

Jerry Fodor responded to John Staddon's comment and a dialogue ensued. The focal point seems to be whether natural selection should (Fodor), or potentially does (Staddon) provide a causal account of evolutionary adaptation. A slightly edited version follows:

Response: A dialogue between Fodor and Staddon

PsyCrit February 12, 2008

Jerry Fodor: I've had a read through but I guess I'm not very moved. Here's why. The argument I have on offer is really very simple and (as far as I can tell) perfectly clearly sound.

1. Evolutionary Adaptationism (EA) is supposed to explain the distribution of phenotypic traits in populations. (General consensus).
2. There are many cases where a trait that is selected for has free riders. (Ditto)
3. Selection can't distinguish a trait that is selected for from its free riders (By definition; that's what a free rider *is*)
4. So there are many phenotypic traits the distribution of which EA can't explain. QED
Do you really think there's something wrong with that?

John Staddon: *What seems to be wrong is considering selection in isolation, separated from variation. Selection by itself is like one hand clapping. You must have both to get anything. What to expect depends then on the reproductive fitness of different phenotypes and the way that phenotypes translate, via genotype and development, to the next generation of phenotypes.*

The next step in the argument is to consider different types of variation. If the range is broad and unstructured, then the direction the population is moved by selection will obviously be more determined by selection than by variation, because the population is free to move in any direction. On the other hand if the range of variation is limited or highly structured, the range of possibilities for selection is obviously also limited and the changes from generation to generation are much more attributable to variation than selection. Incremental improvement, in body shape of a fish, say, seems much more explicable by selection with less contribution from variation which may be more or less random wrt body shape. But recurrent mutations, 'sports', Lyudmilla Trut and some of the examples of correlated variation given by Darwin and Lewontin-Gould, clearly implicate constraints on variation—you can't have a big beak without getting big feet, sometimes there are large changes from one generation to the next, and so on. Is Darwin's 'correlated variation' what you mean by "free riders." If so, the problem is not with selection but with the range of variation it has to work with.

JF: I entirely agree with this. If you want an alternative to selection, endogenous constraints on variation look like being the only candidate.

JS: No! Not an *alternative!* Selection by itself is meaningless. Selection is just a filter: the output depends also on the input—variation. Selection alone can explain nothing (maybe that's your point?)

JF: That is assuming, however, that the idea of selection is coherent; and I think it isn't; since (to repeat) selection can't, in principle, distinguish traits that are selected (i.e. ones that affect fitness) from free-riders (coextensive traits that don't affect fitness.)

JS: But doesn't that restriction also apply to artificial (i.e., by a human agent) selection as well? So would you say that the concept of artificial selection is also incoherent? When the Russian

lady selected for tameness she also got floppy ears etc. But she did also get tameness. Why is the 'free rider' a problem?

JF: By a 'free rider' I mean what Gould and Lewontin mean by a 'spandrel' i.e. any property of a phenotype that is neutral in respect of fitness but covaries with a property that is selected for. In the LRB article, I tried to explain why the argument works by appealing to a number of (tentative but philosophically interesting) theses: The intensionality of 'trait' and 'selection for'; the fact that traits are individuated by counterfactuals, etc. (There's more on this sort of stuff in an article I just published in MIND AND LANGUAGE, q.v. But these theses are ancillary to the central argument, which stands on its own feet.

Specifics (I'll go through your paper more or less para by para): Para 1. Oh, come on. Who cares whether Skinner was a Skinnerian (which, of course, he was). Probably Marx wasn't a Marxist, Freud wasn't a Freudian and Darwin wasn't a Darwinian. So what?

JS: What I was beefing about here was not so much calling Skinner a theorist (which he denied although, in a subtle, implicit and offhand – but irrelevant – way he of course was, as everyone is) as your essentially unsupported assertions in other pieces that humans (and perhaps animals?) "must" plan, compute and so forth in conscious, symbol-based ways. And of course it is no more fair to take as a model for behaviorism Skinner's obsolete variety than it would be to use Aristotle as a model for cognitive psychology.

JF: OK, but I don't think it's 'unsupported', there can't be a serious doubt about whether (at least some) creatures plan and think; and, to my knowledge, the computational story is the only serious proposal about how plans, thought and the like could have causal powers (how they cause behaviors (and/or one another). I don't, however, think – nor have I ever suggested – that any of this is conscious. Quite the contrary, as far as I can tell.

JS: OK!

JF: You are, however, quite right that Darwin is aware (in *Origin*) of connectivity between traits (and also of what he calls 'Laws of Growth', which has a very Evo/Devo sound).

JS: Yes it does!

JF: He says, however, that though such phenomena are doubtless very important, nothing is known about them. I think that's still how things stand.

JS: Not really, evo-devo is a real field, isn't it?

JF: (BTW acknowledging a counterexample doesn't stop it from being a counterexample.)

JS: Well, acknowledging variation is not – cannot be – a *counterexample* to selection, since variation and selection must be considered together to explain evolution (see above).

JF: I don't understand this. The question isn't whether there is variation; of course there is on anybody's story. It's rather whether variation is (as Darwin often suggests) incremental and gradual. Evo/Devo is the thesis that it's not.

JS: Agreed. But even Darwin knew of striking exceptions to gradualism – 'sports', for example.

JF: Para 3: Where in my paper do I assume that "natural selection always yields perfect adaptation" or that Darwin thought it did? Notice that neither thesis occurs in the argument sketched above? (Notice too, by the way, that my claim *isn't* that Darwinists (wrongly) think that EA accounts for *all* the phenomena of phenotypic distribution. It's that free-riding offers innumerable many counter examples EA. (Which indeed it does.) If I'm right to hold that the problem traces,

eventually, to the intensionality of traits and selection-for, then the counterexamples are not just innumerable, but also principled.

JS: Not sure I follow this para, but here goes. I am indeed not totally sure (a) what you mean by ‘adaptationism’ and (b) whether it corresponds to what biologist believe. You write in the *LRB* article “Perhaps the consensus view among Darwinists is that phenotypes evolve because fit individuals are selected for the traits that make them fit. This way of putting it avoids the ambiguity, but whether it’s viable depends on whether adaptationism is able to provide the required notion of ‘selection for’; and it seems, on reflection, that maybe it can’t...” Well, by ‘selection for’ surely all that is meant is that individuals that have trait X (large beaks, say) have more offspring in environment A than individuals that have trait Y (small beaks), and conversely in environment B (more Y offspring than X). Just what is problematic about that?

JF: No, no, and more no. For a trait to be selected for is for it to be the (a) *cause of* (and hence to explain) effects on fitness. This is why issues about free riding are essential. If (trait) A free rides on B. then selection *can’t*, as a matter of principle, decide which is the cause of the corresponding phenotypic variation. It can’t answer the question ‘which is the one selected for and which is the free rider.’ (It can’t distinguish the spandrels from the arches). This really is the crucial point on which the whole argument turns.

JS: Aha! Maybe I do see the problem. Of course selection-variation is not a causal explanation. It is what Pittendrigh¹ years ago called *teleonomic*. That is, an account that does indeed refer to a final cause, as in teleology, but in a way that can potentially be reduced to an efficient-causal account once we understand the evo-devo details.

JF: This is very dangerous territory, for at least three reasons.

i. If adaptationism is teleological explanation, then it doesn’t do what Darwinists always say it does: viz. provide the MECHANISM of evolution. Teleological explanations are *ipso facto* not mechanistic (i.e. not causal). So we’re back where we started: in need of a mechanism for evolution.

ii. The stuff about reduction is *very* tricky; but, roughly: Adaptationism was supposed to be a *biological* theory; if they are really *reduced* to genetics (physics, whatever), then the explanations adaptationism was supposed to offer are actually provided by the reducing theory; which may not (probably won’t) reconstruct the *taxonomy* of the reduced theory. (Try to say ‘ecological variable) in the vocabulary of genetics; or quantum physics, come to think of it. (There’s a lot of stuff about this sort of issue in the literature on relations between psychology and neurology. It is, to put it mildly, unclear what morals ought to be drawn. If I were Darwin, I’d make a point of staying away from it.)

iii. If you explain ‘selection for’ in terms of teleology, you can’t, on pain of circularity, also explain teleology in terms of selection for. In particular, you can’t say (as adaptationists invariably do) that the (biological function) of a trait is ‘whatever function it was selected for performing’) So you are now in debt for a *non-adaptationist* account of biological function. Lots of luck!

Gould and Lewontin: Their point is not to deny that “the spandrels arose for some other reason”; it’s that free riding is a source of counterexamples to EA. They’re right about that. What they missed is that the free riding cases point to a crucial difference between selective breeding and natural selection: Spandrels are the effects of *mental* causes; they are to be explained in terms

¹ Pittendrigh, C. S. Adaptation, natural selection, and behavior. In A. Roe & G. G. Simpson (Eds.), *Behavior and evolution*. New Haven: Yale University Press, 1958.

of the architect's intentions. *This matters because there isn't an architect in the case of natural selection.* So EP has to make the selected for/free rider distinction in some other way. *What way, I wonder.*

Of Course I don't think that selection happens by chance. *Of course* there are reasons why phenotypes survive (or don't)., The point is that the reasons there are aren't somebody's reasons; they don't involve mental causation. This makes natural selection and breeding *very* different sorts of processes. Breeding is an instance of intelligent design, but natural selection isn't ... Gopnik missed this (and so did Darwin).

JS: I don't think Darwin missed this. His point, I think, is that it *doesn't matter* how selection comes about, whether by human agency or via 'natural' causes – competition, predation, intrinsic fecundity, or whatever. His point surely is that *the results are indistinguishable*. The Darwinian argument is just an 'as if' argument. His point is that we can sometimes infer from differential reproduction patterns that certain traits are (naturally) selected, in the same way a human breeder might select. That's all. I guess I just don't see why that is problematic.

JF: To the contrary; the results are distinguished *by the counterfactual*: If A free rides on B, then you'd still get A in situations which don't produce B (and vice versa). So the critical issue is 'what supports the counterfactuals in cases where there isn't an agent of selection?' I think Darwin hasn't the foggiest idea; he simply *takes for granted* that natural selection works like breeding. But it can't; only creatures with minds can respond to (merely) counterfactual outcomes (see next para.).

So, no it *doesn't* apply to the human case. In the human case, there is a fact of the matter about the breeder's intentions (likewise in the case of the arches and the spandrels.) These intentions determine what *would* happen in the counterfactual case (if the architect could have had arches without spandrels, that's what he would prefer.) The breeder (/architect), since he has a mind, is able to represent these counterfactual situations, and the representations guide his behavior. (He thinks to himself: If I took the spandrels away the arches would fall too. Then the whole damned thing would fall down. So I'd better stick with the spandrels.) *Nothing of that kind can happen in the case of natural selection.* That's why it was a mistake of Darwin to attempt to explain evolution on the model of intelligent design. (Ironies abound.)

Last page. My point wasn't epistemological. Of course there are (counterfactual) situations in which coextensive traits come apart (unless they are logical or nomologically coextensions) so, of course, we can often distinguish the traits that contribute to fitness from their free riders. But I do try to keep my epistemology apart from my ontology. My point was that (merely) counterfactual outcomes *can't affect selection*; only creatures with minds can respond to (merely) counterfactual outcomes; and, though we have minds, natural selection doesn't. Most of the history of philosophy over the last century has consisted of confusing epistemological issues from ontological ones (the rest has consisted of confusing epistemological issues with semantic ones.) It really is time to stop this nonsense.

JS: Well, I confess I am mystified by this para. Are you just saying that the fact that trait X is preferred in environment A is necessarily unaffected by the counterfactual (is that right?) that trait not-X is preferred in environment B? Well, yes, but so what? And are you simply insisting that the notion of selection necessarily implies a sentient agent? (Surely not...)

JF: Almost. I'm suggesting that SELECTION-FOR necessarily implies a sentient agent. (Actually the situation is more complicated than that; but the present approximation is close enough). This is exactly why there can't be 'natural selection for.' Darwin missed this – in effect, he thought that the analogy between selection and selection for is unproblematic– and so, as far as I can tell, did everybody else. 150 years of sheer blunder, as far as I can see.

One thing I do agree with you about is the Skinner/Darwin similarity. They both held essentially the same theory (as, indeed, Skinner kept pointing out). That wasn't, however, because Skinner was right; it was because Darwin was wrong.

JS: Skinner wasn't the first to see the similarity between operant conditioning and natural selection, incidentally, Brit biologist, Pringle, and cyberneticist, Ross Ashby, anticipated him by many years, as did the American psychologist D. T. Campbell.