is psychology. I studied closely with Larry Jensen, Lynn Scoresby, and Terry Warner (from philosophy) at Brigham Young University, and with Lawrence Kohlberg at Harvard. Though I have not done so myself, based on my intellectual experience in this discipline, I have no doubt that someone suitably inclined could identify in detail how various human and other complicating elements have played out in psychology and its various schools of thought over the last hundred years. Think again of the recent biographers of Joseph Smith (see p. 164): insufficiently cautious in their examination of the Prophet, they placed their confidence in a psychological theory that, it was discovered long ago, suffers under careful scrutiny. Had they been more alive to the discontents of scholarly investigation in general, these authors might have been less prone to wield this psychological tool in such an unquestioning and ultimately fruitless manner.

Humility and Wisdom

I think it is impossible to know all of the assumptions and subtle influences that affect our intellectual conclusions at any one time over the wide range of disciplines that interest us. No doubt this includes

23. The journal *Philosophy of Science* is devoted to these topics. A good historical overview of the philosophy of science, from Aristotle to the twentieth century, is found in John Losee, *A Historical Introduction to the Philosophy of Science* (Oxford: Oxford University Press, 1980). The most thorough treatment of modern philosophy of science, up to postmodern developments, is found in Suppe, *The Structure of Scientific Theories* (see note 6). As a general introduction to some of the central classical issues, I recommend Laudan, *Science and Relativism* (see note 21).

various expressions of publication bias that might be influencing not only what we believe, but even what we deem worthy of interest or study in any number of fields. Further, we probably cannot know the extent to which we find certain intellectual viewpoints repugnant primarily because of professional stigma rather than because of our acquaintance with actual disconfirming data. Nor can we probably know the opposite—the degree to which other intellectual viewpoints hold us in their thrall simply because they are accepted by people we admire and not, again, because of our acquaintance with any actual evidence.

I think it is also impossible to know, based on the authority of textbooks, exactly what we should accept with full certainty and what we should doubt. And finally, in disciplines where we are not truly expert I think it is impossible to be aware of all the negative evidence faced by various statements within a theory, much less to know how to regard such evidence—whether as disconfirming or as merely puzzling. Moreover, it is impossible to know the degree to which we are cognitively captive to any number of worldviews and to trace all of the limitations and errors, large or small, that are entailed by this unavoidable, but constricting, reality of intellectual life.

To one degree or another, these are all inevitable realities of intellectual inquiry. They are unavoidable. But recognizing and explicitly acknowledging such tensions and discontents is preferable to ignoring them. By ignoring them we are apt, in our naïveté, to ascribe more certainty than is warranted at any given moment to a particular discipline's range of intellectual conclusions (as happened with psychoanalytic theory, for example) and to risk developing an attitude of dogmatism and defensiveness as a result.

Recognizing such factors, on the other hand, we can be saved from such dogmatism and instead attain something approaching wisdom: a lingering tentativeness and humility about many of the beliefs we hold at any one time. In other words, we can be sure we are mistaken in one way or another even if we cannot be sure exactly where. I believe such an attitude would have helped these biographers of Joseph Smith, to mention only two, and it seems the prudent course for us all. It's probably best to live with the expectation that despite our best efforts we

will turn out, to some extent at least, to be mistaken on many matters: we should live in anticipation of surprise.

Of Scripture

I have tried to show why I wish Stephens and Meldrum had said more about scientific attitudes and practice than just identifying the ideal. I now want to treat two scriptural matters—one that they discuss and one that I *wish* they had discussed because it is so central. Finally, I will illustrate why I think surprise is inevitable on scriptural topics, just as it is on strictly intellectual matters. On the things of eternity, I believe we should embrace our ignorance, enthusiastically and with wonder.

2 Nephi 2:22

In the course of their book, Stephens and Meldrum address scriptural and other authoritative statements from the Brethren that relate to the current state of scientific thinking on evolution. They do this at some length, which is admirable in light of their intended audience. Much could be said about their discussion on these topics, but I will restrict my attention to what I think is their most important argument.

The authors believe that Adam and Eve were not inherently immortal in the Garden of Eden, but instead sustained a condition of immortality by eating of the fruit of the tree of life. Following their transgression, they were no longer permitted to eat of that tree, so mortality reentered the world for them. At the same time the rest of creation was living, dying, and evolving just as it had “for millennia” (p. 135).²⁴

According to scripture and official Latter-day Saint doctrine, Adam and Eve represented the first progenitors, or parents, of the human race. According to scientific evidence, human ancestors descended through the hominid line by natural selection. Once having achieved their “human” stature

24. From this point forward, page numbers refer to Stephens and Meldrum’s *Evolution and Mormonism*.

through evolution, Adam and Eve could have been placed into the Garden of Eden where they ate of the fruit of the Tree of Life and were rendered immortal for as long as they partook of its fruits. They were told not to eat of the Tree of Knowledge of Good and Evil. In fulfillment of God's plan that they exercise their agency, they yielded to temptation and ate of the forbidden fruit. They were consequently exiled from the garden, separated from the Tree of Life, and thus reverted to their mortal state. (p. 185)

This, of course, contradicts the more common view that prior to the fall the earth enjoyed a paradisiacal state: living things, and at a minimum Adam and Eve, were *inherently* immortal and basked in the spiritual presence of God. According to Stephens and Meldrum, on the other hand, nothing was inherently immortal; life and death occurred through the immensity of time until evolutionary mechanisms (including certain constraints on developmental possibilities) produced bodies suitable for the two spirits, Adam and Eve; upon entering those bodies, Adam and Eve then became immortal through eating of the fruit of the tree of life. Then came the fall, the banishment of Adam and Eve from further eating of the tree of life, and a return to their inherently mortal condition, including the inevitability of physical death for themselves and their posterity.

Latter-day Saints are familiar with the scriptural passage that has always appeared to stand in the way of this view and that must be accounted for if the view is to be compelling:

And now, behold, if Adam had not transgressed he would not have fallen, but he would have remained in the garden of Eden. And all things which were created must have remained in the same state in which they were after they were created; and they must have remained forever, and had no end. (2 Nephi 2:22)

Pointing out that this is the only passage that supports the notion that "Adam and Eve, as well as all the animals, for that matter, were *inherently* immortal and incapable of reproduction" (p. 134), Stephens and Meldrum say (please note their ellipsis): "The central part of this

scripture is the phrase ‘all things . . . must have remained in the same state in which they were after they were created; and they must have remained forever, and had no end’” (pp. 134–35). “What does the term ‘all things’ refer to?” they ask. “Can we be certain that ‘all things’ in verse 22 means Adam, Eve, all the animals, and all the plants?” After a few observations about the passage, they suggest that the phrase *all things* actually refers to “conditions.” Thus in their view it seems that the passage might be read as follows: “All things *regarding the condition of Adam and Eve* would have continued. They would have continued to be immortal because they would have been able to continue eating of the tree of life.” Read this way, the verse presents no problem to the authors’ view of evolution and the fall because only Adam and Eve were changed; everything else continued as it always had in an inherently mortal condition: living, dying, and evolving through the mechanisms of natural selection.

But there is a difficulty with the authors’ analysis of this passage. Asking what the phrase *all things* refers to is a reasonable question, and their reading is a plausible reading, but only because of the ellipsis *they* insert into the text. The verse actually says: “And all things *which were created* must have remained in the same state in which they were after they were created; and they must have remained forever, and had no end.” If the verse is read in its entirety, we are not faced with a puzzle about what the phrase *all things* might mean. The verse itself tells us that it means all things “which were created.” We naturally think of the tangible substances (e.g., “the greater light to rule the day, and the lesser light to rule the night,” Moses 2:16) and the living things (e.g., “cattle, and creeping things, and beasts of the earth,” Moses 2:24) described in the creation accounts, and plausibly suppose that this would be the most natural reading of the term in 2 Nephi 2:22 as well; it would not normally occur to us that it might refer to something more abstract like “conditions.” So the authors help us out by omitting that part of the verse; *now* we have a question. But notice what has happened here: The way the authors elicit a question in our minds, thereby producing an opening for an alternative inter-

pretation of this verse, is to omit the part of the verse that provides the very interpretation that they then say is missing.

I don't want to be too quick about this matter, however. After all, it is logically possible that the phrase *all things* actually does refer to something like "conditions," and someone could make an argument to demonstrate this. In that case, for example, the verse would be read something like this: "And all conditions which were created must have remained in the same state in which they were after they were created; and those conditions must have remained forever, and had no end." I think the argument would be somewhat complicated and probably implausible in the end, but it might be worth a try.²⁵ I want only to point out that Stephens and Meldrum themselves do not make such an attempt. They avoid it by omitting the very phrase that would require this more complex argument.

Miracles and the Creation

Stephens and Meldrum view evolutionary mechanisms as the method by which God has brought about the various forms of life on earth, including man, over the immensity of geological time. They discuss many scriptural passages in order to flesh out their view, and other Latter-day Saint scientists naturally do the same. But I have never seen anyone (at least that I can remember) who addressed the following two considerations: First, the prophet Moroni, in Mormon 9, explicitly places the creation in the context of God's miraculous power. He begins in verse 11 by emphasizing that "behold, I will show unto you a God of miracles, even the God of Abraham, and the God of Isaac, and the God of Jacob; and it is that same God who created the heavens and the earth, and all things that in them are." Then, after querying in verse 16, "Behold, are not the things that God hath wrought marvelous in our eyes? Yea, and who can comprehend the marvelous

25. For example, because the term *condition* is synonymous with the term *state*, the verse would say that "all conditions which were created must have remained in the same condition in which they were after they were created." What argument could be given that would make this seem a natural expression for Lehi to utter rather than an odd one (an expression in which the notion of "condition" is used as both a first-order concept and a second-order metaconcept)? There might be such a sensible and plausible argument; I would just like to see it.

works of God?” Moroni asks in verse 17, “Who shall say that it was not a miracle that by his word the heaven and the earth should be; and by the power of his word man was created of the dust of the earth; and by the power of his word have miracles been wrought?”

Running them together as he does, there seems to be no distinction in Moroni’s mind between the marvelous and miraculous nature of God’s works and the nature of his works *as Creator*.

In this, Moroni is similar to his predecessor Jacob, who says:

Behold, great and marvelous are the works of the Lord. How unsearchable are the depths of the mysteries of him; and it is impossible that man should find out all his ways. And no man knoweth of his ways save it be revealed unto him; wherefore, brethren, despise not the revelations of God. For behold, by the power of his word man came upon the face of the earth, which earth was created by the power of his word. Wherefore, if God being able to speak and the world was, and to speak and man was created, O then, why not able to command the earth, or the workmanship of his hands upon the face of it, according to his will and pleasure? (Jacob 4:8–9)

How, then, might God’s miraculous power as described by Moroni and Jacob relate to evolutionary theory? After all, evolutionary theory explicitly eschews anything that even hints at the miraculous, while these prophets see hardly anything *but* the miraculous in their view of the creation.

One natural argument is that evolutionary mechanisms themselves are the miraculous manifestation of the power of God—initiated, presumably, “by the power of his word”; coming to understand those mechanisms, therefore, is simply coming to understand the manner by which he works. Moreover, to satisfy Jacob’s assertion about the necessity of revelation to understand God’s ways, one could say that guiding scientific discovery is the method God *uses* to reveal his ways.

I understand the reason for this kind of argument, but I think it less plausible than it appears at first glance. To see this—and this

is the second consideration—think of the following examples of how the Lord has worked with physical elements, both during and following his earthly ministry. Listing this many examples might seem to belabor the point, but it is important to get the full impact of various events in which the Lord interacts with the physical world.

He turns water into wine (John 2:1–10).

He walks on water (Matthew 14:23–33; Mark 6:46–51).

He feeds more than five thousand people with five loaves and two fishes (Matthew 14:13–21).

He feeds more than four thousand people with seven loaves “and a few little fishes” (Matthew 15:32–38).

He vanishes from a hostile multitude (Luke 4:28–30; John 8:59).

He stills a storm by verbally rebuking it (Matthew 8:23–27; Mark 4:36–41).

He fills his disciples’ nets with fish (Luke 5:4–9; John 21:3–6).

He heals the sick at will (these healings are too numerous to list exhaustively, but consider Matthew 14:35–36; 15:30–31; Mark 5:22–23, 35–43; 5:25–34; Luke 8:22–56; John 9:1–15; 11:1–44; 3 Nephi 17:6–9).

He arranges for Peter to find “a piece of money” in a fish’s mouth in order to pay tribute (Matthew 17:24–27).

He suddenly appears to his apostles though the doors to the room were shut (John 20:24–28).

He ascends through the air after teaching his disciples (Acts 1:9–11).

He descends gradually through the air as he appears to the Nephites (3 Nephi 11:1–8).

He miraculously provides sacramental bread and wine for the Nephite multitude (3 Nephi 18:3–7).

He performs a transformation in the Three Nephites that permits them to interact in a constantly miraculous way with the physical world (3 Nephi 28:19–22; 4 Nephi 1:30–33).

He and the Father descend in a “pillar of light” and appear to the boy Joseph Smith at the time of his first vision, and they both then hover in the air to address him (Joseph Smith—History 1:16–17).

Moroni, a resurrected being from ages past, passes through material boundaries and hovers in the air while talking to Joseph (Joseph Smith—History 1:30, 43).

Consider also how the Lord worked with various physical elements in the millennia prior to his earthly ministry:

He performed various miracles before Pharaoh through Moses (Exodus 4; 7–12).

He parted the Red Sea to permit passage by the children of Israel (Exodus 14).

He provided manna for the children of Israel for forty years (Exodus 16:35; Joshua 5:12).

He ensured that the clothing worn by the children of Israel did not wear out during their forty-year sojourn in the wilderness (Deuteronomy 8:4; 29:5; Nehemiah 9:21).

He blessed the widow's meal and oil through Elijah (1 Kings 17:10–16).

He raised the widow's son from death through Elijah (1 Kings 17:17–23).

He consumed the offerings of the priests of Baal through heavenly fire (1 Kings 18).

He parted the waters of Jordan through Elijah (2 Kings 2:8).

He parted the waters of Jordan through Elisha (2 Kings 2:14).

He healed the waters of Jericho through Elisha (2 Kings 2:19–22).

He multiplied the widow's oil through Elisha (2 Kings 4:1–7).

He raised a boy from the dead through Elisha (2 Kings 4:12–37).

He moved Mount Zerin through the brother of Jared (Ether 12:30).

He caused mountains to flee and rivers to move through Enoch (Moses 7:13).

He raised a whole city to heaven to spare the people the coming destruction (Moses 7:21).

And note how Nephi, the son of Helaman, describes God's miraculous power:

For behold, the dust of the earth moveth higher and thither, to the dividing asunder, at the command of our great and everlasting God. Yea, behold at his voice do the hills and the moun-

tains tremble and quake. And by the power of his voice they are broken up, and become smooth, yea, even like unto a valley. Yea, by the power of his voice doth the whole earth shake; yea, by the power of his voice, do the foundations rock, even to the very center. Yea, and if he say unto the earth—Move—it is moved. Yea, if he say unto the earth—Thou shalt go back, that it lengthen out the day for many hours—it is done; and thus, according to his word the earth goeth back, and it appeareth unto man that the sun standeth still; yea, and behold, this is so; for surely it is the earth that moveth and not the sun. And behold, also, if he say unto the waters of the great deep—Be thou dried up—it is done. Behold, if he say unto this mountain—Be thou raised up, and come over and fall upon that city, that it be buried up—behold it is done. (Helaman 12:8–17)

All of the above are just examples, of course, but they are sufficient to suggest that (1) when the Lord wants to accomplish something with physical elements, he is not hesitant to employ processes that are thoroughly out of the norm and that are inscrutable to us; and (2) he is not hesitant to work out of the norm with physical elements when he wants to make things happen more quickly than they otherwise would. Consider, for example, what motivation the Lord might have had for turning water into wine. Since at a minimum doing so saved time, that is likely to have been one of his reasons. And consider the multitude of the Lord's healings. We are all familiar with the dimension of time in the healing process, and even in cases where miraculous assistance is involved, but in hundreds of cases the Lord eliminated all such time-related features by restoring health *instantly*. And think of his multiplying fishes and loaves (twice) to feed hungry multitudes. And of parting the Red Sea. And of dividing the waters of Jordan (twice). And of moving mountains and rivers. And of filling his disciples' nets with fish. And of traveling—whether at greater than light speed or by some other unknown means—from the heavenly realm to visit the earth. Whatever other motivations he might have had in any of these cases, at a minimum time was affected by the

miracles performed and thus seems likely to have been at least one of his motivations for performing them.

I think these considerations about God's miraculous interventions in the world are worth keeping in mind when pondering his methods of creation and everything else he reveals in the scriptures. I think it is natural to wonder, for example, why the Lord would be in such a hurry to provide wine at a wedding—in so much of a hurry that he would use a miracle, completely mysterious to us, to accomplish it—while he is in no hurry at all to create the earth and all of its life-forms, including man, and thus simply puts in place a complex and immensely time-consuming mechanism that (even if it includes constraints) is guaranteed to require billions of years before finally producing a set of physical organisms suitable for housing the spirits of Adam and Eve. And even if we can provide a plausible answer to this question, we are still left wondering whether anything that takes so long could reasonably be considered a *miracle*.

So I don't see how coming to conceive of man's creation in completely naturalistic terms—which is the explicit program, praised by Stephens and Meldrum, of Darwin and his theoretical descendants (including Gould, Dawkins, and countless others)—can qualify as coming to understand the miraculous ways of God when (1) the explanation specifically depends on the immensity of geological time to bring the whole process about and (2) the attempt at a naturalistic explanation excludes, *by definition*, any reference to miraculous intervention. If we are going to go in this direction, we might as well embrace the inevitable and simply deny, along with these mainstream and prominent figures, that the process was miraculous at all. It may have been God's chosen method, but it was one of the times (lasting billions of years, please note) that he chose not to intervene in any way that would seem to us significantly out of the ordinary.

But then we're back to Moroni and Jacob, both of whom thought the process quite out of the ordinary, to say the least. As they contemplate the creation, both prophets are immensely impressed with *God*. If they understood man's creation as a naturalistic process that did not in fact require God (again, the explicit program of standard evolu-

tionary theory), or that only required him minimally, why would they have come away so impressed with *him*?

So we have to wonder. Was the creation of man a miraculous process—that is, one that cannot be entirely accounted for by known naturalistic principles? Or was it, though God’s method, a process in which he played no appreciable role until it came time to place the spirits of Adam and Eve into the bodies that a naturalistic, complex, and time-immense process had finally produced for them? And if the latter, how can we explain Jacob’s and Moroni’s wonder at it all?²⁶

In short, it seems to me that, given the Lord’s propensity for working at least part of the time in a miraculous way with physical elements—and often in a way that seems intended specifically to save time—it’s worth wondering why he wouldn’t do this to some degree in the largest project of all (the creation of the earth and its life-forms), or at least in his most important project of all (the creation of man). Are these the places where time does not matter and where miraculous, inscrutable processes have no place? And more importantly, if so, then why do Moroni and Jacob seem unable to see this and, in fact, insist on just the opposite?

There are many related questions, of course, and I claim no final answers to them. I recognize that the Lord could have any number of reasons in mind for anything he does and that they don’t have to be translucent, much less transparent, to me. At the same time, I think these questions are unavoidable given certain elements of the scriptures, and since Latter-day Saint scientists must always consider

26. One argument might be that they lived so long before the twenty-first century that they didn’t have the benefit of modern science to acclimate them to things miraculous. That may seem a natural direction to go at first, but I think that in the end it is implausible. Consider Moroni. The Lord appeared to him, and probably even hovered. Now, certainly that would have seemed marvelous and miraculous to Moroni in the fifth century, but it would have seemed no less miraculous had it happened to him in the twenty-first. No one has an explanation for such a phenomenon even in these modern and information-drenched times. Indeed, can we identify *any* miracle Moroni had record of in the fifth century—and that would have conditioned his view of what counts as miraculous—that science can explain even today? I can think of none. So while I think Moroni would be tremendously impressed by what man is able to do in the twenty-first century, I do not think it would change in the slightest his view of what it is marvelous for God to do.

scriptural dimensions anyway, I think this question about miraculous creation is one to add to the list. I hope the next scientists who write about evolution and the gospel will take a stab at answering it.²⁷

Of Scripture and Surprise

From a scientific and intellectual standpoint, we should live in anticipation of surprise. But of course the same is also true when it comes to understanding many matters that are presented in the scriptures. Certain doctrines are repeated so often in the standard works, and are so central to the plan of salvation, that they are well understood in their basics and are subject to the Lord's promise of spiritual confirmation. Fundamentals regarding the Godhead, the reality of Jesus Christ as the Savior of mankind, the truth of the Book of Mormon, the divine calling of the Prophet Joseph Smith, and the reality of God's kingdom on earth as led by living prophets all fall in this category. These are certain.

But the scriptures teach a lot more than these fundamentals, including making various statements about the creation of the earth and of man and other forms of life. On these and many other matters, intelligent, devoted, and spiritually sensitive people can see issues differently and reach different conclusions. That's to be expected, and it is an argument, just as in academic inquiry, for a lingering tentativeness and humility regarding whatever reading we adopt among the manifold possibilities.

27. Next to timeless passage through space, the most astounding of all God's characteristics (whether the Father or the Son—they are largely interchangeable in the relevant scriptural passages) seems to me to be his intuitive union, through the Spirit, with all the elements throughout his vast kingdom (see D&C 88:5–13, 41). Thus he knows all things (2 Nephi 9:20) because all things are present before his eyes (D&C 38:2; also Moses 1:6), and he also knows the very thoughts and intents of the heart of all the children of men (Alma 18:32) throughout his creations. This consideration, among others, convinces me that God's dimension of life differs so radically from our own, both in quality and in capacity, that we cannot even begin to appreciate the difference, much less comprehend and account for it. We cannot explain the least of his miracles, much less the greatest. This realization compels me to a general skepticism toward mortals' intellectual conclusions, including my own, regarding the things God is able to do and the means at his disposal to do them.

Although by itself this is a helpful reminder, there's actually more to the issue than this, for however much the scriptures reveal to one extent or another—and however difficult it is to be certain of any one reading and to reach 100 percent agreement among all careful students on the matter in cases of such revelation—there is far more that the scriptures *don't reveal at all*. From the level of minute detail about human accountability and agency (which is why it is impossible to judge others and their situations) to the level of the cosmic panorama of realm upon realm of celestial life, celestial activity, and celestial beings, we know next to nothing. Despite how much we think we know, the scriptures contain barely a fraction of all reality, barely a fraction of all that is true and that the Lord *could* reveal.

Doctrine and Covenants 76 as a Surprise

To see this we need think only of the surprise that the revelation on the three degrees of glory must have been to the early Saints, who had been steeped in the simple heaven/hell topography of the Book of Mormon and the Bible (1 Corinthians 15 notwithstanding). What reason did they have to think that there was much more to discover about a topic mentioned so frequently and so similarly in both books of scripture? Then came section 76. Talk about surprise! Indeed, upon hearing of the vision experienced by Joseph Smith and Sidney Rigdon, Brigham Young could only say: "I was not prepared to say that I believed it, and I had to wait. . . . I handed this over to the Lord in my feelings, and said I, 'I will wait until the Spirit of God manifests to me, for or against.'"²⁸

But that's only a beginning to the matter of surprise on this topic, for recall the Prophet's statement that he could explain "a hundred fold more" than he actually published regarding the degrees of glory.²⁹ That would make over eleven thousand verses—nearly twice the verses in all the Book of Mormon. So on just this one matter, what the Prophet

28. Brigham Young, in *Journal of Discourses*, 18:247 (1877), cited in Richard Lloyd Anderson and Scott H. Faulring, "The Prophet Joseph Smith and His Plural Wives," *FARMS Review of Books* 10/2 (1998): 103.

29. Joseph Smith, in *Teachings of the Prophet Joseph Smith*, comp. Joseph Fielding Smith (Salt Lake City: Deseret, 1972), 305.

revealed to us is a mere 1 percent of what he himself knew—and what fraction was that of what *God* knows?

So we don't know much, certainly less than it is easy to think we know. But the problem is bigger yet. For again, on just this one matter, when we are finally in a position to know all that the Prophet knew, we are likely to understand new truths and experience new perspectives that will dramatically inform and revise matters that we *think* we understand now: subsequent revelations have a way of informing previous ones. Consider how the Lord's explanation of the term *endless punishment* revises and clarifies all earlier thinking about the nature of God's punishment (D&C 19:5–12); eight short verses reorient everything revealed on that topic in any other book of scripture. And surely that will happen again when we learn the full truth about degrees of glory in eternity.

So while it's easy to think that we know a lot about this topic already, I daresay we are in for a bigger surprise than Brigham Young's. And, of course, that will happen on an endless number of other topics as well: logic requires it, and scripture promises it (D&C 101:32–34), including new information on the details of creation.

In this connection, note what President Spencer W. Kimball reported on one occasion: “I have learned that where there is a prayerful heart, a hungering after righteousness, a forsaking of sins, and obedience to the commandments of God, the Lord pours out more and more light until there is finally power to pierce the heavenly veil and to know more than man knows.”³⁰

“To know more than man knows.” No statement could better capture my point about both scriptural and intellectual matters, and from a firsthand personal witness no less. The point is inescapable: in both intellectual and scriptural matters we should eschew naïve dogmatism and live instead in welcome anticipation of surprise.

A Prediction

This all brings me to a final, and personal, point regarding science, scripture, and surprise.

30. Spencer W. Kimball, “Give the Lord Your Loyalty,” *Ensign*, March 1980, 4.

From Darwin to the present, it has been common to see evolution as a mechanism for the creation and development of life-forms, including man, that does not require God. This naturalistic theory has undergone and is undergoing revision in various dimensions,³¹ but the absence of a role for God, and often the consequent implication of his nonexistence, is, at least among high-profile figures in the scholarly world, an enduring feature.³² Those who disbelieve in the

31. Gould summarizes the revisions he sees in the following way. First, he identifies as the central core of Darwinian theory three primary principles that are part of the central Darwin “logic” in addition to the fundamental idea of natural selection: (1) insistence on the level of individual organisms as the exclusive locus for selection and evolutionary change (which Gould considers the most radical and distinctive feature of Darwin’s theory); (2) insistence on the efficacy of natural selection to “create the fit”—as well as eliminate the unfit in crowded populations—by the slow microevolutionary accumulation of favorable variations over time; and (3) insistence, by extrapolation, on the sufficiency of such favorable variations to yield macroevolutionary changes as well: to generate “the entire pageant of life’s history, both in anatomical complexity and taxonomic diversity” (p. 15), which obviously includes the insistence that no additional causal principles would be required to generate this pageant of life’s history. Then, to capture these central features of Darwin’s theory, Gould employs a drawing of a fossil coral consisting of a central trunk (representing the basic theory of natural selection), three extending branches (representing the three principles mentioned above), and sub-branches (representing less central elements of Darwinian theory). Gould then describes as “K-cuts” any modifications of the theory that would kill at least one of the three central principles of Darwinian logic and thus destroy the theory. He describes as “R-cuts” any modifications that would revise enough of the original form of one of the three central branches to be an important revision of the theory, while leaving the Darwinian foundation intact. And he describes as “S-cuts” (subsidiary cuts) modifications that affect only a sub-branch of the theory. Gould believes that Darwinian theory has undergone and/or is undergoing R-cuts to all three central branches and that these revisions make Darwinism “a far richer and fascinatingly different theory,” although it retains the Darwinian core “rooted in the principles of natural selection” (p. 20). He also says: “I do believe that the Darwinian framework, and not just the foundation, persists in the emerging structure of a more adequate evolutionary theory. But I also hold . . . that substantial changes, introduced during the last half of the 20th century, have built a structure so expanded beyond the original Darwinian core, and so enlarged by new principles of macroevolutionary explanation, that the full exposition, while remaining within the domain of Darwinian logic, must be construed as basically different from the canonical theory of natural selection, rather than simply extended [from it]” (p. 3). Of course, Gould assumes in this model a particular reading of Darwin—a reading criticized by some, but defended by Gould. See again note 10 herein.

32. See, for just two prominent authors, Richard Dawkins, *The Blind Watchmaker: Why the Evidence of Evolution Reveals a Universe without Design* (New York: Norton, 1996), and *The God Delusion* (Boston: Houghton Mifflin, 2006); and Daniel C. Dennett, *Darwin’s*

existence of God are flatly wrong, to say the least, and eventually they will see this. The surprise for them will be large indeed.

At the same time there are those who see evolutionary mechanisms as God's *method* not only for creating and developing the various forms of life on earth but also for creating man. This is the view of Stephens and Meldrum, and no doubt of many other Latter-day Saint scholars. I have not argued for it here, but I want at least to state my personal conclusion on this matter: I anticipate being surprised about many things once I reach the other side, but nothing would surprise me more than to discover that such scholars are right about this. I think time will eventually show that the current state of thinking about the evolution of man is thoroughly false and that we might have recognized this earlier if not for some of the complexities (of all kinds) that inhere in scientific investigation itself. So while I am completely willing to be surprised should the theistic evolutionists turn out to be right, my best thinking leads me to expect otherwise.

I believe they're the ones who are in for a surprise.³³

Dangerous Idea: Evolution and the Meaning of Life (New York: Simon and Schuster, 1995), and *Breaking the Spell: Religion as a Natural Phenomenon* (New York: Viking, 2006).

33. Though the matter of the evolution of life-forms other than man is to me a lesser issue, I believe mainstream thinking about this is destined to be mistaken on any number of levels as well. I don't know what the truth will turn out to be, but I'm guessing that everyone will be surprised in the end—including those who have thought about it the most.