Darwin Tried and True

Robert J. Richards

WHAT DARWIN GOT WRONG. Jerry Fodor and Massimo Piattelli-Palmarini. xxii + 264 pp. Farrar, Straus and Giroux, 2010. $26.

In November of 1859, when Charles Darwin excitedly held in his hands the first published copy of *On the Origin of Species*, virtually no reputable naturalist in England, on the Continent or in America accepted the proposition that species had altered over vast periods of time. The conception itself was not new: Jean-Baptiste de Lamarck had advanced it at the beginning of the 19th century, as had Darwin's own grandfather Erasmus Darwin at the end of the 18th. Even Immanuel Kant entertained the hypothesis, although he rejected it for lack of evidence. By the time Darwin died in 1882, however, hardly a naturalist of standing could be found who denied the transmutation of species, so utterly convincing was the argument of his great book.

In the *Origin*, Darwin gathered evidence of various sorts: from biogeography, from embryology, from systematics, from anatomy, from behavior and especially from the experience of breeders in transforming their stocks. Virtually all of these areas of evidence were already well known to naturalists. Darwin, however, wove these strands together into what he called "one long argument," the force of which depended on his causal device of natural selection. Without natural selection as its unifying theme, his argument would have unraveled into its heterogeneous threads.

Darwin's triumph in the 19th century occurred in the face of historical irony. Although naturalists came to agree that species altered over time, many had objections to the cause Darwin posited. The astronomer and philosopher John Herschel called natural selection "the law of the higgledy-piggledy," as if chance could shape the design of organisms. The biologist St. George Jackson Mivart maintained that natural selection was only an extrinsic, negative factor and that descent required a positive, intrinsic principle. Even Alfred Russel Wallace, cofounder of the theory of evolution, thought that Darwin's term *natural selection* was too anthropomorphic, suggesting as it did intentionality on the part of nature; he preferred Herbert Spencer's phrase *survival of the fittest.*

In the 1930s and 1940s, with the synthesis of modern genetics and Darwinian theory—prepared by such leading neo-Darwinists as Ernst Mayr, Sewall Wright, Theodosius Dobzhansky, R. A. Fisher, Julian Huxley and J. B. S. Haldane—the device of natural selection was cemented into the foundation for most subsequent work in evolutionary biology. Nonetheless, several of the 19th-century objections to natural selection have been reinvented and advanced by Jerry Fodor, a distinguished philosopher of mind, and Massimo Piattelli-Palmarini, a cognitive psychologist, in their book *What Darwin Got Wrong*. In the view of the authors, Darwin and the neo-Darwinians got almost everything wrong, because they founded their theories on the vacuous concept of natural selection.

Fodor and Piattelli-Palmarini divide their polemic into three parts. In the first, they maintain that most current developments in evolutionary theory show that two conjoined neo-Darwinian propositions are invalid: that phenotypic traits are the simple products of selection by an external environment, and that such selection operates without any internal constraints on variability. In the second part, they argue that the idea of Darwinian selection smuggles into the operations of nature the assumption of intentional discrimination, based as natural selection is on the model of the breeder's selecting for certain traits in organisms. In the third part, they suggest that accounts of species descent ought to be regarded not as scientific but as historical explanations. The belief that underlies all three sections is that "Darwin's theory of natural selection is fatally flawed." The authors have created a three-headed Hydra, with each head distractedly chewing off the other two.

The first part of their argument seems to be principally the work of Piattelli-Palmarini, who has some training in biology, whereas the second is most likely the creation of Fodor, who has mounted comparable conceptual arguments against natural selection in a couple

of recent essays. In the first part, the strategy is to survey a fair amount of work in evolutionary biology and then to show that this work demonstrates the operation of internal constraints on the range of genetic and phenotypic variability, thus undermining the assumption, attributed to the neo-Darwinians, that nature exogenously selects from a completely filled space of morphological possibilities.

For example, some species of the plant genus *Achillea* display "norms of reaction" that are not simply monotonically related to graduated changes in the environment: Clones of some varieties of the plant are tall at the foot of mountains and also at higher levels but shorter at intermediate levels—thus expressing endogenous constraints on phenotypic expression. The authors bring to the bar two articles (one published in 1981, the other in 1993) discussing this phenomenon, but they seem completely unaware that the classic studies of *Achillea* these articles cite were actually conducted more than half a century ago by researchers who fully embraced natural selection and the emerging neo-Darwinian synthesis (Jens Clausen, David D. Keck and William M. Hiesey, *Experimental Studies on the Nature of Species: III. Environmental Responses of Climatic Races of* Achillea [Carnegie Institution of Washington, 1948]). This is a typical example of the authors' misunderstanding of the history of evolutionary biology.

A more egregious case occurs when they favorably quote Ernst Mayr as protesting against what he has referred to as "beanbag genetics"—the view that genes singly determine phenotypic traits. Mayr, one of the architects of neo-Darwinism, is thus called on to oppose neo-Darwinism. Fodor and Piattelli-Palmarini quote at length from a 1999 study by Geoffrey B. West, James H. Brown and Brian J. Enquist (*Science*, 284:1677–1679) concerning "invariant scaling laws," which constrain phenotypic expression quite independently of natural selection; they apparently do not realize that Julian Huxley was a pioneer of this kind of allometric study in his *Problems of Relative Growth* (1932) and that he then integrated these very concepts of allometric constraint into his *Evolution: the Modern Synthesis* (1944), a classic work of neo-Darwinism.

My point is that none of the major contributors to the neo-Darwinian synthesis subscribed to the assumption against which Fodor and Piattelli-Palmarini pit more recent studies. When Mayr threw the beanbag at population geneticists like Haldane, the latter agreed that mathematical genetics made unrealistic, simplifying assumptions in order to make computation more tractable; he did not deny that actual organisms have all sorts of internal constraints. Even Darwin himself recognized that traits are often linked—that is, internally constrained; one of his "laws of variation"

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in the *Origin* is precisely that of "correlation of growth," of which he gave many examples.

The logic of this first part of the book chews right through the evidentiary support that the studies cited might appear to offer. Consider the article by West and colleagues, which Fodor and Piatelli-Palmarini obviously think is decidedly probative. The very first line of that article reads, "Evolution by natural selection is one of the few universal principles in biology." Virtually every recent study that Fodor and Piattelli-Palmarini cite likewise has natural selection as the fundamental principle of the science—the principle that, in the second part of their book, they claim to be utterly muddled. In other words, they draw their evidence from scientists whom they believe to be confused about the fundamental mechanisms of evolution. (This is no less true of the concept of "constraint on," a matter taken up below.)

Moreover, the authors are frequently able to explain the work they cite only by using the concept of natural selection. Hence, to explain the studies that they believe undermine neo-Darwinism, they must use a concept at the heart of neo-Darwinism that they suppose to be without any scientific merit. Or do they? They write, rather offhandedly: "Natural selection is real, of course (when properly construed)." Yet in the second part of the book they attempt to demonstrate that "selectionism cannot be true" and is thus not construable at all. Natural selection, then, seems to be real precisely in the sense that it is not.

The authors finally make a confession: "It's only fair to acknowledge that the majority of biologists whom we have cited here, including several of the discoverers of these quite intricate levels of endogenous regulation, still today endorse natural selection as the determinant par excellence of the course of evolution." "Fair" is hardly the term I'd apply to this admission. Fodor and Piattelli-Palmarini are conceding that what they take as evidence against neo-Darwinism is regarded by the scientists they cite as simply the normal development of evolutionary theory within the boundaries of neo-Darwinism.

What part of the work of these important scientists would the authors have us believe? Apparently none of it. Although they spend half the book reciting studies about the endogenous mechanisms that help to explain evolutionary change and branching, they preface their account with this warning: "In fact, we don't know very well how evolution works. Nor did Darwin, and nor (as far as we can tell) does anybody else." This remark is perhaps more indicative of a supercilious attitude than of a considered epistemology. If taken literally, it would mean that a great deal of time has been wasted in rehearsing studies by scientists whom the authors believe to be ignorant about the very subjects for which they've been cited. The first head of the authors' argument lies not merely severed, but chewed into indigestible gristle.

In the second part of the book, the authors argue that Darwin, along with virtually every other evolutionary biologist, has been misled by the supposed analogy between artificial selection and natural selection. When breeders select for traits, say finer wool in lambs or more desirable marbling in cattle—or, as in Darwin's own breeding efforts with pigeons, the color patterning of the ancestor—they *intend* to produce a certain trait. But nature, as the authors constantly remind us, has no intentions. Yet, in the natural situation, as they point out, a given trait that is supposedly selected for might have other features not assumed to be selected for but which come along as "free riders." To use their example, when the neo-Darwinist maintains that hearts were selected for pumping blood, he ignores that they also have the concomitant feature of making pumping noises. Thus to say that blood pumping was the trait selected for ascribes to nature an intentional act that discriminates between the two correlated traits. The human breeder might intentionally select for one of two such linked traits, but nature cannot. "Selecting for" necessarily assumes intentional power, which only human nature has.

Fodor and Piattellini-Palmarini allow that the neo-Darwinist might assume that the causal matrix formed by the environment might yet filter through one of the coextensive traits that has superior fitness value while simply allowing the free rider with no or even negative value also to pass through. (The fitness value of the peacock's tail in the presence of females might outweigh its concomitant negative values as an attractor and hindrance in the presence of predators.) But to perform this discrimination, the authors maintain, nature would have to be sensitive to the counterfactual situation: namely that, for instance, if hearts were to pump blood silently they would nonetheless have fitness value in the various environments in which vertebrates find themselves. "So, thinking of selection as an intentional process is one way to bring into play the counterfactuals that we need to make the distinctions that we need in order to individuate phenotypic traits." But, again, natural selection doesn't have intentions.

The authors helpfully suggest that maybe there are natural laws of selection. Laws certainly can support counterfactuals. I am entitled to claim that should I attempt to light my cigarette in the absence of oxygen, it would not light. That claim is supported by the law that oxygen is necessary for combustion. But laws aspire to generality, whereas the adaptiveness of phenotypic traits is context-dependent, say Fodor and Piattelli-Palmarini. It may be the case that in a given environment large creatures have the advantage, but "it simply isn't true, for example, that being big is in general better for fitness than being small." Because environments and conditions are always in flux, no trait will inevitably win in competition with other traits: Sometimes bigness is an advantage for a creature, sometimes smallness. Hence there are no laws of necessary connection that govern filtering by natural selection; hence counterfactuals cannot be supported; hence "selection for" cannot be the causal process by which species are altered over time.

In reading through all this, I was reminded of John Dewey, who began his philosophic career as a Hegelian but said he finally came to realize that a system of thought can be internally coherent and still be crazy. *What Darwin Got Wrong*, at least across the three parts, doesn't even have the virtue of being consistent. If "selection for" attributes to nature an intentionality that it cannot have, then "constraint on," the favored conception of the first part of the book, must also operate under the tainted assumption.

To constrain expression of either genetic or phenotypic variability means that, contrary to fact, if the constraint were removed, expression would have

a different pattern. But, as the authors insist, only intentional systems can be sensitive to contrary-to-fact conditions. Hence "constraint on" cannot be ascribed to nature as a property that helps explain transmutation—even though such ascription forms the conclusion of the first part of their book.

The concepts "selection for" and "constraint on" do indeed have intentional properties, because they express judgments made not by nature but by human beings—intentional judgments by biologists to the effect that in particular environmental situations certain features of traits are causally relevant. Quite routinely, for example, medical experts attribute the evolution of drug-resistant strains of bacteria to the excessive use of antibiotics in hospitals—or in cattle feedlots. No hospital workers or cattlemen intend to select for drug-resistant bacteria, although their actual intentions obviously play a causal role. Scientists understand quite well how selection operates in these instances; indeed, they are able to breed drug-resistant bacteria experimentally precisely in the way these organisms are selected for in the "wild," thereby confirming the natural selection of drug resistance.

Had Fodor and Piattelli-Palmarini read the first chapter of the *Origin*, they would have seen that Darwin argues there not so much that artificial selection is a model for natural selection as that it is exactly the same thing. Darwin regarded the breeder's intention, correctly I believe, as simply another environmental condition—one that rarely has a predictable outcome, as he discovered when he tried to breed fancy pigeons back to their original ancestral colors. Darwin thus directly demonstrated natural selection at work. And we do the same in the case of drug resistance.

There are, of course, many instances in which the biologist can only hazard a guess as to what is being selected for; but at other times, experiments and systematic observation can sift out the likely properties being selected. In the well-known case of sickle-cell anemia, researchers are fairly confident that the property of malarial resistance was selected for, although obviously not the free-riding trait of poor oxygen transport when the alleles are homozygous. These connections between traits and conditions may not have the canonical status of "laws of metaphysical necessity," but they do express well-founded generalizations (such as that vertebrates need oxygen to survive) that can indeed support counterfactuals. The second head of the authors' polemic hangs upside down by a thread.

In the third part of the book, the authors suggest that if evolutionary accounts of the transmutation of species cannot be scientific—because their supposed causal explanations are based on a vacuous concept—such accounts may be better regarded as historical narratives—that is, chains of "causally sufficient conditions," which are invoked ex post facto, to explain, for example, "why the dinosaurs became extinct." What else do accounts of natural selection purport to be but causally sufficient explanations of the evolution—or extinction—of species via adaptations or the failure thereof? There goes the third head.

The authors, in a denigrating mode, claim that a historical account cannot be supported by counterfactuals, as if evidence and generalizations were unknown to the historian. History and thus evolution are both, they say, "just one damned thing after another." Yet they concede that many historical narratives, that is, causally sufficient accounts, are "reasonable" and "plausible." Unless this is an utterly empty concession, they must allow what the historian takes for granted: namely, that he or she, on the basis of evidence and supported generalizations, can uphold the relevant counterfactuals—counterfactuals to the effect that if the significant antecedent conditions mentioned in the narrative had not occurred, neither would the event of interest, at least not in the form that it did. If the historian could not defend such counterfactuals, then it would be impossible to assess his or her narrative as "reasonable" or "plausible."

Historical accounts depend on evidence and generalizations derived from observation, and evolutionary explanations have the added advantage of experiment. These well-honed techniques of inquiry form the basis for the kinds of discriminations that Fodor and Piattelli-Palmarini both deny and grant the laborers in these fields. The authors thus orchestrate a medley of contradictions that can delight only the ears of creationists and proponents of intelligent design.

In the legendary meeting of the British Association in 1860, Bishop Samuel Wilberforce attacked the Darwinian defender Thomas Henry Huxley with this barb: Did Mr. Huxley claim his descent from a monkey through his grandmother's or his grandfather's side? Huxley reputedly whispered to a friend: "The Lord has delivered him into my hands." Huxley retorted that he would rather have a monkey as his ancestor than be connected with a man who used great gifts to obscure the truth.

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POLICY

Weathering Nuclear War

Sean L. Malloy

A NUCLEAR WINTER'S TALE: Science and Politics in the 1980s. Lawrence Badash. xvi + 403 pp. The MIT Press, 2009. $40.

A Nuclear Winter's Tale, by Lawrence Badash, is an intricately detailed history of a subject that, by the author's own admission, never managed to capture the sustained attention of the media, the public or policy makers—even at the height of its visibility in the early to mid-1980s. Thus it is not the sort of book the average educated reader, even one interested in nuclear weapons or the history of science, is likely to pick up. Scientists and historians of science, meanwhile, will likely be wary of the amount of space that Badash devotes to the bureaucratic and organizational wrangling over the subject of nuclear winter. Any book that contains chapters titled "Report after Report" and

