

The Scope of Natural Selection

The author replies to H. Allen Orr's review, "Dennett's Strange Idea" (*Boston Review*, Summer 1996).

Daniel C. Dennett

When Professor Orr published his hostile review of *Darwin's Dangerous Idea* in the biology journal *Evolution* last February, I was not pleased. His view was full of falsehoods and misconstruals, but I had no recourse; that journal, like most academic journals, does not permit authors to respond to reviews. Luckily for me, it was so eager to warn the world of my errors that he restated his attack, with embellishments, in *Boston Review*, which has invited me to respond. Months have passed, the damage has been done, but at least I get to set the record straight.

I am grateful to Orr for embellishing his attack for the benefit of his lay readers, since these additions vividly expose his own errors and confusion, which were somewhat masked in the more professional version. In what follows I will concentrate on his criticism of my understanding of his field: biology. I trust that the readers of *Boston Review* have seen the flaws in his philosophical arguments without my help, but a non-biologist might well suspect that, on his home ground, Orr's complaints are as authoritative and devastating as he makes them out to be. They are not.

Before getting to the meat of his criticism, Orr warns readers that my book is:

... with factual errors, some scientific and some historical. Population genetic theory, for instance, does not prove that evolution by random change is faster than evolution by natural selection, as Dennett claims.

... did indeed misspeak (p. 126), but the result is ambiguity, not error. The issue is complicated: it depends on whether you're measuring the (average) speed of departure from starting point in genetic space, or the speed of attainment of some particular evolutionary product. I meant the former, as the context ought to have made clear (I was in the process of observing that there has been plenty of time in evolutionary history for the sheer genetic diversity observed to accumulate). If I did couch my claim in terms that permitted Orr to put the false reading on it. Score the half point for Orr. He goes on:

And it was Darwin's theory of sexual, not natural, selection that he called an "awful stretcher."

I made no such claim. I entitled a chapter section "Natural Selection—an Awful Stretcher." The vivid epithet comes from Darwin, but it was I who applied it to natural selection. Orr, having already stretched too far his effort to find an historical error to impute to me, topples over when he tries to elaborate Darwin branding natural selection as "about as plausible as Chomsky and Universal Grammar as a howler." What? Darwin branded sexual selection an awful stretcher in the very act of endorsing it—it is, after all, a special kind of natural

selection, not an alternative! Here's what Darwin said (as I noted, when I introduced the term): "It is an awful stretcher to believe that a peacock's tail was thus formed; but, believing it, I believe in the same principle somewhat modified applied to man." Surely Darwin would have agreed, if asked, that natural selection was another awful stretcher he believed in. Orr's remark about Chomsky makes it appear that he thinks Darwin was dismissing sexual selection when he called it an awful stretcher, but I suspect he knows better and simply got carried away by his desire to rub it in about my nonexistent error.

These are the only two "factual errors" he comes up with, and he judiciously grants them the status of "peccadilloes." There are some factual errors in my book that others have pointed out, but since Orr does not brandish any of them in his essay, I think we can conclude that they eluded his scrutiny. Let's turn, then, to his main concern: my book's alleged deep misunderstandings of biology.

Orr concentrates on my criticism of Gould and Lewontin's famous attack on adaptationism, "The spandrels of San Marco and the Panglossian paradigm." Evolution—what Darwin called "descent with modification"—is an uncontroversial fact, as secure as any in science, but there is still controversy among biologists about just how many facts about evolution are explained by natural selection, the mechanism Darwin proposed as the most important force shaping evolution. To see what the issue is, consider the popular retort of software engineers when somebody finds a flaw in their program: "It isn't a bug, it's a feature." In other words, it's supposed to be that way; it was designed to work like that. Now is everything observable in the biosphere a "feature," an "adaptation," as an evolutionary biologist would call it? Are there no bugs, no undesigned bits, no historical accidents? Of course there are. Everybody agrees on that. Everybody does not agree on how important these non-adaptations are: the greater the role you give to natural selection, the more "adaptationist" you are, and the Gould/Lewontin essay was an attempt to swing opinion the other way.

Orr begins by usefully perpetuating some of the misunderstandings I exposed in my discussion of Gould and Lewontin's central architectural metaphor for a non-feature: a "spandrel"—one of the curving wedges of masonry that serve as the transition from the domes of the Basilica di San Marco in Venice to the arches that hold them up. (I find this useful because some critics have wondered if any biologists have actually been confused on this score; my answer is yes—see, e.g., H. Allen Orr.) This is what he says:

Although spandrels are often decked out with mosaics, no one would seriously argue that spandrels are there because they provide such swell surfaces for mosaics. Instead, spandrels are there because they have to be—they are, it turns out, the inevitable

by-product of putting a dome on rounded arches.

"Inevitable by-product"? This is either flat false or true but irrelevant. If by "spandrel" Gould and Lewontin mean the particular structure used in San Marco (properly called a *pendentive*) then what they say is false; pendentives are one of many options, but they are probably the optimal engineering solution to the problem of supporting the dome—what I would call a Forced Move. In this sense, spandrels (pendentives) are adaptations *par excellence*. If, on the other hand, Gould and Lewontin mean by "spandrel" just "whatever you put in that place between the dome and the arches," then spandrels are trivially inevitable—you have to put *something* there. Gould himself has recently¹ opted for this reading, but as I had already pointed out in my book, pp. 272-3, this interpretation also disqualifies spandrels for their role as lead metaphor in Gould and Lewontin's biological argument. In this sense, architectural constraints present a problem, not a solution, in biology as much as in building, and that is where natural selection comes in.

This has been much discussed recently, and the architectural engineer Robert Mark, in a fascinating article in the July issue of *American Scientist*, shows that my own amateur engineering (and architectural history) led me to underestimate the structural requirements of putting a large dome on arches. The pendentives are in fact not just aesthetic adaptations, as I had claimed, but also structural adaptations. So the San Marco "spandrels" are a doubly poor choice for eponymous non-adaptations. The mosaics on the pendentives are aesthetic design choices enabled by prior structural design choices. An example of one adaptation exploiting a prior adaptation hardly strikes a telling blow against adaptationism.

But why are we quibbling about these fine points of architectural history? Why, indeed, do I pay such obsessive attention to Gould and Lewontin's attack on adaptationism? Orr expresses his bafflement:

Why, after all, should a man hoping to export Darwinism from biology be so obsessed with defending the minutiae of adaptive storytelling within biology? I would not think the legitimacy of, say, cultural Darwinism hangs critically on whether selection is very important in biological evolution (as everyone, including Gould and Lewontin, believe) or is almost exclusively important (as Dennett believes).

First, Orr strains to create a gulf that does not exist between me and the run of biologists. What, we may ask, is the difference between "very important" and "almost ex-

¹After an address at Dartmouth, in April 1996, in response to a question from me.

clusively important"—the presumably dissenting view he attributes to me? It is Gould and Lewontin, in fact, who have had trouble clarifying their charge. Everybody agrees, as Orr says, that selection is very important, so just what are Gould and Lewontin saying? That it is not as important as some people think? If that had been their message, I would agree with John Maynard Smith that their paper was "on the whole, welcome." Orr quotes this verdict with approval, and so did I (p. 278), noting, however, that this was not their message—or at least it has not been Gould's version of their message.

I think Orr's professed perplexity must be disingenuous. I make it crystal clear why I have to go to all this trouble clarifying these minutiae: Gould has persistently misrepresented the import of the Gould/Lewontin paper outside biology, and many have been taken in. My task was to show the non-biologists that they have been seriously misled by Gould about this. In my book I list four propositions that are widely believed by non-biologists to have been demonstrated by Gould. The first two are relevant here:

If you believe: 1) that adaptationism has been refuted or relegated to a minor role in evolutionary biology, or ... (2) that since adaptationism is 'the central intellectual flaw of sociobiology' (Gould, 1993a, p. 319), sociobiology has been utterly discredited as a scientific discipline ... then what you believe is a falsehood. (p. 265)

Well, are these truths or falsehoods? They *are* widely believed. Many non-biologists are under the weird misapprehension, thanks to Gould's rhetoric, that one is under no obligation to provide an adaptive account of the evolution of a complex competence or organ—such as human language. In some misguided quarters, indeed, adaptationist explanations of anything are automatically suspect! For more than a year before the publication of my book and on several occasions since then, I have repeatedly requested that he clarify his position on these propositions. Steve Gould is undeniably a Great Communicator. If these propositions are not what he meant, if over-eager readers have misunderstood him, he should find it both obligatory and easy to correct these widespread misapprehensions. If he meant them, he should either defend them against my charge that they are false, or concede that he has misled his readers. He has not accepted my invitation to clarify his position, so it falls to me to explain to the world, at whatever length it takes, why these are *not* the take-home messages from Gould and Lewontin's article.

The centerpiece of Orr's critique of my defense of adaptationism is his feigned astonishment that I omit any discussion of Motoo Kimura's neutral theory, "the most serious and famous of all challenges to selectionist story-telling." Here he plays a cute rhetorical trick: Boy oh boy, Dennett must be really out

of it to have overlooked Kimura's work! Why on earth did he omit this? Was it because he never heard of it? Naw. Or because Kimura's book was too difficult for him to read? Naw. Or because he "doesn't want to let the cat out of the bag" about the fact that biologists have found out that "non-Darwinian" evolution is common? Naw. Having relieved himself of these snide but indefensible suggestions—as he himself so generously acknowledges—he arrives at the obvious truth: "Dennett is interested in Design . . . and the neutralist/selectionist controversy has nothing to say here." Right. It wasn't relevant. Kimura's theory is about *typographical* change, visible at the molecular level of the genome. As Orr says, "most evolution *at the molecular level* [my emphasis] is *not* caused by natural selection, but by 'genetic drift.'" Before Kimura, theorists hugely underestimated the possible role of the accumulation of random mutations that are (as Orr notes) functionally equivalent. Kimura's work is wonderful, surprising, and still controversial; it claims (among other things) that the diversity observed at the molecular level could be much more the result of random genetic drift than many theorists—"selectionists"—had thought, but this aspect of selectionism has little or nothing to do with adaptationism, which is why I left it out. Oh, I could have had some fun with the fact that there are indeed some biologists—Gould and Lewontin come to mind—who have often suggested to the lay public that Kimura's neutral theory is a rival to natural selection as an account of morphologic evolution (the shaping of complex phenotypic features), but as Orr as much as admits, this is not taken seriously among biologists.

From all Orr's hooting one might get the impression, by the way, that I had nothing to say on these topics in my book; but in fact, I discuss the role of random genetic drift (pp. 125-6) and the ubiquitous possibility of typographically different but functionally equivalent genetic recipes for proteins (pp. 195-6, p. 287). Where Orr mentions functionally irrelevant differences in varieties of hemoglobin, I mention functionally irrelevant differences in lysozyme. Same points made, right down the line. In fact I also make all the other points about adaptation he goes on to explain for his lay readers, saying "Dennett never confronts these legitimate worries." (All of them? Yes. If you want to test my claim, you can go back and match page 30 of Orr's review with my pages 199, 248-249; I'll spare readers the tedious details.)

One last point about Orr's understanding of central concepts in his own field before we turn briefly to the evolution of culture. I say that natural selection is an algorithmic process, and hence is "substrate neutral"—it doesn't matter what material the algorithm is executed in, so long as the recipe is followed. Orr says, correctly:

This substrate neutrality argument is supremely important to Dennett. It—and nothing else—explains why selection can be lifted from its historical base in biology. It is what makes Darwinism so dangerous.

He goes on: "But Dennett slips here." It is Orr who has slipped, falling flat on his face with his proposed counterexample to my claim that natural selection is a substrate neutral process: "blending inheritance." Orr says:

Evolution would quickly grind to a halt, for instance, if inheritance were blending, not particulate. With blending inheritance, the genetic material from two parents seamlessly blends together like different colored paints. With particulate Mendelian inheritance, genes from Mom and Dad remain forever distinct in Juniors. This substrate problem [*sic*] was so acute that turn-of-the-century biologists—all fans of blending inheritance—concluded that Darwinism just can't work. Modern evolutionary genetics was born in 1930 when Sir Ronald Fisher cracked this problem: Population genetics shows that particulate Mendelian inheritance saves the day. It is just the kind of substrate [*sic*] needed for evolution by natural selection to work.

Orr is right that Mendel saved the day for Darwin (a point of history I discuss in somewhat greater detail in my book), but this was a triumph for substrate neutrality! Mendel—and Fisher, too, for that matter—was clueless about what the substrate was. (It's DNA, of course, but that wasn't discovered until 1953, by Crick and Watson.) *Mendelian* genes are a paradigm case of substrate neutrality; they are pure data structures, whose material composition is irrelevant just so long as they obey the combinatorial rules Mendel laid down. The field of population genetics thrives to this day in almost complete independence of any concern with the nitry-gritty biochemistry of the substrate—just the way software engineering is conducted by people who need know little about the electronics or physics of their silicon substrate. The problem with blending inheritance is the code, not the substrate; it would be just as hopeless a system to use in evolutionary software engineering as it would be in carbon-based biological evolution.

Orr has apparently missed this fundamental point about the abstract nature of evolutionary theory, and having missed it, he compounds his confusion when he turns to my exploitation of Richard Dawkins' concept of memes in defense of a Darwinian theory of cultural evolution. His "basic problem" with any attempt at such a theory is that we are "very ignorant of how humans hold ideas in their heads. . . . So how can we possibly conclude that the process 'must be Darwinian?'" Well, population geneticists were very ignorant of how organisms held genes in their bodies until recently, but this did not stop Fisher and his colleagues from establishing (in a substrate neutral way) the soundness of the modern synthesis, did it? Orr comes close to contradicting himself here, and doesn't notice.

But then his whole discussion of memes is inattentive to detail. Orr says: "The fitness of memes is strangely tautological. While we can often point to ecological reasons why certain genes are fitter than others, a meme is deemed 'fit' only because it is common." This is obviously false, belied by many of the examples discussed in my book. He goes on: "'Elvis is alive' is certainly a fit meme, but it is neither true nor helpful." Has he been paying attention at all? This independence of meme fitness from truth and utility (to us) is the single most important feature of the meme-eye view of cultural evolution. He continues: "Last, Dennett confesses [*sic*] that memes often show a *Lamarckian*, not Darwinian, style of

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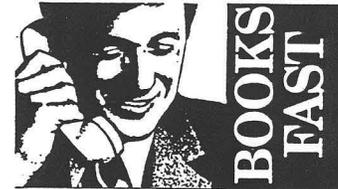
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evolution, in that acquired traits get passed on," ignoring the fact that I explain why the "charge" that cultural evolution is Lamarckian misses the mark entirely (p. 355).

Given this obliviousness, his "astonishment" that I would point out how difficult—and maybe impossible—it will be to establish a predictively powerful science of memetics counts for little or nothing. There are many uncontroversially evolutionary phenomena in biology that also defy formal scientific inquiry by being either computationally intractable or bereft of accessible data. Ancient memes, like ancient genes for soft body parts, leave almost no fossil traces. The genetic contributions to many complex abilities in many species are both practically impossible to research and utterly indubitable. For instance, does Orr doubt that there is an evolutionary account—probably forever inaccessible, alas—of the nest-building behaviors in birds? He interprets my cautious discussion of the prospects for memetics as "backing off" from stronger claims about memes, but this retreat is a figment of his imagination. (Aficionados will recognize this trick as an instance of the Gould two-step, which I exposed several years ago in the *New York Review of Books*.)

Orr's other criticisms of memes follow the same pattern as his earlier criticisms of my biology. Once again, he reiterates points I make myself about important problems to be solved, but he treats these problems as clearly insoluble, without giving any arguments or acknowledging that he is following me. He claims that I have overlooked—or deliberately suppressed—trenchant criticisms

of memetics from within biology, but he doesn't mention any. I deal with all the criticisms that I have encountered in the literature. If he knows of others, he should have referred to them specifically.

One final point: Orr's failure to understand substrate neutrality leads him to miss the otherwise obvious point that in any theoretical analysis of the evolution of a cultural phenomenon—say, morality—there can be complex interplay between genetic and memetic sources of design; substrate neutrality means that the work of evolution can be accomplished in either medium (at different rates, under the same or different selection pressures) and combined in the final product. He apparently thinks that any account of the evolution of morality must either be all genetic or all cultural. He asks, at one point: "Is it obvious that genetic changes are required for such a thing? [No. It is false.] Where are Dennett's trusty memes when we need them? [Right under his nose.]" Orr might want to look at some of the literature I cited on these topics, such as the recent theoretical work by Philip Kitcher, an alumnus of Lewontin's lab. It's substrate neutral, all the way.

I will not comment on Orr's critical treatment of the more purely philosophical parts of my book, beyond noting that the charitable interpretation would be that he doesn't understand the difference between serious criticism and the mug's game of quoting out of context. (Those who are curious may consider as Exhibit A his abuse of my discussion of "bait-and-switch" in evolutionary theory.)

One does not lightly undertake the task of dislodging heroes from their pedestals so that their ideas can be critically assessed in the same arena with the ideas of ordinary mortals. So I expected to be treated fairly roughly by their fans, especially in their home town. Gould and Lewontin and Chomsky have so far all chosen to leave the counter-attack to others, my criticisms being too far

beneath their notice, one gathers, to merit any detailed public response. The attacks I have seen to date—of which Orr's is the best, by the way—have been long on sneering and short on substance. It's been surprisingly easy to take, since my task has been far from thankless. Indeed, the thanks I have been receiving from biologists around the world has been most gratifying. ■

H. Allen Orr Replies

If you're alarmed by Daniel Dennett's vitriolic style, let me reassure you: *He's not that mad.* (To see the truly splenetic Dennett, check out his response to John Searle in the December 21, 1995 *New York Review of Books*.) And, looking past the bluff and bluster, it's not hard to find what annoys Dennett: His real targets—Gould, Lewontin and Chomsky—have thoroughly ignored him. Alas, he is reduced to responding to obscure young upstarts—a fact that seems to trouble him to no end given that half his response is bizarrely directed not at me, but at Stephen Jay Gould. As Gould is a big boy and can handle his own fisticuffs, I'll restrict myself to Dennett's critique of my ideas, not Gould's.

Dennett brands my review "hostile," and I suppose it was. There were two reasons. First, I think Dennett's chief claim—that natural selection may explain everything

from the value of physical constants to the vagaries of cultural change—is wrong. Second, I think that Dennett all too often misleads and manipulates his readers. Because it's easy to lose sight, in the thicket of his legalistic charges, of the real issues that separate us, let me touch on these two key points before I answer his charges.

Our fundamental disagreement is simple: Dennett sees natural selection everywhere—in cosmology, in the spread of songs, and in the demise of architectural styles. Natural selection is the one Big Idea that explains, if not everything, next to everything. And I think this is silly. Natural selection explains a tremendous amount of biology. And, undoubtedly, it will occasionally explain facts outside of biology. But I cannot understand the nearly religious zeal that drives Dennett to conclude that

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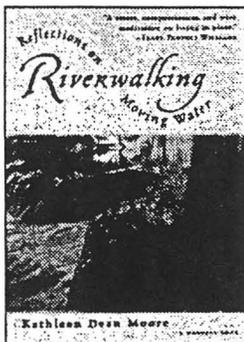
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natural selection is the cardinal force governing the ebb and flow of every substance—from quarks to consciousness—in the universe. Indeed I find the notion that natural selection has much to tell us about, say, the origins and fate of political movements downright bizarre. It is, of course, an occupational hazard of the intellectual trade to think that there is some one great “-ism” that encompasses everything under the sun (take your class struggle, sex, natural selection). But the voice of experience suggests that the world is not so tidy. And, as I’ll argue below, the voice of science further suggests that Dennett’s attempt to stamp everything from Planck’s constant to Plato’s *Republic* with Darwinism is flawed.

As for Dennett’s tendency to manipulate readers, I offered two examples: his infamous bait-and-switch (promising revolutions—*Consciousness Explained! Universal Acid!*—and delivering cabinet shuffles, a trick Searle also picked up on), and his tendency to brow-beat those in the humanities with scientific claims couched in fancy language. So, for instance, if you don’t buy his “population memetics” explanation of cultural change, it’s not because the idea is silly. It’s because you’re a mushy “Darwindreader” who’s terrified of science. But we needn’t look to Dennett’s book to find such tactics. His response is loaded with dubious maneuvers. For instance, anyone who doubted Dennett’s ability to issue pseudo-scientific bluster should consider my own exhibit A: Dennett’s new explanation of what he meant when he said (incorrectly) that evolution by random change is faster than that by natural selection. He now tells us:

I did indeed misspeak (p. 126), but the result was ambiguity, not error. The issue is complicated: it depends on whether you’re measuring the (average) speed of departure from a starting point in genetic space, or the speed of attainment of some particular evolutionary product. I meant the former.

Now I’ve been in the population genetics business for some time and, frankly, I have no idea what Dennett is talking about. And—I can find no polite way of putting this—it’s hard to escape the conclusion that Dennett has no idea what he’s talking about, either. Even the most charitable interpretation I can come up with is just plain wrong.

But this is minor league stuff compared to exhibit B: Dennett’s presentation of yet another exegesis of Gould and Lewontin’s spandrels metaphor. Although this is the part of his *jihad* where he mistook me for Gould, it provides such a good example of Dennett’s evasions and obfuscations that it deserves close scrutiny. Here’s the chronology:

1) Gould and Lewontin say that spandrels are “necessary architectural by-products of mounting a dome on rounded arches.” Although spandrels get decked out with elaborate mosaics, it’s silly to think that spandrels were designed *in order* to show off mosaics: “[T]his would invert the proper path of analysis. The system begins with an architectural constraint.”

2) Dennett doesn’t buy it. He claims that there are “indefinitely many ways that those spaces could be filled with masonry, all of them about equal in structural soundness.” Remarkably, he concludes that Gould and Lewontin have it all backwards—spandrels are there because they’re the best way to display your mosa-

ic collection: “The conclusion is inescapable [sic]: the spandrels of San Marco . . . are adaptations, chosen . . . for largely aesthetic reasons. They were *designed* to have the shape they have precisely in order to provide suitable surfaces for the display of Christian iconography.” To prove his point, he sketches a few spandrel-less “alternatives.”

3) An engineer, Robert Mark, shows that Dennett is wrong. He slaps Gould and Lewontin on the wrist for their diction (they should have said “pendentive” not “spandrel”), but concludes: “Dennett’s critique of the architectural basis of the analogy goes even further astray because he slights the technical rationale of the [pendentives] . . . his treatment of crucial structural elements as a kind of surface decoration that can be altered at will . . . ignores the . . . centuries of construction experience that led to their incorporation.” He concludes that, for large structures, pendentives “are necessary structural elements” and that Dennett’s alternatives might well collapse.

So what does Professor Dennett conclude? That Gould and Lewontin were more or less right and he was more or less wrong? Not by a long shot. In a breathtaking display of *chutzpah*, Dennett concludes that, since they’re structurally important, spandrels were “a doubly poor choice” for Gould and Lewontin! Heads I win, tails you lose! In case you’re confused (and you should be), I urge you to re-read

the above quotes, or, better yet, to read Mark’s article.

This example shows off all the less savory aspects of Dennett’s *modus operandi*: His intemperate drive to prove his opponents wrong, no matter what the record says (no wonder they ignore him). His ability to duck and weave in such a way that, while admitting an error, he magically emerges victorious and those who were right are found “doubly” wrong. And an ability to pull the whole thing off with such bravura that the reader naturally falls in behind him, certain that Gould, Lewontin, and, yes, H. Allen Orr, were deeply confused about those darned spandrels.

But enough about Dennett’s methods. What of the substance of his response? It’s worth noting, first of all, that I agree with several of Dennett’s pronouncements. I agree that biological evolution is an uncontroversial fact. I agree that natural selection is the most important force driving this evolution. I agree that anyone who claims that adaptive thinking “has been refuted or relegated to a minor role in evolutionary biology” is wrong. Natural selection is alive and well.

But what does this have to do with my review? My charge was not that natural selection is dead, dying, or even collecting a pension. My charge was that Dennett misrepresents biologists’ *real* and legitimate worries about adaptationism. Dennett’s response is remarkably silent here. My claim was

simple: Biologists don’t get jittery about adaptationism because we fall for some alternative cause of biological “design,” but because, sometimes, we’re just not sure that a feature is designed (by natural selection, that is).

One cause of our uncertainty is “neutrality”—the possibility that the biological differences we see don’t affect fitness much one way or the other. In his response, Dennett tries to justify his failure to tell his readers about the neutral theory. He suggests that neutrality can be ignored since neutral changes are just “typographical change, visible at the molecular level of the genome.” This is simply wrong. Neutral changes might well affect the way an organism looks, its “phenotype” (has Dennett confused “neutral” with “silent”?). This was perfectly clear from the examples I gave: e.g., when Dobzhansky wondered if flower color differences in *Linanthus* are neutral, he was talking about the way an organism looks, not about its DNA.

But all this neutrality talk is misleading. As I made painfully clear, there are other—and surely more important—reasons for doubting we should spin adaptive yarns about every bump and wrinkle on an organism. (Here’s one: you just can’t optimize a zillion traits at once.) Dennett, in his response, plainly admits the existence of such non-adaptive traits. Which leads us to the big question: If he agrees non-adaptive traits are out there, why does he find biologists’ worries about adaptationism a national emergency, calling, one would guess from the tone of his book, for public floggings?

In any case, Dennett completely ignores my biggest worry about adaptationism: Adaptationist culture encourages wild story-telling just where Design is *least* obvious. The problem is sociology: nobody ever got famous for speculating on why birds have wings (“So they can fly?”). The road to glory instead demands ingenious stories that are far from obvious, and the whole business can degenerate into a display of cleverness. Ironically, Dennett’s response provides a superb example of this problem. Now that he’s told us it’s wrong, consider this hypothesis: Spandrels were “designed to have the shape they have precisely in order to provide suitable surfaces for the display of Christian iconography.” This, I submit, is a *perfect* example of the peculiar excesses encouraged by adaptationism. If you want to understand why biologists—while loving our natural selection—worry over adaptationism, study this example. Note its features: flatly implausible (a guy hoping to hold up a 42 foot dome is worried about *mosaic*?), but very cleverly argued. And just wild enough to turn heads. A more sober hypothesis (spandrels are, say, the cheapest way to do the job) would have a far better chance of being right. But, alas, such a hypothesis wouldn’t make much of a splash. Exactly the same dynamic occurs in biology with exactly the same result: a big waste of time.

What of Dennett’s comments on cultural evolution—memetics? (“Population memetics” is Dennett’s attempt to build a Darwinian science that explains cultural change.) Dennett says my main problem with memetics is that we’re “very ignorant of how humans hold ideas in their heads. . . . So how can we possibly conclude that the process ‘must be’ Darwinian?” That is, in fact, half of my problem with memetics and, frankly, it’s the kinder, gentler half. As I made clear, my other problem is that what we *do* know about how humans hold ideas in their heads suggests that

Again, From the Beginning

Love hangs a sign
written in huge letters
but your mouth won’t form the words.
The right places never held meaning
and though you knew life
wasn’t art, you wanted it to be:
the confessional too easy a box
to enter and leave or East
becoming solidly *East* because the map said so.
Not understanding
but acquiescence to form. Flowers on a tomb.
The cars passing and then gone
along the avenues suggested a moral
equation you had to follow
to know. The yellow hills made you lose
heart, but the sounds of pity
heard through the intercom in the broken
tongue of your city!
You’ll never translate them,
but you are tracing
their shapes in the palms of hands.
You are recognizing their signature
on the calligraphed backs
along the interstate
and in the meaningless body America
promises at each exit.
You cannot get home.
You have not done your job.

—Claudia Keelan

"memes"—ideas, songs, fashions—aren't anything like genes. And if memes aren't sufficiently gene-like, we have little reason for thinking that "concepts from population genetics transfer quite smoothly" to population memetics, as Dennett hopes.

So does Dennett believe that memes are like genes? He admits: 1) Memes are produced by "directed mutation," while genes are produced by random mutation; 2) exchange between long-isolated cultures has everything to do with cultural evolution, while exchange between long-isolated species can't happen; 3) memes can blend together, while genes don't; 4) memes show a Lamarckian style of evolution, whereas genes show only Darwinian evolution. By the end of this list, one begins to suspect that the most important feature memes and genes share is the sound of the words. This does not, of course, mean that no sort of theory of cultural change is possible. But it *does* mean that

Dennett's memetics—founded on a strict meme-gene analogy—is in a bad way.

Dennett also claims that I botched my "substrate neutrality" objection to memetics. Dennett's central claim, in his book, is that because natural selection doesn't care what material it works on—because it's "substrate neutral"—it can be lifted from biology and used to explain everything from the origin of the universe to the evolution of musical styles. I pointed out that biological evolution works only because the hereditary substrate behaves in a special way—genes don't blend. If a substrate doesn't behave in this way—if the evolving units blur and blend together—Darwinism may simply not work. So if memes blend, Dennett's got problems.

There are two interesting things about Dennett's answer. First, he concludes that my objection involves "code," not "substrate," problems. It does not, after all, matter that

genes are made of DNA and not plastic—selection would work either way. Fine. But so what? My point remains: Some substrates behave in such a way that Darwinism clearly works (particulate), and some don't (blending). If Dennett wants to call this a code problem, I have nothing against this diagnosis. But I suspect that his obstreperous outburst ("It is Orr who has slipped, falling flat on his face," etc.) is intended to make you miss this point: Whatever you call the disease, the patient is still dead. If memes blend, *memetics* falls flat on its face. The second interesting thing about this blending objection is that I'm not the first to make it. I find this a bit disappointing as I rather liked it, but I now find that Dawkins also noted that memes, unlike genes, blend. As far as I know, Dennett has not assailed Dawkins for recognizing this sad fact.

I did not mean to imply that Dennett blithely ignored criticisms of memetics from

biology—he did not. What I *did* mean to imply was that, by downplaying the severity of these problems, he misled readers about why memetics never caught on. I said: "He would have the naive reader believe that memetics was shot down by soft-headed humanists ('Darwin-dreaders') who panicked when facing the encroachment of science." Is this fair? Here's what Dennett said in his book:

I suggest that the meme's-eye view of what happened to the meme meme is quite obvious: 'humanist' minds have set up a particularly aggressive set of filters against memes coming from 'sociobiology,' and once Dawkins was identified as a sociobiologist, this almost guaranteed rejection of whatever this interloper had to say about culture—not for good reasons, but just in a sort of immunological rejection.

This is nonsense. There are two reasons why the "meme meme" never caught on, and both are perfectly good: 1) Scientists saw that it's plagued by the problems I listed above and haven't given it a second thought in ten years. 2) Humanists found that the meme perspective didn't *do* anything for them: Where are the previously baffling patterns in the history of music or of politics that a meme's-eye view suddenly explained? I know of none and Dennett tells us of none. Has Dennett ever wondered why Darwin's ideas—which pulled the rug out from under cherished humanist ideals far more violently than any talk of memes could—caught on, while memetics did not? Even in an age of postmodernist babble the answer is surely obvious: Darwin's ideas *worked*. They made sense of a staggering number of previously puzzling patterns and gave order to thousands of once disconnected facts. Memetics did—nothing.

Last, a remark or two on Dennett's astonishing claim that the troubles facing memetics are, after all, much like those facing any other science. We biologists, for instance, may never be able to give an "evolutionary account . . . of the nest-building behaviors in birds." I've read this remarkable passage over and over and each time I reach the same depressing conclusion: Dennett really thinks these situations are similar. Apparently, he can't distinguish between a science admitting it faces unsolved problems and a field admitting its problem is that it's *not* a science. The difference is, of course, profound. For one thing, a field that admits it's no science must give up its absurd claim that it has united cosmology, biology, and culture, indeed "[l]ife and all its glories . . . under a single perspective" of Darwinism. For another, a field that admits it's no science must give up bullying humanists by saying, "But if you weren't so terrified of science, you'd see that. . . ."

For my money, those mushy Darwin-dreaders have far more to say about the pulse of cultural change than does Dennett and his memetics. If you want to understand, say, the rise and fall of fascism, I'd suggest you learn about the International Brigade or Winston Churchill. I'm not sure much is gained by talking loudly of the "war meme" or the "Churchill meme." ■

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Evolution is henceforth the magic word by which we shall solve all the riddles that surround us.

— Ernst Haeckel¹

A reviewer has a duty to take a book's intended audience into account. A picture-book of dinosaurs for children and a hefty tome on paleontology should and must be different. I bring this up because the market for Daniel Dennett's new book, *Darwin's Dangerous Idea*, is, at first, unclear: Is this pop science or a serious attempt to think new thoughts? The question matters because the verdict changes with the audience. As a work of popular science (with a healthy dose of philosophy thrown in), Dennett's book is a modest success. Although he lacks the graceful prose of a Gould or Dawkins, Dennett delivers a thoughtful and provocative book, and many of his readers will surely dig deeper into evolution as a result. Because Dennett's book certainly sports many of the trappings of pop science (e.g., Cute New Names for every old idea), my review might have ended here, favorably.

But Dennett is not in the edutainment business. Near the beginning of his book, he tells us: "I want to play a more direct role in changing what is ignorable by whom. I want to get thinkers in other disciplines to take evolutionary theory seriously, to show them how they have been underestimating it, and to show them why they have been listening to the wrong sirens." In short, Dennett wants to talk to the pros. And it is here — in his attempt to change the way intellectuals, both scientists and humanists, think about Darwinism — that his book fails.

Dennett's thesis is simple enough. He is convinced that intellectuals underestimate the explanatory power of evolutionary theory, which to Dennett means natural selection. He is sure that natural selection is both more potent within biology than many biologists believe and more relevant to problems outside biology than many social scientists and philosophers pretend. In an analogy that runs through the book, Dennett likens Darwinism to a "universal acid," an allusion to childhood lore about an acid so corrosive that it eats through everything — including the jar in which you desperately try to contain it. According to Dennett, the universal acid of natural selection can spread both *downward* from biology, explaining the origins of the universe and life, and *upward* from biology, overturning our views of consciousness, cultural change, and the origin of morality. The resulting Darwinian Science

of Everything "eats through just about every concept, and leaves in its wake a revolutionized world-view." Dennett takes this Science of Everything idea very seriously: "The idea of evolution by natural selection unifies the realm of life, meaning and purpose with the realm of space and time, cause and effect, mechanism and physical law."

Lewontin and Stephen Jay Gould, and the philosophers John Searle and Jerry Fodor. Remarkably enough, Dennett thinks he has found a common anti-Darwinian sin that binds these men together. In one way or another, Dennett argues, they each invoke a "skyhook" — a miraculous force or process that gets snuck in the back door to

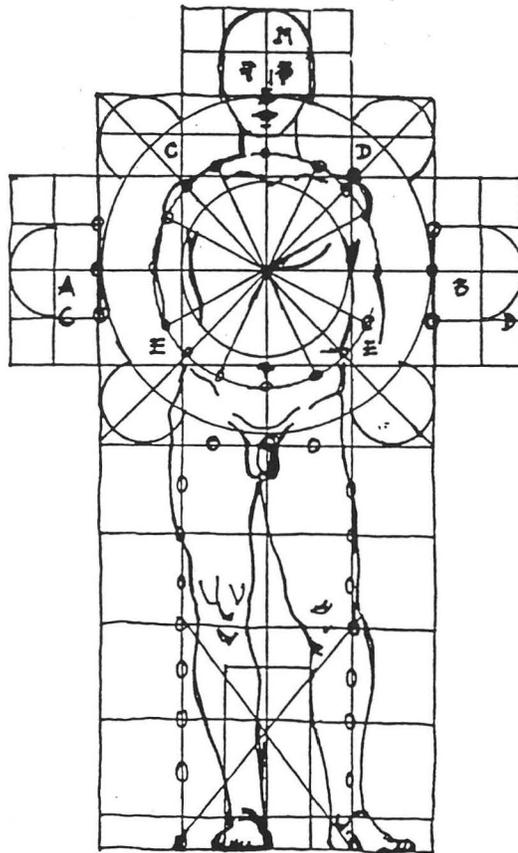
Dennett is an accomplished philosopher who has thought long and hard about these problems. His arguments are sophisticated and are often fleshed out in erudite detail. And judging from the rave reviews on the jacket-cover, he has won over several leading evolutionary biologists. Dennett cannot be dismissed lightly.

That said, I cannot join in the chorus of praise. Dennett's book suffers from a number of problems. The most obvious has nothing to do with the soundness of his arguments, but with how they are tendered — the book suffers from an annoying stylistic inconsistency. Although many sections are written in a breezy conversational style, the reader, with an almost audible thud, will plow into a technical section laden with alien jargon. Similarly, Dennett will introduce some cute idea that, it seems, will play an important role in his story, e.g., the "Cosmic Pyramid." But that ethereal Pyramid is barely mentioned again and the reasons for its invention and debut are frustratingly unclear. I later discovered one cause of this choppiness: Dennett is not fond of re-working an idea he's once committed to paper. As a result, a ten page section from one of his previous publications in, say, the *Journal of Aesthetics and Art Criticism* will be plopped down nearly verbatim in his book (indeed that particular passage made yet a *third* appearance in another of his books, *Consciousness Explained*). A surprising portion of Dennett's book consists of snippets from previous papers stapled together. The result is not seamless prose.

The book is also marred by factual errors, some scientific and some historical. Population genetic theory, for instance, does not prove that evolution by random change is faster than evolution by natural selection, as Dennett claims. And it was Darwin's theory of sexual, not natural, selection that he called an "awful stretcher." (Darwin branding natural selection a stretcher is about as plausible as Chomsky branding Universal Grammar a real howler.) But these are peccadilloes and do not touch on Dennett's main claims. Unfortunately, Dennett has many main claims and we cannot consider them all. I will focus here on two: the notion that natural selection is more powerful within biology than biologists often claim; and the notion that Darwinism can diffuse out from biology, throwing light on the evolution of cultures and the origin of morality.

Adaptationism

Dennett is deeply troubled by what he sees as assaults on natural selection within biology. He devotes two chapters to defending the view that "Biology is Engineering" — that organisms are best viewed as machines that have been optimally "designed" by natural selection. If correct, this engineering angle is immensely important to biologists as it suggests a research program. Just as we can



Searching for Design: centralized Greek cross superimposed on a human figure.

The bad guys in Dennett's book are those intellectual reactionaries who have convinced others that Darwinism can be safely bottled up within biology. Panicked by the spread of scientism, they plead:

Cede some or all of modern biology to Darwin, perhaps, but hold the line there! Keep Darwinian thinking out of cosmology, out of psychology, out of human culture, out of ethics, politics, and religion!

Dennett is not shy about identifying the obstructionists. Noam Chomsky is a ringleader, along with the biologists Richard

explain some aspect of design in the world, whether a *Bauplan*, Universal Grammar, or even a moral code. Among pre-Darwinian naturalists, God served as an unusually conspicuous skyhook. Why bother with conceiving a natural process that yields adaptation when God will do the trick? But bring on the acid: Darwinism teaches us that skyhooks are mythical. Design — whether manifest in cuttlefish or culture — is always built from the ground up by natural selection, without intervention from any outside superintendent.

Sketched like this, Dennett's claims may sound a bit fantastic, the wild dispensations of a barroom know-it-all. They are not.

¹ *The Riddle of the Universe at the Close of the Nineteenth Century* (New York: Harper and Brothers, 1901).

sensibly ask about the purpose of any part in a human-engineered machine ("Why do cars have spark plugs?"), so we can ask about the purpose of any trait in an organism ("Why do men have nipples?"). According to adaptationism, then, evolutionary biologists are in the business of constructing adaptive stories about why organisms are the way they are.

But Dennett does not so much champion adaptationism as excoriate those biologists who dare question it. His main targets are the Harvard biologists, Gould and Lewontin. In 1979, they published an influential (and still-controversial) paper in which they argued that adaptationism is flawed.² Their argument was simple: Although natural selection is an important force driving evolution, it does not follow that each arbitrary character one can point to has an adaptive purpose. Instead, some features of organisms are like "spandrels" in ecclesiastical architecture — the v-shaped spaces formed when two rounded arches meet at a right angle. Although spandrels are often decked out with mosaics, no one would seriously argue that spandrels are there because they provide such swell surfaces for mosaics. Instead, spandrels are there because they *have to be* — they are, it turns out, an inevitable by-product of putting a dome on rounded arches. Gould and Lewontin's warning was obvious: Organisms may also sport

² Stephen J. Gould and Richard C. Lewontin, "The spandrels of San Marco and the Panglossian paradigm: a critique of the adaptationist programme," *Proceedings of the Royal Society of London* 205 (1979): 581-598.

spandrels. Some traits have no adaptive tale to tell, but reflect structural constraints imposed by an organism's development or by its quirky evolutionary history.

Consider, for example, the blind spot, a small hole in our visual field. The blind spot represents the point where the optic nerve — inside our eye — plunges through the retina on its way to the brain. The blind spot is a maladaptive legacy of our evolutionary past. Early in vertebrate evolution, light sensitive tissue folded up in such a way that our proto-eye was left with its neuronal wiring *on the inside*.³ Once evolution veered off in this direction, a blind spot was a structural inevitability. If the wiring starts on the inside of the eyeball, some wire somewhere must plunge through the back of the eye to reach the brain. One makes up adaptive stories about the resulting blind spot at some peril.

Although many biologists suspect that Gould and Lewontin overstated their warning, most would, I think, agree with John Maynard Smith's assessment that their paper was "on the whole welcome."⁴ Alas, such moderate positions do not come naturally to Dennett. Although he pays lip service to the notion that any adapted system suffers from "undesigned" features, Dennett is hot under the collar about Gould and

³ See George C. Williams, *Natural Selection* (New York: Oxford University Press, 1992) for a discussion of this and other structural maladaptations in humans.

⁴ John Maynard Smith, "Dinosaur Dilemmas," *New York Review of Books*, April 25, 1991, p. 6.

Lewontin and their nefarious spandrels. The strange thing is, it's not so obvious why. Why, after all, should a man hoping to export Darwinism from biology be so obsessed with defending the minutiae of adaptive storytelling within biology? I would not think the legitimacy of, say, cultural Darwinism hangs critically on whether selection is very im-

■
*Dennett
does not so
much champion
adaptationism as
excoriate those
biologists who dare
question it.*
■

portant in biological evolution (as everyone, including Gould and Lewontin, believe) or is almost exclusively important (as Dennett believes). Cultural Darwinism could be right — or wrong — in either case. Dennett, though, seems convinced that he has a big stake in this distinction.

Apparently, Dennett sees red whenever adaptationism is questioned because he

suspects a plot to sneak in a skyhook: If you don't like adaptationism, what *do* you think explains all this biological design? Unfortunately, Dennett's skyhook-phobia gets the best of him, undermining any hope of a balanced review of the powers and perils of adaptationism. His review of attempts by biologists to circumscribe the role of natural selection is extraordinarily unbalanced. Consider this remarkable omission. Although a full third of his book examines challenges to evolution by natural selection "that have arisen within biology itself," Dennett never once mentions *the* most serious and famous of all challenges to selectionist story-telling: the neutral theory. Championed by Motoo Kimura,⁵ this theory claims that most evolution at the molecular level is *not* caused by natural selection, but by "genetic drift," the random replacement of one gene by another, functionally equivalent gene. If this is true, many differences between species have no adaptive significance. The fact that your hemoglobin looks like *this* and a spider monkey's looks like *that* reflects chance, not adaptation.

Why doesn't Dennett at least acknowledge this enormous challenge to selectionism? One possibility is that he may not know the neutral theory. Kimura — a mathematical population geneticist — is a tad harder to read than the pop biology Dennett appears to devour. This interpretation is supported by Dennett's claim that

⁵ Motoo Kimura, *The Neutral Theory of Molecular Evolution* (Cambridge, UK: Cambridge University Press, 1983).

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selection can detect any favorable variant, "[h]owever tiny the advantage in question" — a claim that the neutral theory shows is false. But this is probably the harmless hyperbole of a popularizer. And in one place Dennett admits that chance replacement of genes can occur, so he has obviously heard the neutralist song. A more serious possibility is that Dennett is interested only in "real" evolution — the camel's hump and the elephant's trunk — and not the evolution of molecules. But this can't be, for Dennett never tires of telling molecular stories *when they're adaptive*. Alternatively, Dennett may not want to let the cat out of the bag. Maybe he doesn't want the naive reader to know that biologists *have* found a place where the "universal acid" of selection becomes (quite literally) neutral and that many biologists believe that such "non-Darwinian" evolution is common.

But I suspect the real explanation is that Dennett is interested in Design (the uppercase "D" is his) and the neutralist/selectionist controversy has nothing to say here. If I am right about this, Dennett fundamentally misunderstands biologists' worries about adaptationism. Evolutionists are essentially unanimous that, where there is apparent "Design" in organisms, it is caused by natural selection. Consider the phenomenon of mimicry in butterflies, where a tasty species resembles an unpalatable species. Anyone who denies that selection is at work here simply doesn't know enough about mimicry in butterflies. But the reason this example goes undisputed is that the design

is so terribly obvious. Even if we cannot, at first, fathom the precise cause of the remarkable similarity between, say, the monarch and viceroy butterflies, we know that unrelated species do not look alike by accident.

Our problem, as evolutionary biologists, is *not* a weakness for fictitious alternative causes of Design. Our problem is that, in many adaptive stories, the protagonist does not show dead-obvious signs of Design. Is it obvious that the recessivity of most genetic diseases is adaptive? Evolutionists used to think so, but we now know they were almost surely wrong. Is it obvious that flower color differences in plants are adaptive? Many evolutionary biologists have begged to differ. Is it obvious that most molecular differences between species need adaptive explanation? The neutralists and selectionists give very different answers. And, last, is it obvious that the neural wiring that allows human language evolved as an adaptation for language? Different linguists reach different conclusions. The fact is we often have enormous difficulty distinguishing what is and is not "Designed" — what does and does not require its very own adaptive story.

If this weren't bad enough, our problem gets exacerbated by a little-noted, but immensely important, characteristic of our scientific culture: *Evolutionary biologists thrive on creating adaptive stories where Design is least obvious*. After all, where is the glory in explaining why some new species of mite is brown ("it hides in dirt")? The great challenge is to explain why some feature —

whose Design is far from apparent — is actually adaptive and optimally Designed ("this enzyme is more common in the mother than the fetus because . . .").

Given our difficulty discerning Design, and this penchant for concocting adaptive stories just where Design is least conspicuous, how could evolutionary biologists *not* have jitters about adaptationism? It would be an extraordinarily unreflective group indeed that did not ask questions like: How seriously should we take these endless adaptive explanations of features whose alleged Design may be illusory? Isn't there a difference between those cases where we recognize Design *before* we understand its cause and those cases where we try to make Design manifest *by* concocting a story? And isn't it worrisome that we can make up adaptive stories (and pen wildly speculative papers) faster than we can make up experimental tests? Note that there is no mystical talk here of imagined alternative causes of Design, nor any fatuous Darwin-bashing. There is just sensible concern about how much adaptationism is too much of a good thing. When does adaptationism stop being a useful research strategy and start being a silly exercise in cleverness? Dennett never confronts these legitimate worries. It is far easier for him to ridicule Gould and Lewontin's rhetorical excesses.

I suspect Dennett fails to appreciate these concerns in part because his thinking is guided by a subtly misleading picture of adaptation. Dennett is fond of speaking of selection as leading organisms through

"Design Space": Selection "lifts" organisms along "ramps" of good Design. Although this imagery is often useful, it invites two subtle misconceptions about adaptation. The first is that natural selection cares about Design. In reality, selection "sees" only brute birth, death, and reproduction, and knows nothing of Design. Selection — sheer, cold demographics — is just as happy to lay waste to the kind of Design we associate with engineering as to build it. Consider the eyes of cave organisms who live in total darkness. If eyes are expensive to make, selection can wreck their exquisite engineering just as surely as it built it. An optic nerve with little or no eye is most assuredly *not* the sort of design one expects on an engineer's blueprint, but we find it in *Gammarrus minus*. Whether or not this kind of evolution is common, it betrays the fundamental error in thinking of selection as trading in the currency of Design.

Second, hazy imagery of selection lifting organisms along Design ramps makes it hard to see that selection sometimes moves individual traits *down* ramps. But this surely occurs. The reason is that selection lifts overall "fitness"; it is not in the business of enhancing the design of isolated traits. Adaptation is built from favorable mutations, but favorable mutations are just genetic changes that have *net* good effects. Such mutations needn't have happy effects on every trait they affect. Indeed, this is extremely unlikely. Consequently, adaptive changes may improve traits A and B at the expense of trait C. In a world of two steps forward and one

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step back, we have no reason to expect optimal Design everywhere. This fact has nothing to do with disreputable non-Darwinian forces "getting in the way" of adaptation. As evolutionary biologists have long understood, it is adaptation.

In the end, though, this gnashing of teeth over adaptationism is not supremely important to Dennett. Despite devoting a hefty portion of his book to this problem, Dennett is not ultimately concerned with how natural selection builds organisms. He has much bigger fish to fry.

Genes, Memes, & Cultural Evolution

Except for a handful of biologists, there is little "dangerous" in the idea that Biology is Engineering. There is something dangerous, though, in the idea that Darwinism transcends biology, undermining our views of culture, consciousness, and morality. Persuading us of this is Dennett's real agenda. Surprisingly, then, he is quite unclear about precisely how Darwinism transcends biology. Sometimes he suggests that Darwin's dangerous idea is that all Design — whether natural or man-made — is connected by lines of descent. But this is only trivially true. Of course all of our good ideas (and bad) spring from brains that sprang from lesser brains back to the primordial ooze. If this is Dennett's Big Idea, it is not only an enormous let-down, it is without consequence. One may as well marvel that all our ideas descend from the Big Bang. More often, though, Dennett goes whole hog and argues that all Design — the spread of good ideas as well as clever adaptations — reflects the same process, natural selection. Indeed, Dennett has long championed the notion that Darwinism might explain why some ideas and styles flourish while others perish.⁶ Darwinism thus explains not just the biological origin of consciousness and culture, but their changing contents.

But this leap from origin to content is extraordinarily problematic. Take, for example, the humble medium of radio. Its origin is explained by the laws of electricity and magnetism: no E&M, no radio. But who thinks that the changing content of radio — the Sex Pistols versus Rush Limbaugh — is described by Maxwell's equations? One does not invoke a "skyhook" when one says, "The laws of E&M do not go this far." One talks sense.

Now Dennett is an able philosopher and this argument is surely not news to him. So why is he ineluctably drawn to the view that cultural change involves some brand of Darwinism? The reason is that he believes natural selection is an "algorithmic process," a blind, formal procedure whose operation is guaranteed to return a certain kind of result. A defining property of an algorithmic process is its "substrate neutrality": An algorithm does a job and returns a result whatever the input. Dennett concludes that natural selection, as an algorithm, is also substrate neutral. One can select between genes on chromosomes, codes in a computer, or ideas in a culture. As long as mutation, replication, and differential survival occur, any substrate can be selected. For instance, ideas can change (mutate), they can spread (replicate), and some can die out while others persist (differential survival), so we would seem to

have a substrate suited for selection. Following Dawkins, Dennett claims that the substrate that gets selected in cultural evolution is the "meme," any memorable idea, jingle, or fashion that lasts long enough to get copied by another person.

This substrate neutrality argument is supremely important to Dennett. It — and nothing else — explains why selection can be lifted from its historical base in biology. It is what makes Darwinism so dangerous. But Dennett slips here. While it is true that many different kinds of substrate can be selected, it is simply not true that Darwinism works with any substrate, no matter what. Indeed Darwinism can't even explain old-fashioned biological evolution if the hereditary substrate doesn't behave just right. Evolution would quickly grind to a halt, for

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instance, if inheritance were blending, not particulate. With blending inheritance, the genetic material from two parents seamlessly blends together like different colored paints. With particulate Mendelian inheritance, genes from Mom and Dad remain forever distinct in Junior. This substrate problem was so acute that turn-of-the-century biologists — all fans of blending inheritance — concluded that Darwinism just can't work. Modern evolutionary genetics was born in 1930 when Sir Ronald Fisher cracked this problem: Population genetics shows that particulate Mendelian inheritance saves the day. It is just the kind of substrate needed for evolution by natural selection to work.

What, then, about Dennett's memes — all those "tunes, ideas, catch-phrases, clothes-fashions, ways of making pots or of building arches." Do they show particulate or blending inheritance? Do street fashion and high fashion segregate like good genes, or do they first mix before replicating in magazines or storefronts? Does postmodern architecture reflect a blending of the modernist and classical or the inheritance of distinct LeCorbusier and Vitruvius genes? I do not know the answers to these questions. And neither does Dennett. And neither does anyone else.

But it's worse than this. As Dennett reluctantly admits, memes and genes differ in other fundamental ways. Species, once isolated, almost never exchange genes, while exchange between long-isolated cultures is immensely important in the history of ideas. Moreover, new ideas — but not genes — are produced by a sort of directed mutation. Newton did not uncover the Fundamental Theorem of Calculus by conceiving millions of random ideas. In addition, the fitness of memes is strangely tautological. While

we can often point to ecological reasons why certain genes are fitter than others, a meme is deemed "fit" only because it is common. ("Elvis is alive" is certainly a fit meme, but it is neither true nor helpful. It is merely popular.) Last, Dennett confesses that memes often show a Lamarckian, not Darwinian, style of evolution, in that acquired traits get passed along.

Despite Dennett's assurance, then, that "concepts from population genetics transfer quite smoothly" to "population memetics," it is far from clear that Darwinism can account for the percolation of ideas, styles, and songs through culture. In fact, there is a basic problem with any such claim — we are very ignorant of how humans hold ideas in their heads and of how the ideas in your head influence the ideas in my head. So how can we possibly conclude that the process "must be" Darwinian? As Searle emphasizes, it is this ignorance of how things actually work that makes the pretensions of these wild theories possible: "Such claims live in the holes of our knowledge."⁷

In light of these problems, how can Dennett be so confident of his Darwinian science of cultural evolution? Here's the real shocker: He isn't. Halfway through his book, Dennett confides that the "prospects for elaborating a rigorous science of memetics are doubtful." But he assures us that, "[w]hether or not the meme perspective can be turned into science, in its philosophical guise it has already done more good than harm." I confess that I am astonished by this move. Without question, the central tenet of Dennett's book is that Darwinism cannot and should not be confined to biology. As he repeatedly reminds us, the fact that selection is substrate-neutral allows a downward diffusion from biology and an "upward spread of universal acid through human culture." Indeed: "According to Darwin's dangerous idea, all possible explorations of Design Space are connected. Not only all your children and your children's children, but all your brainchildren and your brainchildren's brainchildren must grow from the common stock of design elements, genes and memes . . . Life and all its glories are thus united under a single perspective."

Or at least this is what Dennett promised us. So what happened? Do we have a Darwinian explanation of our brainchildren or not? Are we left with a science of cultural evolution or a vaguely interesting half-analogy between genes and memes? The answer is clear: Dennett neither describes nor is confident of a Darwinian science of culture. He is in good company; Dawkins backed off his meme-talk long ago. Indeed his more recent (and very sensible) views on the limited usefulness of memes should be required reading for all would-be cultural Darwinists.⁸ Although Dennett never quite admits it, he has also backed off, as can be seen by comparing his 1990 article on "Memes and the Exploitation of Imagination" (see note 1) and his 1991 book *Consciousness Explained*⁹ with chapter 12

⁷ John R. Searle, *Minds, Brains, and Science* (Cambridge, MA: Harvard University Press, 1984).

⁸ Richard Dawkins, *The Extended Phenotype* (Oxford: W. H. Freeman and Company, 1982), pp. 111-112.

⁹ (Boston: Little, Brown and Company), pp. 199-210.

of *Darwin's Dangerous Idea*. Although he still talks up memes *ad libitum*, he now feels compelled to mention that maybe they're not so much like genes after all and (gulp) maybe they're not even Darwinian. They just provide a "valuable perspective." Given this denouement, Dennett's habit of ridiculing humanists for their reluctance to face the cold, hard facts of cultural Darwinism is incomprehensible. The fact is there is no such science.

Dennett also never comes clean about why his hoped-for cultural Darwinism failed to fly. He would have the naive reader believe that memetics was shot down by soft-headed humanists ("Darwin-readers") who panicked at the encroachment of science. Surely his readers deserve to know that many of the most trenchant criticisms of memetics came from scientists. Dennett's strategy seems clear. When attempting to intimidate humanist opponents, he is fond of rattling the saber of science. He is allied with the armies of Progress, and his foes are mushy poets. The problem with this strategy (aside from its arrogance) is that it assumes there really is some science underlying his views. How much more intimidating to swing the club of Darwinian science at one's opponents than to announce that you come armed with a useful "philosophical guise."

Given the gulf between his promise of a universal acid and his delivery of an anemic memes-as-a-valuable-perspective, it comes as no surprise that Dennett is a fan of the bait-and-switch strategy. This, he reminds us, is

[the] practice of attracting customers by advertising something at a bargain price and then, when you've lured them to the store, trying to sell them a substitute. Unlike that practice, evolutionary bait-and-switch is not really nefarious; it just seems to be, because it doesn't explain what at first you thought you wanted explained. It subtly changes the topic.

Evolution and Ethics

Pervading Dennett's discussion of ethics is a similar confusion about how far Darwinism extends. Here the pattern isn't so much bait-and-switch as bait-and-switch-and-switch. Dennett's argument careens through three stages:

1. Early on, we are treated to plenty of hype about how Darwinism undercuts many of our views of mind and ethics and "illuminates them in ways that have eluded the traditional approaches, recasting ancient problems and pointing to their solution." We are also enticed by chapter and section titles like "Can Ethics be Naturalized?" and "Redesigning Morality."

2. Surprisingly, Dennett then delivers a measured exposé of the excesses of human sociobiology. Many sociobiologists, he warns, commit the genetic fallacy, wherein one assumes that moral codes appeared "for the benefit of the genes." Dennett dutifully reminds us that the appearance of culture radically alters the rules of the game — once memes arrive, they can call the shots. Indeed, people, "as a result of interactions between their meme-infested brains, are not at all bound to answer to the interests of their genes alone."

3. But Dennett suddenly lurches back to Darwin, asserting that, while culture

⁶ See his "Memes and the exploitation of imagination," *Journal of Aesthetics and Art Criticism* 48 (1990): 127-135.



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renders the world more complex than many sociobiologists pretend, this means only that we must perform "a more careful inquiry" into the origins of morality. And what does this more careful inquiry look like? One thing is certain: It is genetic. Indeed it looks a lot like textbook sociobiology. Dennett explains that a "vivid way of posing the question is to imagine becoming an *artificial* selector of altruistic people" (his emphasis). But since God appears not to have performed this experiment for us, we must explain "the evolution of ethics by *natural* selection" (his emphasis). This occurs via the usual sociobiological steps. Building on care of progeny, selection encourages an investment in other relatives (so-called "kin selection"). Nothing controversial so far. But this must be followed by the far more difficult task of extending the moral circle to include all, kin or not. ("Thou shalt not kill" says nothing about relatedness.) Fortunately, a genetic theory of "reciprocal altruism" shows how such behavior can, in fact, evolve by natural selection.

It is probably unfair to brand this zig-zagging, "It's all Darwin . . . it's all culture . . ." argument bait-and-switch. I suspect that Dennett is genuinely unsure where to plant his feet. After passionately arguing that the memes of culture can snap the leash of genetic control, why abruptly argue that morality evolved via selection of genes?

I should make it clear that I am not troubled by the possibility that your civility (as well as mine) reflects ancient genetic changes. It may well. But the notion that the diffusion of morality among humans *must* have involved natural selection is absurd. Imagine, for instance, that evolution blessed our hirsute ancestors with consciousness and language. But, alas, these immoral brutes — who are kind only to their families — have run out of genetic variation and, thus, *no* further evolutionary change is possible. What reason is there to think that social contracts, cooperation among hunters, and the other ingredients of an *Ur*-morality couldn't catch on among these thinking, speaking beasts? None as far as I can see. Is it obvious that genetic changes are required for such a thing? Where are Dennett's trusty memes when we need them?

The deeper point, though, is that this navel-gazing — these endless attempts to theoretically reconstruct what "must have" occurred during the emergence of human morality — is no more than academic exercise. The ugly fact is that we haven't a shred of evidence that morality in humans did or did not evolve by natural selection. We do not even know what such evidence would look like. We can, if we like, construct plausible adaptive scenarios ("What would happen to a gene that said be nice to strangers if . . ."). But, in the end, a thought experiment is not an experiment. We have no data.

Dennett's treatment of evolutionary ethics is symptomatic of the problem plaguing his entire book. He is forever suggesting that the universal acid of natural selection *may* be involved here or there. Natural selection of alternative universes may explain why we live in a world having just these physical constants (I spared you this one). Selection may explain the rise and fall of ideas and songs. Selection may explain why "strong" artificial intelligence

is destined to work (nature got semantics out of syntax, so digital computers can too). Selection may explain the spread of ethical codes among humans. But at each milestone the skeptical reader grumbles, "But maybe not." After all, the evidence for each claim ranges from non-existent (alternative universes, origin of morality) to negative (Darwinian evolution of memes). All Dennett really shows is that — if one squints hard enough — one can *sort of* see how Darwin's dangerous idea *might* play a role in this, that, or the other. Although he has produced a provocative and intermittently entertaining book, Dennett's chief claim is unconvincing. Darwinism may have little to tell us outside of biology.

Dennett, ever optimistic, triumphantly concludes that "[t]here is no denying, at this point, that Darwin's idea is a universal solvent, capable of cutting right to the heart of everything in sight." Drawing his argument to a close, he quotes, with some approval, Nietzsche's bleak vision of a Nature ruled by an apathetic but omnipresent Darwinism:

"According to nature" you want to *live*? O you noble Stoics, what deceptive words these are! Imagine a being like nature, wasteful beyond measure, indifferent beyond measure, without purposes and consideration, without any mercy and justice, fertile and desolate and uncertain at the same time; imagine indifference itself as a power — how *could* you live according to this indifference!¹⁰

But Nietzsche's real message about the Stoics — which I suspect Dennett would rather you not hear — is very different. It is, however, surprisingly relevant. In the next paragraph Nietzsche reverses himself:

In truth, the matter is altogether different: while you pretend rapturously to read the canon of your law in nature, you want something opposite, you strange actors and self-deceivers! Your pride wants to impose your morality, your ideal on nature — even on nature — and incorporate them in her; you demand that she should be nature "according to the Stoa" . . . For all your love of truth, you have forced yourself so long, so persistently, so rigidly-hypnotically to see nature the wrong way, namely Stoically, that you are no longer able to see her differently . . . But this is an ancient, eternal story: what formerly happened to the Stoics still happens today, too, as soon as any philosophy begins to believe in itself. It always creates the world in its own image; it cannot do otherwise.¹¹

It is perhaps understandable why Dennett — so fond of creating Darwinian works of cosmology, culture, and mind — chose not to quote the entire passage. ■

¹⁰ Friedrich Nietzsche, *Beyond Good and Evil*, trans. Walter Kaufmann (New York: Vintage Books, 1966), p. 15.

¹¹ *Beyond Good and Evil*, pp. 15-16.