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Letters

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From Daniel Dennett

I love the style of Jerry Fodor's latest attempt to fend off the steady advance of evolutionary biology into the sciences of the mind. He tells us that 'an appreciable number of perfectly reasonable biologists' are thinking seriously of giving up on the half of Darwinism that concerns natural selection. Did you know that? I didn't. In fact, I wonder if the appreciable number is as high as one. Fodor gives no names so we'll just have to wait for more breaking news. He does provide two of his favourite foretastes, however: evo-devo and the famous case of the domesticated Russian foxes. These interesting developments both fit handsomely within our ever-growing understanding of how evolution by natural selection works. Briefly, evo-devo drives home the importance of the fact that in addition to the information in the genes (the 'recipes' for making offspring), there is information in the developmental processes (the 'readers' of the recipes), and both together need to be considered in a good explanation of the resulting phenotypes, since the interactions between them can be surprising. Of course the information in the developmental processes is itself all a product of earlier natural selection, not a gift from God or some otherwise inexplicable contribution. The foxes are a striking instance of how selection acting on one trait can bring other traits along with it – which may then be subject to further selection. It corrects the naive assumption that *everything* is directly evolvable – docile foxes with zebra stripes, or green foxes, or pigs with wings – but nobody makes that assumption, aside from the straw men constructed by some ideologues.

I won't bother correcting, one more time, Fodor's breezy misrepresentation of Gould and Lewontin's argument about 'spandrels', except to say that far from suggesting an alternative to adaptationism, the very concept of a spandrel depends on there being adaptations: the arches and domes are indeed selected for, and they bring spandrels along in their wake. No 'perfectly reasonable biologist' has claimed that the hugely various and exquisitely tuned sense organs of animals, or the superbly efficient water-conserving methods of desert plants, are spandrels, even if they spawn spandrels galore.

What could drive Fodor to hallucinate the pending demise of the theory of evolution by natural selection? A tell-tale passage provides the answer: 'Science is about facts, not norms; it might tell us how we are, but it couldn't tell us what is wrong with how we are. There couldn't be a science of the human condition.' There can indeed be a science of the human condition, but it won't tell us, directly, 'what is wrong with what we are'. It can, however, constrain our ultimately political exploration of what we think we ought to be by telling us what is open to us, given what we are. Fodor's mistake, which he is hardly alone in making, is to suppose that if our minds are scientifically explicable bio-mechanisms, then there could not be any room at all for values. That just does not follow, but if you believe it, and if you cherish – as of course you should – the world of values, then you have to stand firm against *any* physical science of the mind. It's admirable, in a way, if you like that kind of philosophy. But it is better to repair the mistake; then you can have a science of the mind and values too. And you don't have to misrepresent science out of fear of what it might be telling us.

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Why Pigs Don't Have Wings

From Jerry Fodor

A perceptible flurry in the dovecote. Here are some replies to my critics. It seems to me that Simon Blackburn has comprehensively missed the point (Letters, 1 November). He takes the problem I raised to be epistemological: 'If two traits occur together, how do we know which was "selected" for?' But I don't do epistemology, and that isn't what I'm worried about (nor, by the way, is it what worried Gould and Lewontin). My question was: how can the operation of selection distinguish traits that are coextensive in a creature's ecology? Perhaps news about mountain hares and such tells us what colour was selected for in polar bears. But selection didn't consider mountain hares when it coloured polar bears. Nor, quite generally, did it consider such counterfactuals as 'what would happen to white bears if the colour of their environment changed?'

The same applies to Tim Lewens's line of thought. The selection of colour in polar bears can't be contingent on such counterfactuals as: 'what if one dyed their fur green?' In fact, it can't be contingent on any counterfactuals at all. We can apply the 'method of differences' to figure out what colour evolution made the polar bear; but selection can't apply the method of differences to figure out what colour to make them. That's because we have minds but it doesn't.

Some of my critics point out the importance of linkage as a mechanism that might explain why, for example, domesticated foxes have floppy ears. Quite so, but linkage is an endogenous trait, and adaptationism is committed to explaining phenotypes by reference to exogenous variables.

The same applies to the remarks by Steven Rose (Letters, 15 November). To give up on the idea that selection is determined by largely exogenous forces is to abandon adaptationism in all but name. No doubt, if we knew enough about the

macro and microstructure of organisms (and of their ecologies) we would understand their evolution. If that's adaptationism, then I'm an adaptationist too (and so is every materialist since Lucretius).

Jerry Coyne and Philip Kitcher make the usual mistake. In fact, I am not worrying about whether we can tell if 'polar bears were selected for being white or for matching their environment'. I repeat: I don't do epistemology. Nor do I deny that we can often focus on different aspects of the causal history underlying an episode of selection. The problem is that it makes no sense at all to speak of the aspect of a causal history that selection focuses on; to say (as it might be) that selection focused on the whiteness of the polar bear rather than its match to the surround. Selection doesn't focus: it just happens.

Coyne and Kitcher then say that 'the concept of "selecting for" characteristics is largely a philosopher's invention.' I don't know who invented it, but that can't be right. If the theory of adaptation fails to explain what phenotypic traits were selected for, it won't generalise over possible-but-not-actual circumstances; it won't, for example, tell us whether purple polar bears would have survived in the ecology that supports ours. It will not be 'news to most knowledgeable people' that empirical theories are supposed to support relevant counterfactuals. If adaptationism doesn't, that *is* news.

Coyne and Kitcher suggest that evo-devo doesn't purport to be an alternative to adaptationism but rather is 'consistent with' natural selection. That's right but not relevant. Part of my point was that if adaptationism is independently incoherent (as, in fact, I believe it to be) then we're in want of an alternative. Evo-devo may reasonably be considered a step towards supplying one.

They also say that it doesn't matter whether selection can draw all the distinctions between traits so long as it can draw the important ones. I don't know how they tell which ones are important, but they ought to bear this in mind: selection is insensitive to the difference between any traits that are even *locally* confounded (i.e. that are confounded in a creature's actual history of causal interactions with its ecology). It can't, for example, distinguish encounters with big tails from encounters with colourful tails if all and only the big tails Miss Peacock has come across are colourful. (Of course, *we* can tell the difference

between selecting for one and selecting for the other; that's because, unlike natural selection, we have minds.) If it isn't important (to, for example, ethology) whether it's big tails or colourful tails that lady peacocks like, then so much the worse for importance.

Finally, Coyne and Kitcher ask how anything but adaptationism can explain the match between a creature's phenotype and its ecology. This question is entirely pertinent. But they will have to read about it in Fodor and Piatelli-Palmarini (forthcoming).

Over the years, I've been finding it increasingly difficult to figure out which bits of Daniel Dennett's stuff are supposed to be the arguments and which are just rhetorical posturing. In the present case, I give up. I'll take it more or less paragraph by paragraph. Dennett speaks of the 'steady advance of evolutionary biology into the sciences of the mind'. He provides no examples, however, and surely he knows that there is a considerable body of literature to the contrary. (See, for example, David Buller's book *Adapting Minds*.) Even Dennett's fellow-critics of my piece express, in several cases, attitudes towards the evolutionary psychology programme ranging from scepticism to despair: it's a recurrent theme of theirs that Fodor is, of course, right about EP; but he's wrong about natural selection at large.

I cite the fox experiments and the literature on evo-devo as evidence of the importance of endogenous factors in directing the course of evolution. Dennett does not deny that lots of endogenous factors constrain the course of evolution; or that the cases I cited are instances; or that appeals to endogenous variables are alternatives to natural selection. 'Of course the information in the developmental processes is itself all a product of earlier natural selection.' What's the argument for that, I wonder. It appears, *prima facie*, simply to beg the question at issue.

Dennett can't be bothered to correct my 'breezy misrepresentation of Gould and Lewontin'. In fact, he can't even be bothered to say what it consists in. That being so, I can't be bothered to refute him.

'The very concept of a spandrel depends on there being adaptations.' This suggests that Dennett has utterly lost track of the argument. Of course the spandrels are free-riders on the architect's design for the arches and domes. But

the question I wanted to raise was precisely whether this account of selection-for can be extended to cases where, by general consensus, there isn't any architect. In particular, I claim, Darwin overplayed the analogy between artificial selection (where there is somebody who does the selecting) and 'natural' selection (where there isn't). How could anybody who actually read my article have missed this?

I said that metaphors like 'evolution selects for what Mother Nature intends it to' have to be cashed. The rules of the game require respectable adaptationists to give an account of selection-for that doesn't appeal to agency. Suppose (what's not obvious) that explaining the scientific results really does require a notion of biological function (hence of selection-for). It simply doesn't follow that it requires a notion of biological function that is reconstructed in terms of selection history. Dennett must know that, de facto, there is no such notion. Biological function is itself an intentional concept, so appeals to it don't cash the Mother Nature metaphor; they just take out loans on its being cashed sooner or later. It seems that everybody understands this except Dennett.

Finally, Dennett says I am worried about preserving my values in the face of scientific reduction. Where on earth did he get that idea? I've spent more of my life than I like to think about arguing that ontological questions about reduction are neutral with respect to epistemological questions about intentional explanations. As a matter of fact . . .

But on second thoughts, to hell with it.

The reader may wonder whether there are any general morals to draw from all this. There are three: don't forget the importance of getting the counterfactuals right; don't confuse your ontology with your epistemology; and do try to keep your cool.

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