TWO BAD WAYS TO ATTACK INTELLIGENT DESIGN AND TWO GOOD ONES

by Jeffrey Koperski

Abstract. Four arguments are examined in order to assess the state of the Intelligent Design debate. First, critics continually cite the fact that ID proponents have religious motivations. When used as criticism of ID arguments, this is an obvious *ad hominem*. Nonetheless, philosophers and scientists alike continue to wield such arguments for their rhetorical value. Second, in his expert testimony in the Dover trial, philosopher Robert Pennock used repudiated claims in order to brand ID as a kind of pseudoscience. His arguments hinge on the nature of methodological naturalism as a metatheoretic shaping principle. We examine the use of such principles in science and the history of science. Special attention is given to the demarcation problem. Third, the scientific merits of ID are examined. Critics rightly demand more than promissory notes for ID to move beyond the fringe. Fourth, although methodological naturalism gets a lot of attention, there is another shaping principle to contend with, namely, conservatism. Science, like most disciplines, tends to change in an incremental rather than revolutionary manner. When ID is compared to other non- or quasi-Darwinian proposals, it appears to be a more radical solution than is needed in the face of the anomalies.

Keywords: conservatism; Dover trial; Intelligent Design; methodological naturalism; theory change

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Yet it may be worthwhile to speculate about possible limits to Darwinism. For we should always be on the lookout for possible alternatives to any dominant theory.

– Karl Popper (1996, 8)

Jeffrey Koperski is Professor of Philosophy, Saginaw Valley State University, 7400 Bay Road, University Center, MI 48710; e-mail koperski@svsu.edu.

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For better or worse, Intelligent Design (ID) is now part of the culture wars in America. Some are happy that the topic is getting so much press. Others, myself included, believe that the bright lights and heated rhetoric make it very difficult to examine the issues in a rational way. As a philosopher of science, I am mainly interested in the arguments. At the end of the day, do we need an explanation for the fine-tuning of the cosmos? If so, what is the best explanation? Likewise for the origin of life on Earth and its evolution over time; what are the competing explanations and how do we assess them? These are questions that can be discussed at a scholarly level. The purpose of this essay is to push that discussion along just a bit.

As the title suggests, we examine here two bad arguments used against ID and then two that are more promising. The first is a relatively simple exercise in informal logic. ID critics often use fallacies that should be familiar to any logic student. The second argument comes from the philosophy of science: using questionable—and sometimes repudiated—claims in order to brand ID as a kind of pseudoscience. Some of the arguments presented in the 2005 lawsuit against the Dover, Pennsylvania, school board are discussed in this section. The third has to do with the progress of ID in the natural sciences. The fourth is a comparison between ID and other non-Darwinian approaches.

Note that although the debate is usually labeled ID versus evolution, this is a false dilemma. There is considerable ambiguity in both terms. Not all ID arguments are about biology. In my view, the best are cosmological, having to do with the fine-tuning of physical constants. For the most part, I restrict the discussion to evolutionary arguments.

Motives and Associations

The purported goal of ID is to push biology out of a cul-de-sac, the degenerating theory of neo-Darwinism. But what is it *really* about? Is it, as Barbara Forrest and Paul Gross put it in the title of their book *Creationism's Trojan Horse* (2004), a way of sneaking good old-fashioned creation science past the censors? The critics clearly think so. They prefer the label "Intelligent Design Creationism" to help drive the point home. ("Neocreos" and even "country-bumpkin creos" may be used when they are feeling less charitable.) Judge John E. Jones III apparently agrees: "The evidence at trial demonstrates that ID is nothing less than the progeny of creationism" (*Kitzmiller v. Dover* 2005, 40).

Is this accurate? *Creationism*—much like *evolution*—is a rather ambiguous term. As one of the members of our biology department defines it, all theists are creationists. But that can't be correct. Kenneth Miller is a well-known ID critic and a Roman Catholic. No one familiar with the debate would consider him a creationist. Traditionally, the term refers to those who believe in a literal reading of Genesis, which entails that the earth is

less than 20,000 years old. If you were to bump into a person on the street who supports ID, that person would likely be a traditional creationist of this sort. Among prominent ID leaders, however, philosopher Paul Nelson is the only classical creationist, so far as I know. Others, such as biochemist Michael Behe, accept the common ancestry of species and have no particular qualms about the fossil record. In other words, Behe believes in macroevolution; it is the sufficiency of the Darwinian mechanism that he doubts.

In my view, labeling those who doubt the efficacy of genetic mutation and natural selection "creationists" is a rhetorical strategy, what some logic texts call "stereotyping." Cable television provides ready exemplars for both the creationist stereotype and its cousin, the fundamentalist. Critics try to shape the debate by connecting ID to these templates. If successful, little work needs to be done. The labels tell us who represents the side of rationality over and against the side of ignorance. Having sorted *us* and *them*, what *they* actually say matters little, whoever *they* happen to be. We must recognize that although this is a common argumentative strategy in talk radio and presidential politics, it is not itself a logical critique. Placing the black hat on one's opponent is no substitute for an argument.

Regardless of what labels we use, the question of motivation appears to be the key issue in this debate. What is it that ID proponents *really* want? Fortunately, an "anonymous source" at the Discovery Institute provided the answer, reports Forrest, in "an internal . . . document, titled 'The Wedge Strategy'" (2001, 3). Judge Jones was quite impressed with this information, citing it as decisive evidence that ID plans to replace current science with "Christian science" (Kitzmiller 2005, 36). To further this goal, says Forrest, "the [Discovery Institute] creationists have taken the time and trouble to acquire legitimate degrees, providing them cover both while they are students and after they join university faculties" so as to "blend more smoothly into the academic population" (Forrest 2001, 38–39). Here, then, is proof that the Discovery Institute, whose fellows are the leaders of ID, has religious motivations. Even if they are not traditional creationists, they are predominately Christian, and their hope is that ID will lead to a scientific revolution that will replace naturalistic science with theistic science.

There is much here for the conspiracy theorists among us, but let us grant all this for the moment. A very important question remains: So what? How is this information relevant to the rational assessment of ID?

Consider an analogy. When I was a graduate student, one of my professors was a committed Marxist. As the faculty advisor for a socialist student group, he admittedly was interested in becoming a professor in order to promote his political views. He hoped to persuade students to do likewise. Now consider the articles he had published in scholarly journals. Did the fact that he had a political motivation affect the strength of his arguments

in those papers? Should the editors of those journals have taken his political agenda into consideration?

As every logic student knows, the answer is No. One's motivations for presenting an argument have no bearing whatsoever on the validity of that argument. Evaluating a conclusion by questioning one's motivation is an *ad hominem* attack. Arguments must be judged on their merits regardless of the source. Lysenko's theory of inheritance was not bad because the Communist Party in the Soviet Union promoted it; it was bad because his theory was an experimental failure. In contrast, Martin Luther King Jr. certainly was motivated by his religious beliefs, as was William Wilberforce. That psychological fact neither adds nor detracts from the strength of their arguments. Stereotyping, *ad hominem*, and appeals to anger are effective rhetorical devices, but they all interfere with rational investigation.

Let us focus on Forrest a bit more. What would she think if we deconstructed *her* motives? She is on the board of directors for the New Orleans Secular Humanist Association (*http://nosha.secularhumanism.net*), a group that actively tries to prove that religious beliefs are based on ignorance and superstition. They strategically promote Secular Humanism; they hold conferences; they have their own newsletters and publications; they take donations. I submit that Forrest's academic publications are motivated by her antireligious views. That may be interesting in terms of biography, but friends and critics alike should agree that it is irrelevant when assessing her arguments. The same goes for ID.

Defining the Problem Away

Robert Pennock, among others, has argued that ID advocates fail to make a distinction between methodological and ontological naturalism (Pennock 1996). The latter is the view that nothing exists beyond the natural causal order. It is the descendant of what Enlightenment thinkers called *materialism*—everything that exists is made of matter. Methodological naturalism (MN) has to do with theory formation: Researchers must proceed as if ontological naturalism were true, regardless of whether it actually reflects their metaphysics. Immaterial entities such as spirits, souls, and formal causes have long been rejected in science. This rejection is neatly captured by MN.

It is therefore not the case, Pennock argues, that neo-Darwinists dogmatically refuse to consider ID because of some antisupernatural bias. Rather, because ID posits a nonphysical intelligence, it violates MN and is therefore not science.

Notice that MN is not a claim found within any given theory. It is rather a metatheoretic shaping principle, what Judge Jones called a "ground rule" for doing science (2005, 37–38, 81).[1] Philosophers of science have discussed a wide range of such principles, both metaphysical and method-

ological. Some are presuppositions without which science would be impossible. For example, we assume that most of the causal regularities observed today will also hold tomorrow. Researchers are thereby relying on two metaphysical doctrines: the uniformity of nature and mechanistic causation. Thus, when astronomers tell us the distance and age of a far-off galaxy, they assume that the physical laws and causal interactions between here and there are the same. If the cosmos operates according to radically different principles beyond our solar system, inferences made about the rest of the universe are most likely false. Belief in the uniformity of nature is rational,[2] but it is not something that is directly observed, nor is it held up for scrutiny within any particular branch of science. Shaping principles fall into a broad, overlapping region between theoretical science and the philosophy of science.

As the name suggests, MN is a methodological rather than metaphysical shaping principle.[3] Others go back to the beginnings of the scientific revolution, such as the reliance on repeatable, intersubjective observations. Some, like procedural norms for conducting experiments, are more recent and discipline-specific. Still others are matters of inductive logic and mathematics, such as the proper use of statistical methods. One could also add the so-called explanatory virtues of simplicity, testability, internal and external coherence, fruitfulness for future research, and wide scope.[4] Each of these is important when considering rival hypotheses.

That MN is responsible for rescuing science from supernatural design is doubtful.[5] As far as I can tell, historical confrontations between naturalistic and design hypotheses were settled by inference to the best explanation using the explanatory virtues just mentioned. The appeal to MN is a more recent move. In any case, I do not wish to challenge the claim that MN is a methodological shaping principle. The problem has to do with the wielding of MN to define ID as religion. As Judge Jones put it, "ID's failure to meet the ground rules of science is sufficient for the Court to conclude that it is not science" (2005, 91). Scientists, teachers, and textbook writers therefore need not consider it.

A crucial assumption in all of this is that once a concept achieves the status of shaping principle it becomes an immutable axiom for all future science. That is a false assumption, if the history of science is any guide. Almost everything in science has been subject to change, from data and models to theories and laws. Like mutual funds, past success does not guarantee future performance. Shaping principles are no exception. In many cases, one desideratum is traded off against another.

Consider simplicity. Among competing explanations, we tend to prefer those that are simple and elegant over those that are complex and convoluted. Scientists, like the rest of us, routinely ignore this preference due to an implicit *ceteris paribus* (all things being equal) condition. As philosopher Robert Kraut used to say, when the ceterises aren't paribusing—and

they usually aren't—simplicity doesn't amount to much. The Standard Model of particle physics has tremendous explanatory power, uniting the strong, weak, and electromagnetic forces, but it is far more complex than anything atomic theorists had envisioned at the turn of the last century. Renormalization methods used to manage its sometimes inconvenient mathematics are neither simple nor elegant. They do, however, work quite well. The point is that, like moral duties, the explanatory virtues often conflict, but nothing in the mythical "scientific method" says how to resolve these tensions.

For a less well-known example of suspended shaping principles, consider the eighteenth-century conflict between Jean le Rond d'Alembert and Leonhard Euler. D'Alembert argued that only very special kinds of motion could possibly be described mathematically (Truesdell 1984, 80–83; Wilson 2000a, 298–301), a view that can be traced back to Descartes in his analysis of curvilinear motion (*Principles of Philosophy* III, §57–61). D'Alembert believed that mathematical physics would have to be restricted to particularly simple cases. Even a plucked violin string would prove to be too complex. Euler, in contrast, believed that the mathematics should be given a longer leash. In doing so, one would have to suspend both the mathematical rules for differential equations and a more fundamental metaphysical rule regarding change. As Clifford Truesdell puts it,

> Euler, in philosophy eclectic, adhered fundamentally to one tenet of Descartes . . . which I translate "by facts and by reason." Thus when faced with the facts of acoustic vibration Euler was ready to relax an overriding and up to then fruitful principle of mathematical philosophy [Leibniz's Law of Continuity], provided reason could be brought to bear so as to produce a decent model. (1984, 81)

Euler knew he was breaking the rules, but he believed it was warranted. Even the deeply embedded law of continuity was not safe.

A similar battle was fought in the twentieth century, this time between Albert Einstein and Niels Bohr. According to the so-called Copenhagen Interpretation of quantum mechanics (QM), the wave function is a complete description of a quantum system; there are no facts in the evolution of the system that the wave function fails to capture. In particular, the wave function will not assign position and momentum to an unobserved particle traveling across this room. Hence, according to this view, it has none. There is no fact of the matter regarding the particle's position and momentum. Following Max Born, the square of the absolute value of the wave function can be used to predict the probability that the particle will strike some region of a target. But, unlike Newtonian bodies, not even a Laplacean demon with complete knowledge of the laws of physics and the initial conditions can know precisely where the particle will land. On Bohr's view, this is because the probabilities involved are ontic, not merely epistemic. Nature is not deterministic, change is not always continuous, and

some events do not have a cause. Each of these constitutes a change of metaphysical shaping principles. Einstein famously hated this view, arguing instead that QM was incomplete. There *are* physical facts within the purported domain of the theory, Einstein believed, yet QM fails to capture them. He vigorously argued that the longstanding principles of local causality and determinism ought not be rejected. If a theory violates these, so much the worse for that theory. Although Einstein was a giant in an age of heroes, Bohr won this debate. Every consistent interpretation of QM forces a change in our commonsense metaphysics, at least at the subatomic scale.

It is undeniable that shaping principles have been suspended and changed throughout the history of science. Aristotelian principles were replaced by Cartesian ones. Cartesian principles did not survive the Newtonian revolution.[6] This sort of flexibility is a necessary condition for advancement. Einstein believed that the universe was infinite and unchanging on a large scale. When his field equations showed that space-time must either expand or contract, Einstein introduced his infamous cosmological constant to allow for a static solution. He soon changed his mind. The point is that if physicists were unable to discard the metaphysics supporting a static universe, Big Bang cosmology would have been ruled out as pseudoscience.

ID critics have argued that MN is sacrosanct. In his Dover testimony, Pennock claimed that "This self-imposed convention of science, which limits inquiry to testable, natural explanations about the natural world, is referred to by philosophers as 'methodological naturalism' and is sometimes known as the scientific method" (*Kitzmiller v. Dover* 2005, 83). Naturalistic explanations are not merely desirable, they are "an essential attribute to science by definition and by convention" (p. 84). ID violates MN and thus "by definition" it cannot be science. As I read it, Pennock appears to have solved the demarcation problem.[7] If so, why has this not appeared in the *Journal of Philosophy*?

The answer, of course, is that he has done no such thing. MN is not an immutable principle either in theory or in fact, and it does not separate science from nonscience. As philosopher of science Philip Kitcher put it in an anticreationist text, "postulating an unobserved Creator need be no more unscientific than postulating unobserved particles" (1983, 125). W. V. O. Quine expressed a similar view both in his early work (Quine [1951] 1980, 45) and in one of his last articles: "If I saw indirect explanatory benefit in positing sensibilia, possibilia, spirits, a Creator, I would joyfully accord them scientific status too, on a par with such avowedly scientific posits as quarks and black holes" (1995, 252).

Kitcher and Quine are speaking of the possible suspension of MN, but we need not go all the way back to Newton to find concrete examples. James Clerk Maxwell argued in 1870 that the uniform properties of atoms, especially their discrete line spectra, indicates design. His reasoning is very similar to modern fine-tuning arguments about the cosmos. As far as

nature is concerned, the frequencies of these line spectra could vary considerably, but in fact they do not.

> The period of vibration of a luminous particle is therefore a quantity which in itself is capable of assuming any one of a series of values, which, if not mathematically continuous, is such that consecutive observed values differ from each other by less than the ten thousandth part of either. There is, therefore, nothing in the nature of time itself to prevent the period of vibration of a molecule from assuming any one of many thousand different observable values. (Maxwell 1870)

Maxwell shared the same conclusion for this as Sir John Herschel: They "must therefore have been made." A surprising fact about this quote is that it comes from Maxwell's *Encyclopædia Britannica* entry on atoms rather than, say, a letter in which he is dabbling in theology. In his view, there was nothing unscientific about his reasoning or conclusion. Philosopher of science Mark Wilson concurs: "surely Maxwell's nonnaturalistic proposal should count as 'purely scientific,'" even though it clearly violates MN (2000b, 1–2).

The bottom line is this: The future use or suspension of MN depends on what is discovered. If the best explanation for some new phenomenon is design, even supernatural design, it would still count as a scientific explanation. It borders on academic incompetence to pretend that science has strict boundaries and then gerrymander those boundaries to keep out the riffraff. Philosophers of science in particular should know better.

Let us consider an objection. Some argue that design explanations would hamper the progress of science. Because "God did it" is potentially an answer to any Why question, allowing design back into biology is a science stopper. If divine fiat is an acceptable explanation, why push on with difficult and expensive research? What I have failed to recognize, then, is the utility MN has for motivating scientific progress.

I think this is a plausible objection. If one already has an answer to a question, why search for another? The only thing I ask is that we hold this claim up against the history of science in order to test it. Is it the case that design explanations proved to be overly tempting for theistic bench scientists and theoreticians and so blocked the development of rival theories? ID critics often assume that this must have happened sometime or other, hence the need for MN. The history of science remains uncooperative on this point, however (see Ratzsch 2005, 136–39). We already have considered Maxwell's design argument based on atomic physics. I see no evidence that his work in statistical mechanics or electromagnetism was impeded by the possibility of design. As far as I can tell, "God did it" is simply not an answer that theists reach for whenever research bogs down.

To sum up this section, I agree that MN is indeed a shaping principle in contemporary biology. This does not mean it is an inviolable maxim that scientists must employ come what may. No one knows what sort of explanatory resources science will need in the future. Naturalistic critics of

ID certainly may bet that no forthcoming discovery will require design, but they cannot guarantee it. In order to know one way or the other, the evidence and arguments have to be evaluated. In order to evaluate design claims, scientists must be allowed to consider them *qua* scientists, rather than being told that such inquiries must be left to theologians and fundamentalist preachers.

We have examined two bad arguments used against ID. Both are rhetorically effective, persuading teachers and judges alike, and I fully expect to see them in the future. My appeal to those in the academy is this: Let us not use bad arguments as a means to an end. Is ID science? Yes, but that is not a particularly interesting question. Cold fusion is science, but it seems to be bad science. This, then, gives us some indication about how to proceed.

Good Science

On what grounds can one properly criticize ID? Although I do not think ID is merely creation science under new management, the older debate between evolution and creationism is instructive. In my view, Larry Laudan got it exactly right:

> Rather than taking on the creationists obliquely and in wholesale fashion by suggesting that what they are doing is "unscientific" *tout court* (which is doubly silly because few authors can even agree on what makes an activity scientific), we should confront their claims directly and in piecemeal fashion by asking what evidence and arguments can be marshalled for and against each of them. The core issue is not whether Creationism satisfies some undemanding and highly controversial definitions of what is scientific; the real question is whether the existing evidence provides stronger arguments for evolutionary theory than for Creationism. . . . Debating the scientific status of Creationism (especially when "science" is construed in such an unfortunate manner) is a red herring that diverts attention away from the issues that should concern us. (Laudan 1982, 18)

Replace *Creationism* with *ID* and Laudan could resubmit this as a response to the Dover decision. ID must be judged on its merits.

So, how does it stack up? If ID is indeed scientific, as I have argued, is it good science? As even its staunch supporters are willing to admit, that is a much more difficult case to make. Although ID clearly has the attention of researchers on both sides, design-driven bench science has failed to keep up with the publicity.

At the moment, the bulk of ID science falls into two categories. The first includes a host of examples that are problematic for neo-Darwinism. These usually are systems that are irreducibly complex in Behe's sense or that display William Dembski's specified complexity.[8] ID critics have replied by showing that although complex systems like the bacterial flagellum are improbable, they are still consistent with neo-Darwinism. In other words, there are many soft anomalies in the literature but no hard ones. A

hard anomaly is an observation that cannot be explained in terms of the reigning theory. For example, black body radiation and the photoelectric effect showed that something was wrong with classical models of the atom. Some change or other had to be made in order to accommodate the new observations. No changes are required in order to accommodate soft anomalies, which do not comfortably fit within the reigning theory but are, strictly speaking, possible. So, although it may be difficult to imagine how a combination of mutation and natural selection could produce highly integrated systems like the flagellum, they are logically consistent with Darwinism.

The second kind of ID science includes research that fits nicely within a design framework, even though the researchers themselves do not support ID. For example, ID proponents often speak favorably of research showing that so-called junk DNA actually is functional (Moore 1996). The idea is that one should expect more purpose in biological structures than would be expected from a Darwinian point of view. The nanotechnology approach to microscopic systems is also considered very ID-friendly. The reason scientists find such utility in thinking of biological systems as machines is because, in some sense, that is what they are. The conceptual link with human artifacts is not merely a metaphor.

These kinds of ID-related science are well and good, as far as they go. What critics rightly clamor for, however, is peer-reviewed research in which design has more than a mere heuristic role. To be fair, there are more published papers out there than most people realize.[9] And, as ID proponents argue, there is a strong bias against design-motivated articles getting into academic journals, as the uproar over Stephen Meyer's (2004) showed.[10] Editors will not risk giving aid and comfort to the enemy. In my view, the ID community is itself partly to blame for this. Some think of ID primarily as a weapon in the culture wars. Anti-design bias in the academy is part of the backlash. Had ID consistently emphasized research over public exposure, the atmosphere of the debate would be different today. Instead, Phillip Johnson and others believed that the underlying ideas were so compelling that, once they were disseminated, ID thought would sweep across the landscape. A 2001 front-page story in *The New York Times* (Glanz 2001) was cause for much celebration not because it was pro-ID but because it helped place the debate in the public eye. This is proving to be a failed strategy.

Tactics aside, is ID a thriving research program by any measure? No, not in my opinion. Bias is an obstacle. How many scientists would come out of the closet if there were no risk to their careers? By my estimation, there are hundreds who would like to make a contribution, who believe that there is something critically flawed about the neo-Darwinian paradigm and that design is a better explanation for what they observe. Yet they withhold their professional opinions because of what it would mean for their careers in the present climate.

Bias is not the biggest obstacle for ID, however. The elephant in the ID room is the lack of a clear vision of what design research might be. Although Dembski has provided an outline for how one might proceed (2004, chaps. 43–44), the average design-friendly scientist still does not know quite what to do. As long as this is the case, critics may rightly contend that although ID may be scientific, it is at best fringe science.

More Radical than Necessary

There is a less visible but equally menacing problem for the long-term viability of ID. As we have seen, MN gets a lot of attention. But there is another methodological shaping principle to be contended with, namely, conservatism. Very broadly, the idea is that when faced with anomalous new data scientists prefer incremental change over more revolutionary change. The second way one may properly criticize ID is to show that it is a more radical proposal than is needed in order to accommodate the evidence against neo-Darwinism.

We find two related but distinct ideas under the rubric of conservatism. One is *epistemic conservatism*, which normally refers to a king-of-the-hill approach: that one should keep one's current set of beliefs unless something better comes along to displace them. The fact that other possible views exist, even equally good ones, ought not be enough to change one's justified beliefs.

A second version may be traced to Quine's doctrine of *minimal mutilation*: New observations may force a change in one's beliefs, but one should make the smallest change possible in order to accommodate the new information (Quine [1951] 1980, 42–44). What Quine actually said in this essay ("Two Dogmas of Empiricism") was that minimal mutilation is "our natural tendency"—a descriptive, doxastic claim. In the hands of philosophers of science such as Larry Sklar (1975), minimal mutilation becomes a normative doctrine. Belief change should be minimal; dramatic changes ought to be considered only when necessary. This is now a widely accepted view among epistemologists.[11]

It is a short step, as Quine himself suggested, from this doctrine about one's noetic web to the collective beliefs of the scientific community. If scientists practice minimal mutilation individually, their theories also will tend to develop in a conservative way. As new discoveries are made, the body of scientific knowledge should change as little as needed in order to accommodate them. I refer to this normative principle of theory change as *scientific conservatism*.

To see how this applies to ID, we must recognize that there is a legitimate scientific controversy over the mechanisms of macroevolution. ID critics often downplay these debates so as not to benefit the enemy, but they are there nonetheless. (In fairness, ID advocates often exaggerate the

controversy.) Molecular biologist James Shapiro describes the current situation this way:

> [The] debate about evolution continues to assume the quality of an abstract and philosophical "dialogue of the deaf" between Creationists and Darwinists. Although our knowledge of the molecular details of biological organization is undergoing a revolutionary expansion, open-minded discussions of the impact of these discoveries are all too rare. The possibility of a non-Darwinian, scientific theory of evolution is virtually never considered. . . . I propose to sketch some developments in contemporary life science that suggest shortcomings in orthodox evolutionary theory and open the door to very different ways of formulating questions about the evolutionary process. (Shapiro 1997)

Years before Shapiro was in graduate school, Stephen J. Gould rocked the boat of what he later called Darwinian fundamentalism with his theory of punctuated equilibrium, but his more damaging proposal had to do with macromutations—large-scale, systemic mutations in a single generation. This was not quite the so-called hopeful monster thesis that prompted such outrage against geneticist Richard Goldschmidt in the 1940s, nor was it the accumulation of tiny changes required by Darwin. Evolutionary biologists remain unimpressed with Gould, and their mutual dismissal continued to the end of his life. (The polemical baton has been picked up by Simon Conway Morris, who has little respect for "ultra-Darwinists" and "their almost unbelievable self-assurance, their breezy self-confidence. . . . [Far] more serious, are particular examples of a sophistry and sleight of hand in the misuse of metaphor, and more importantly a distortion of metaphysics in support of an evolutionary programme" [2003, 314].) Other proposals that are neither design-based nor fully Darwinian come from Stuart Kauffman (self-organization and autocatalysis), Brian Goodwin (morphogenesis and developmental constraints), Richard Watson (compositional evolution), Shapiro (natural genetic engineering), Conway Morris (genotype convergence), and a host of neo-Lamarckian geneticists arguing for various types of directed mutation (Jablonka and Lamb 1995). This list is far from complete.

That others recognize anomalies for contemporary Darwinism is generally thought to be good for ID. Not all Darwinian critics can be pigeonholed into *Inherit the Wind* stereotypes. My point in this section is to show that these non-Darwinian and quasi-Darwinian proposals in fact undermine the viability of ID. Even if orthodox neo-Darwinism collapses, design obviously is not the only alternative. More important, the rivals are more conservative vis à vis the reigning theory. They do not challenge MN. In fact, very little would have to be added to textbook evolutionary theory if one or more of these is accepted. If any one of them is capable of resolving the problems posed by complex structures and macroevolution, ID is a more radical solution than is needed. In short, if scientific conservatism is a defensible, normative principle of theory change, it undermines the acceptability of ID.[12]

My advice—although I do not expect anyone in the ID movement to heed it—would be to adopt a less radical approach. As we have seen, critics attack ID because it violates MN. The standard view among ID theorists is that somewhere in the history of life an intelligence has interrupted the normal flow of natural law. Although the "science itself" cannot unequivocally point to the source of this interruption, most believe that it is supernatural. My not-so-original suggestion would be to ignore MN and instead focus on mechanical causation. Instead of arguing for intelligent intervention, the focus should be on the need for irreducible teleology.

Biologists use teleological notions all the time. Published papers are strewn with such terms as *function*, *purpose*, and (perhaps less often now) *design*. No one objects because of a longstanding promissory note that biological teleology can be completely explained by or reduced to natural selection. Can it? A few brave philosophers have argued not, and recent work in the philosophy of science should make one skeptical of hand-waving reduction claims.[13] More important here is the introduction of new telic notions by scientists themselves. Conway Morris has drawn from modern dynamics to make a case for evolutionary convergence (2003). Oversimplifying somewhat, he considers a vast state space where each point represents a particular genotype. When each genotype is ranked according to its fitness, the space becomes a fitness landscape (for example, Figure 1). Peaks represent greater fitness. Evolution moves each species progressively toward the peaks. This is not a new idea. According to neo-Darwinism, whether a hill in the space is instantiated depends on chance—random

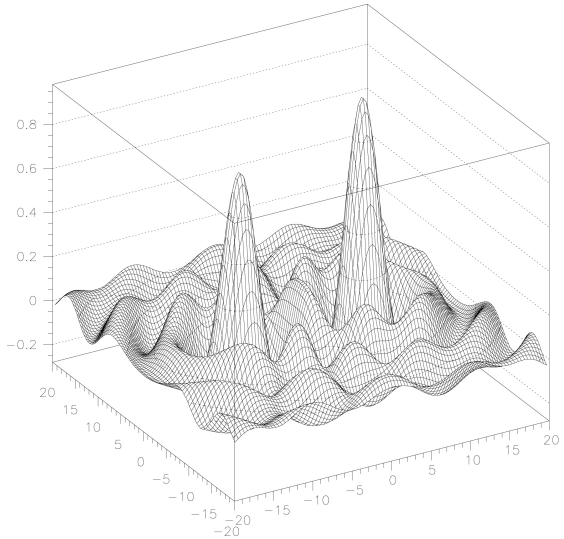

Fig. 1. Sample Fitness Landscape (© Karel Soustruznik, reproduced by permission)

mutations from one generation to the next. Whether a given zone constitutes a hill or valley depends on the environment and natural selection. On another planet, the landscape would look very different. In the standard view of adaptation, fitness landscapes are merely descriptive devices capturing the types of life most suited to a given, contingent environment.

Conway Morris instead argues that evolution tends to converge toward particular peaks in nonrandom ways. Rather than a wide-open, contingent space of possibility,

> the number of alternatives is strictly limited, with the interesting implication that the vast bulk of any given "hyperspace" not only never will be visited during evolutionary exploration but it never can be. . . . If this is correct then it suggests that an exploration of how evolution "navigates" to particular functional solutions may provide the basis for a more general theory of biology. (2003, 309)

Borrowing from dynamics, the peaks constitute attractors in the hyperspace and would be the same for any planet similar to our own. If so, alien ecosystems would evolve something very similar to mammalian species. These attractors, and therefore the fitness landscape, are determined not primarily by chance, environment, and natural selection but by matters of natural law applying to the entire cosmos. Conway Morris is careful to use the word *telos* only once, and there is nothing inherently supernatural in his account. Still, his convergent evolution sets the stage for several new questions: How does evolution navigate from peak to peak? What are the natural laws that determine these universal attractors? Are there fundamental parameters that require fine tuning to achieve the observed results? If there is a peak representing intelligent humanoids, why is that?

ID arguments about the limited explanatory power of mutation and natural selection fit comfortably within Conway Morris's picture. Both agree that new kinds of explanations are needed that would lead to a more general theory of biology. Dembski himself sometimes equates ID with a "telic process" (2004, 308) and acknowledges that some forms of naturalism are compatible with it, namely those that include irreducible teleology (2004, 172–76). Yet instead of joining hands against Dawkins-style reductionism, Dembski criticizes non-ID proponents of teleology, mainly on theological grounds. Although ID purports to have a "big tent" approach, the tent apparently is not big enough for theistic evolutionists. Mere teleology is too abstract for Dembski. If design is not empirically detectable, he believes, there is no ID. Others, like former Discovery Institute V. P. Mark Ryland, have been quietly arguing for a broader view.

The point, again, is that less radical approaches have scientific conservatism on their side.[14] Conway Morris's convergent evolution is just one example of these more moderate proposals. ID theorists have agreed to put off intramural debates about other issues, like the age of the earth. If ID really is about science, why not opt for a bigger tent, even if it is occupied with those of less conservative theology?

Conclusion

In the end, there is both good news and bad news here for ID. The bad news is obvious. There isn't enough hard science to spark the sort of revolution some imagine. What science there is can potentially be explained in less revolutionary ways. The good news is that many scientists and philosophers with impeccable credentials believe there is something right about design. As Harvard astronomer Owen Gingerich has said on more than one occasion, "I believe in intelligent design, lowercase *i* and *d*. . . . I do have a problem with Intelligent Design, capital *I* and capital *D*, because it's being sold as a political movement" (*http://www.npr.org/templates/story/story.php?storyId=4490227*).

Thus we arrive full circle at the culture wars. In my view, the leading proponents of ID often have been too quick to spot enemies and too slow at finding common ground with others. Similarly, to ID critics there can be no honest skeptics of Darwinism, only Creationists who have gone to extremes to appear dressed and in their right minds.

I doubt that the tone and tactics will change on either side. Too many rhetorical bombs have been dropped. The enemy has already been demonized. Surrounded by sophists, we are left without a Socrates. My appeal is to fair-minded academics, those who believe that logic and truth are more important than persuasion and power: Let us do better.

Note

A version of this essay was presented at the Third Annual Contemporary Philosophy Research Conference, University of Toledo, 7 April 2006. My thanks to J. Brian Pitts for many helpful comments on a previous draft.

1. For an in-depth critique of the Dover ruling by a philosopher of science, see Monton 2006.
2. David Hume, Nelson Goodman, and others, of course, have raised doubts about the trustworthiness of induction itself. Most philosophers consider these doubts as problems to be solved rather than an argument that induction is irrational.
3. It can, however, have metaphysical ramifications when coupled with scientific realism. If one takes scientific theories to be at least approximately true (realism), and if MN is used in forming those theories, then true scientific theories will be naturalistic. Hence, MN plus scientific realism leads to ON. One can avoid this conclusion by recognizing that science under MN may well produce an incomplete description of its subject matter, that is, a description circumscribed by naturalistic presuppositions. Instead, naturalists routinely cite the success of science as the primary reason for accepting ON.
4. For more on the epistemic importance of these virtues, see Lycan 1988, especially chap. 7. Chapter 8 is also relevant to my discussion in a later section of this essay.
5. See Ratzsch 2005 for more. Eighteenth- and nineteenth-century debates over flood geology might be an exception. There certainly were theological tensions between uniformitarians such as James Hutton and catastrophists like Georges Cuvier. Even in this case, however, uniformitarians generally faulted catastrophism for not being inductive rather than for violating something like MN.
6. As Larry Laudan has argued (1984, 60–61), Cartesian intelligibility ("clear and distinct ideas") had to be sacrificed in order for Newtonian gravitation to be accepted. The fact that Newtonian gravitational forces act at a distance rather than by mechanical contact was considered to be extremely problematic.

7. The difficulty is in finding criteria that count every scientific specialization as "in" while other disciplines and pseudoscience remain "out." Repeated failures to find such criteria have left philosophers of science skeptical about a hard and fast demarcation. On my view, the reason for this failure is that there is no boundary between, say, science and metaphysics. Although there are clear cases for both, the two overlap a great deal. Consider: Was Bohr's conflict with Einstein scientific or metaphysical? I believe the question contains a false dilemma.

8. I have critiqued these elsewhere (Koperski 2003; 2004) and do not rehearse the arguments here. For popular introductions to these topics see Dembski's *http://www.arn.org/docs/dembski/wd_idtheory.htm* and Behe's *http://www.discovery.org/scripts/viewDB/index.php?command=view&id=54.*

9. See the Discovery Institute's annotated bibliography at *http://www.discovery.org/scripts/viewDB/index.php?command=view&id=2640&program=CSC%20-%20Scientific%20Research%20and%20Scholarship%20-%20Science*.

10. For a chronicle of the backlash against the editor, see *http://www.rsternberg.net/.*

11. See Lycan 1988, chap. 8, for more. Good Bayesians, like Timothy McGrew, argue that the metaphor will need to be cashed out in order to measure the degree of change. He suggests that a Bayesian paraphrase might be something like "in general a conditionalizing shift that keeps the likelihoods rigid will be more reasonable than a saltation to a new (though coherent) distribution that changes the likelihoods" (private correspondence).

12. In my view, conservatism is also the reason ID attracts so many theists and relatively few nontheists. If one already has an intelligence in one's ontology that can play the role of designer, ID-based science is not a radical move. For an atheist, making ontological room for a designer, especially a supernatural one, requires a greatly expanded metaphysic.

13. Mark Bedau (1991) shows that processes equivalent to natural selection occur in inorganic structures, such as crystals. If natural selection is sufficient for teleology, crystal growth must also count as teleological. The argument is a *reductio*. Because the production of inorganic crystals does not require teleology, natural selection is in fact not a sufficient condition for it. As for general skepticism about theory reduction, the paradigm cases have taken a beating of late. Contrary to popular belief, thermodynamics has never been reduced to atomic physics (Sklar 1999); classical mechanics—chaos in particular—does not reduce to quantum mechanics (Batterman 2002, chap. 7); nor can ray optics be deduced as a special case of wave optics (Batterman 2002, chap. 6).

14. As philosopher/physicist Brian Pitts has pointed out to me, things are different when we consider cosmology and fine-tuning. If one thinks that fine-tuning requires an explanation, there are only two options at the moment: design or some kind of multiverse. In both cases, shaping principles will be violated, MN on one hand and Ockham's razor on the other. It is not clear to me that conservatism favors one or the other. It would depend, it seems, on what other metaphysical principles one already accepts.

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