AGAINST DARWINISM

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Preface

This started out to be a paper about why I am so down on Evolutionary Psychology (EP), a topic I've addressed in print before. (see Fodor, 19xx; 19xx). But, as I went along, it began to seem that really the paper was about what happens when you try to integrate Darwinism with an intentional theory like propositional attitude psychology. And then, still further on, it struck me that what the paper was *really* really about wasn't the tension between Darwinism and theories that are intentional (with a `t'), but the tension between Darwinism and theories that are intensional (with an `s`).[1] The latter is more worrying since Darwinism, or anyhow adaptationism, is itself committed to intensionally individuated processes like `selection for.' So the claim turned out to be that there is something seriously wrong with adaptationism per se. Having gotten that far, I could have rewritten this as straightforwardly a paper about adaptationism, thereby covering my tracks. But I decided not to do so. It seems to me of interest to chart a route from being suspicious of Evolutionary Psychology to having one's doubts about the whole adaptationist enterprise.

The central claim of Evolutionary Psychology (EP) is that heritable properties of psychological phenotypes are typically adaptations; which is to say that they are typically explained by their histories of selection. In particular, this is claimed on behalf of heritable phenotypic properties that involve intentional

[1] It's hard to imagine a less fortunate terminology than the philosopher's `intention/intension' distinction. But I suppose there's nothing can be done at this late date. In what follows, an intensional context is one in which the substitution of coextensive expressions isn't valid. Intentional states are just the familiar beliefs, desires, intentions and so forth that populate theories of cognition and of the integration of behavior. I assume, following the tradition, that expressions that refer to propositional attitudes typically establish intensional.contexts, so that one can believe that Venus is the Morning Star and yet not believe that Venus is the Evening star, despite the astronomical fact that…. Etc.

states like believing, desiring, and acting (or being disposed to act) in one way or another.[2] It is possible to doubt that the empirical evidence for claiming this is, so far at least, overwhelmingly persuasive. (See, eg. Buller, 19xx). Be that as it may. In the first part of this paper, I want to argue for something much stronger: that the whole idea of an evolutionary psychology is very likely ill-conceived.. Much of the main line of argument I'll pursue is already to be found in the philosophical literature, especially the literature on evolutionary semantics. So my strategy is to start by reminding you of some of the morals of that discussion and to contend that they apply quite generally to selectionist accounts of the cognitive psychological phenotype.

The edifying fable of the frogs and the flies

Frogs snap at flies; having caught one, they then ingest it. It is plausibly in the interest of frogs to do so since, all else equal, the fitness of a frog that eats flies (and hence the likelihood of its contributing to the local gene pool) exceeds the fitness of a frog that doesn't. It is likewise plausible that the frogs' penchant for catching flies is an adaptation; which is to say that it was established in the frog's behavioral phenotype by a process of natural selection. If so, then perhaps it follows that the *function* of the behavior (and/or of the physiological mechanisms by which it the behavior is implemented), is precisely to mediate the catching of the flies by the frogs. Maybe, that's to say, some selectionist story about the phylogeny of fly-snapping can provide, at the same time, an account of the teleology of that response. I don't believe much of that, but never mind; let's assume for now that it's all true.

I suppose it is likewise plausible that frogs catch flies with the intention of doing so. (If you are unprepared to swallow the attribution of intentions to frogs, please feel free to proceed up the phylogenetic ladder until you find a kind of creature to which such attributions are, in your view, permissible.) Now, intentions-to-act have intentional objects, which may serve to distinguish among them. A frog's intention to catch a fly, for example, is an intention *to catch a fly*, and is ipso facto distinct from, say, the frog's intention to sun itself on the leaf of a lily. This consideration may encourage the following bold speculation: the fact about the teleology of the frog's fly catching mechanisms and the fact about the intentional object of its snaps *both* reduce to the fact that the frog's behavior is an evolutionary adaptation selected for catching and eating flies; which is, in turn, a fact that the selectionist account of the behavior's phylogeny may be supposed to entail. If that's right, then the transition from an adaptationist theory that explains the frog's

[2] I will take metaphysical realism with respect to the intentional (hence, a fortiori, with respect to the intensional) for granted throughout. It strikes me as not worth worrying about how intentional phenotypes arise in a population if one assumes that, strictly speaking, there aren't any.

behavior in terms of its effects on fitness, to a functional theory that explains the frog's behavior in terms of its teleology, to a psychological theory that explains the frog's behavior in terms of the content of its propositional attitudes amounts, in effect, to a reduction of intentionality to selection. This line of thought is not without its partisans, either in philosophy or in cognitive science at large

For every ointment, there's a fly; in the present it's not far to seek. The problem is that nothing about content or about teleology appears to follow *directly* from the assumption that fly-catching is an evolutionary adaptation in frogs. At a minimum, such inferences require the further, stronger, assumption that fly-catching behavior is an adaptation *for catching flies;* (i.e that catching flies is what the behavior was selected *for*). But `adaptation for…', `selection for…' and the like[3] are themselves intensional contexts (just like `belief that…' and `intention to…'.). A mechanism that's selected for catching flies is *not* ipso facto a mechanism that's selected for catching ambient black nuisances; not even if, either in this part of the woods or in general, all and only the ambient black nuisances are flies. This logical quirk distinguishes `selection for' from mere selection. If you select a mechanism that catches Xs, and if the Xs are Ys, then you *thereby,* select a mechanism that catches Ys. Selection is an extensional process, so it can't, as it were, `see' the difference between intentional states that are extensionally equivalent. But the analogous point *doesn't* hold if the topic is `selection *for*…' If you are selecting for Bs and Bs are Cs, it doesn't follow (and it needn't be true) that you are selecting for Cs. `Select' doesn't distinguish among extensionally identical states, but `select for…' does.[4]

All that being so, a blatant sort of circularity infects typical philosophical discussions of evolutionary theories of intentionality. Here, for example, is a passage from Sterelny and Griffiths

[3] `Reverse engineering' might well be included in this catalogue of EP concepts that depend, in one way or another, on the notion of *selection for*. To reverse engineer a biological mechanism is just to infer from evidence about its structure to hypotheses about *what it was selected for* (what it was an adaptation to) in the environment in which it evolved . Some pretty imposing claims are made for this methodology in the canonical EP literature. Thus Dennett (19xx. p.213): "…even at the molecular level, you just can't do biology without doing reverse engineering". If that were actually true, I suppose it would follow that nobody did any biology prior to the general acceptance of the Darwinian theory of evolution. Counterexamples, however, abound; Pasteur's name suggests itself, to say nothing of Harvey, Linneaus, Mendel and, come to think of it, Aristotle. In fact, in the period between Darwin's death and the `modern synthesis' of evolutionary theory with genetics in the 1940s, there was a developing scientific consensus that Darwinian evolution couldn't explain speciation after all. The main worry was that an accumulation of small, random variations of traits couldn't have happened fast enough to produce the observed variety of phenotypes in the geological time available. (What was lacking was, of course, the notion of a heritable mutation.) Biologists, however, continued to find employment throughout this long interval.

[4] There are various ways of drawing the of/for distinction, but for our purposes the differences between them don't matter. (I suspect they don't matter very much for *any* purposes.) For example, there's a usage according to which organisms (or their phenotypes) are what get *selected,* but what they gets selected *for* are some or other of their *traits* (or `properties'). It's perfectly ok to talk that way, so long as you bear in mind that traits are themselves individuated intentionally. Distinct traits can have the very same extensions; that is, they may be distinct traits of the very same organisms.

(1999):"…it is so hard to see how intentional information could be a property of physical systems that this has become one of the great stumbling blocks of contemporary philosophy of mind.." Fair enough so far, it seems to me; but then they consider an evolutionary approach, which they appear to view with favor.. "One of the most popular attempts to explain intentional content in scientific terms appeals to the evolution of the mind… a thought is about the things that evolution has designed it to be about… The teleo-semantic theory suggests that the thought PREDATOR has the intended content that there is a predator here and now because it was produced by mental mechanisms selected for detecting predators." (104) Sterelny and Griffiths are certainly right to that there's a lot of this sort of stuff around, but it's hopeless on the face of it. As just remarked `selected for…' (to say nothing of `has the intended content that…') is intentional, and I don't suppose that intentionality can explain itself. This point will recur, in a variety of guises, as our discussion goes along.

So the situation so far is this: either natural selection is a species of `selection for…', and is thus itself a kind of intensional process; or natural selection is a species of selection *tout court,* and therefore cannot distinguish between coextensive mental states. In the former case it may, but in the latter case it doesn't, provide an explanation either of the teleology or of the intentional content of the frogs' snapping.

In the literature on philosophical semantics, the present point is often formulated as the `disjunction problem'. In the actual world, where ambient black dots are quite often flies, it is in a frog's interest to snap *at flies* . But, in such a world, it is equally in the frog's interest to snap at *ambient black dots.* Snap for snap, snaps at the one will net you as many flies to eat as snaps at the other. Snaps of which the intentional objects are black dots and snaps whose intentional objects are flies both affect a frog's fitness in the same way and to the same extent. Hence the disjunction problem: what *is* a frog snapping at when it, as we say, snaps at a fly?[5]

Thus far: It's plausible that natural selection can account for (heritable) intentional properties of a creature's phenotype only if it can distinguish selection of creatures *that have* such properties from selection

[5] The disjunction problem is only one of the ways that the peculiarities of intensional indivduation can make trouble for the projects of evolutionary psychologists. In paradigm adaptationist explanations (why the peacock's tail is so big; why the giraffe's neck is so long) one can specify the phenotypic property that's in question *prior to, and independent of,* figuring out what it was selected for. But that won't be possible for intentional phenotypes if (as evolutionary psychologists like to claim) the content of an intentional state is itself a function of its selectional history. If the content of intentional states is constituted by what they're selected for, it can't also be (contingently) true that intentional states are selected for having the content they do. Not, anyhow, short of massive circularity. (More on this later).

of creatures *for having* such properties.[6] If that's right, we can take the line of thought a step further. It would seem that the relevant difference between *mere* selection and selection *for* has to do with the status of certain counterfactuals. For example, according to this suggestion, to claim that frogs were selected *for* snapping at flies is to say (first) that in this world, where the ambient black- dots-or-flies are generally flies, frogs that snap at them are selected; and (second) that such frogs would not be selected in (nearby[7]) counterfactual worlds where the ambient flies-or-black dots generally *aren't* flies (perhaps they're bee bees)[8]. So, now: can natural selection settle the issue between these counterfactuals?

I can think of two ways in which it might be supposed to do so. Both crop up, more or less explicitly, in the philosophical literature, but I'm going to argue that neither of them has a prayer of working. I haven't heard of other alternatives and I can't prove that there are none. But I do rather think that these two exhaust the options. I am even prepared to wager moderate sums on that. (However, see note 24).

First option: Mother Nature

There's a sort of analogy between what natural selection does when it culls a population and what breeders do when they select from a population those members that they encourage to reproduce. This analogy was emphasized by Darwin himself, and it has been influential in the popularizing adaptationist literature ever since. Suppose Granny breeds zinnias, with the intention of selling them on Market Day. Then Granny is selecting zinnias for their value on the market, and not, say, for the elaboration of their root-systems. This is so even if, as a matter of fact, it's precisely zinnias with elaborate root-systems that sell at the best prices. Likewise, the fact about her intentional psychology that explains which ones Granny

[6] Compare (eg.) Sober (200) p. 68: "If the individuals with trait A survive to adulthood more often than the individuals with trait B, this is evidence that A is fitter than B." That is the usual way of describing the relation between the *individuals selected* and the *traits they're selected for*; but it's clearly untrue unless the observed asymmetry holds not just for the actual individuals, but also for the (nomologically) possible ones. This is hardly surprising. To claim that creatures are selected for being t is to claim (inter alia) that their being t was causally involved in their having been selected. But inferences from observed correlations to causal involvements are *always* contingent. It is thus perfectly possible that most (or all) of the individuals selected were in fact t but that none of them was selected for being t; and that whether a creature is t has no effect whatever on a creature's fitness. (Consider the case where it is not being t but being t' that was selected for, but where t and t' are extensionally equivalent in the observed cases.)

[7] As usual in `possible world' discussions of counterfactuals, the `nearby' does most of the work; and, also as usual, it rests more or less without explication.

[8] It needn't be claimed that the difference between the counterfactuals *constitutes* the difference between selection and selection for. Nor need it be claimed that, metaphysically speaking, the select/select for distinction grounds the counterfactuals rather than the other way around. But I am assuming that a theory that can't choose between such counterfactuals likewise can't distinguish selection from selection for. I take it that this is relevantly uncontroversial as claims go in this line of work.

chooses when she sorts her zinnias is that she is interested in selling them, and *not* that she is interested in their having lots of roots. (Granny may not even know about the connection between market values and root systems. Or, if she knows, she may not care.) In short, since Granny is in it for the money and not for the roots, there is a matter of fact about what she selects *for* when she selects some of the zinnias and rejects the others. What Granny selects for is: *whatever it is that she has in mind when she does her selecting.*[9]

So, then, perhaps we should take the analogy between natural selection and selective breeding at its face value. Perhaps we should say of natural selection just what we said of Granny: that what it selects for is whatever it has in mind in selecting? The counterfactuals fall out accordingly: If Granny is interested in high market value rather than big roots, that decides what she would do in a world where the salable zinnias are the ones with short roots, or no roots, or green roots with yellow polka dots, or whatever. Likewise, if natural selection has it in mind that there should be lots of frogs that catch flies, then, in the actual world, where the flies or bee bees are mostly flies, it favors *both* frogs that snap at flies and frogs that snap at flies-or-bee bees. But in the counterfactual world where the flies-or-bee-bees are mostly bee bees, natural selection will favor only the frogs that snap at flies.[10]

That, surely, is the thought that explains the prominence of anthropomorphized avatars of natural selection in the EP literature: Mother Nature, The Blind Watchmaker, The Selfish Gene or, for that matter, God. All of these are supposed to be (as one says); `intentional systems': they have intentions in light of which they act.[11] So, if one construes natural selection on the model of selection by an intentional system, one thereby makes room for a distinction between selection that has it in mind to propagate frogs that snap at flies and selection that has it in mind to propagate frogs that snap at flies-or-bee-bees; which is, I'm

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[9] Compare a small but consequential slip that Elliot Sober makes (1993, p.18) . "[An] obstacle that Darwin had to overcome [in using artifical selection as a model for natural selection] was *consciousness* [sic]. Artificial selection is the product of intelligent manipulation. Why think that organisms could be adapted to their environments without this sort of guidance." But the relevant consideration isn't either that the process of artificial selection is intelligent or that it's conscious; it's that it's *intentional*. Perhaps God is stupid or Granny is a robot (no *xonscious* intentional states). The logic of the situation remains unchanged so long as their selecting is performed *with an end in view*. I think the failure to keep these distinctions clear is an important reason why the problems about natural selection with which this paper is primarily concerned have gone so generally unnoticed.

[10] Or, rather, it favors them in `nearby' counterfactual worlds. Presumably natural selection prefers fly-or-bee bee snappers to fly snappers in worlds where bee bees are edible. (See note 5).

[11] But that repetition dulls sensibility, one should find the proliferation of such theories very puzzling. Nobody thinks it would be a good idea to postulate a `Granny Gravity' whose preference for objects that accelerate at thirty two feet per second squared when they are unsupported in a vacuum explains why so many objects (would) do that in such circumstances. Nor are we urged to marvel at the ingenuity with which these objects have `solved the problem' of falling in exactly that way in such circumstances. What, one might well wonder, is supposed to be the salient difference between Granny Gravity and Mother Nature?

supposing, precisely the distinction that you need room for if you are going to make sense of selection for beliefs, desires, goals and the like.

When it's put that baldly, however, it's perfectly obvious what's wrong with this line of thought: *natural selection doesn't have a mind;* a fortiori, it has nothing in mind when it selects among frogs.[12] Likewise, if genes were intentional systems, there would be an answer to, for example, the question whether natural selection favors creatures that really do care about the flourishing of their children or creatures that really care only for the propagation of their genotypes. All you would have to do, if you want to know, is find out which phenotype their genes prefer.

Except, however, that genes don't have preferences. The logic of all these cases is always the same: what's *selected* underdetermines what's selected *for* because outcomes *always* underdetermine intentions. But if genes are themselves intentional systems, or if there is a Mother Nature who selects with ends in view, then which creatures are selected can after all determine which traits they are selected for. That's the good news. The bad news is that, unlike natural selection, Mother Nature is a fiction, and fictions can't select things, however hard they try. Nothing cramps one's causal powers like not existing. Likewise, mutatis mutandis, the genes that make you cause your children to flourish (if, indeed, there are such genes) couldn't care less about why you want your children to do so. They couldn't care less about that because they don't care at all about anything.

Only agents have minds, and only agents act out of their intentions, and natural selection isn't an agent. To the contrary, it's an important part of the advertising for adaptationism that its way of explaining why the selection of phenotypes generally tends towards increasing fitness *doesn't* require attributions of agency. Because that's so (and assuming that it's true), adaptationism can legitimately claim to advance the scientific program of naturalizing nature.[13]

You may think the preceding speaks without charity; that I am, in fact, shooting in a barrel that that contains no fish. Surely, you may say, nobody could *really* hold that genes are *literally* concerned to replicate themselves? Or that natural selection literally has goals in mind when it selects what it does? Or

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9. For example, it belongs to (as one used to say) the logic of intentions that they are sometimes thwarted. Well, could Mother Nature intend to select for F *but fail to do so?* Do such failures depress her a lot?

[13] Speaking as a fully signed up atheist, I can't see much difference between claiming that God intelligently selects for fit phenotypes and claiming that Mother Nature does. So I find it puzzling that many of my co-religionists insist on that distinction with such vehemence.

that it's literally run by an intentional system? Maybe.[14] But, before you deny that anybody could claim any of that, please do have an unprejudiced read through the EP literature.[15] Meanwhile, I propose to consider a different way of arguing that adaptationism, because it can support the counterfactuals that distinguish mere selection from selection for can likewise distinguish fly-snapping frogs from fly-or-bee-bee snapping frogs; thereby providing a paradigm for selectionist accounts of the content (and the teleology) of intentional states.

Second option: laws of selection

Laws can support counterfactuals.[16] Arguably, that's what makes laws different from mere true empirical generalizations. So, then, suppose there is a law from which it follows that t1s are selected in competitions with t2s (where *being t1* and *being t2* are traits that have intentional properties).16 It's truistic that, if there is such a law, then it holds in all nomologically possible situations; which is to say that it determines the outcome of any nomologically possible t1 v. t2 competition. That includes competitions that are merely counterfactual, so long as they are nomologically possible. None of that should seem surprising (or at least none of it should on the assumption that laws are relations between properties and that properties don't have to be instantiated in order to be real.) So then, if there's a nomic relation between the property of *being a t1* and the property of *competing successfully with t2s*, and if the distinction between selection *of* t1s and selection *for being* t1s turns on the corresponding counterfactuals, then laws of selection might vindicate the selection/selection for distinction. A consummation devoutly to be wished, or so I'm often told.

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[14] Admittedly, the tactic of resorting to scare quotes when push comes to shove (as in `what natural selection `prefers,'' `what Mother Nature `designs,'' `what the selfish genes `want'' and so forth) can make it hard to tell just what is being claimed in some of the canonical texts. Still, there are plenty of apparently unequivocal passages. Thus Pinker (1997, p.93).: "Was the human mind ultimately designed to create beauty? To discover truth? To love and to work? To harmonize with other human beings and with nature? The logic of natural selection gives the answer. The ultimate goal that the mind was designed to attain is maximizing the number of copies of the genes that created it. Natural selection cares only about the long-term fate of entities that replicate….." Fiddlesticks. The human mind wasn't created, and it wasn't designed and there is nothing that natural selection cares about; it just happens. This isn't Kansas, Toto.

[15] A very recent example: "[Darwin} argues by example, not analogy; the point of the the opening of `The Origin' isn't that something similar happens with domesticated breeds and natural species; the point is that the very same thing happens, albeit unplanned and over a much longer period." (Gopnik, A. `Rewriting Nature, Charles Darwin, natural novelist, The New Yorker, 23 Oct, 2006, p.56.) You might have thought that the caveat deserves some explication; how *could* a studied decision to select for one or other trait be `the very same thing' as the adventitious culling of a population? If that's not just an analogy, what would be? Gopnik doesn't say.

Well, are there such laws? I think it's most unlikely.

It's a thing about laws that they aspire to generality. In the paradigm cases, a law about Fs is supposed to apply to all instances of F per se[17]. Conversely, to the extent that a generalization applies not to Fs per se, but only to Fs-in-such-and-such circumstances, it's correspondingly unlikely that the generalization is a law (or, if it is a law, it's correspondingly unlikely that it's a law about Fs.) I take that to be common ground; but if it's right, then quite likely there aren't laws of selection. That's because *who wins a t1 v. t2 competition is massively context sensitive.* (Equivalently, it's massively context sensitive whether a certain phenotypic trait is conducive to a creature's fitness.) There are a number of respects in which this is true, some obvious some less so.

For example, it's obvious that no trait could be adaptive *across the board.* Rather, the adaptivity of a trait depends on the ecology in which its bearer is embedded. In principle, if a trait is maladaptive in a certain context, you can fix that *either* by changing the trait *or by changing the context.*[18] Is a creature's being green good for its fitness? That depends on whether the creature's background is green too. Is being the same color as its background good for a creature's fitness? That depends on whether the camouflage that makes the creature hard for predators to find also makes it hard for the creature to find a mate. Is it good for a creature's fitness to be big? Well, being big can make it hard to flee from predators. Is it good for a creature to be small? Perhaps not if its predators are big. Is it good for a creature to be smart? Ask Hamlet. And bear in that when selection has finally finished doing its thing, it's more than likely that the cockroach will inherit the earth. Whether a trait militates for a creature's fitness is the same question as whether there's an `ecological niche' for creatures that have the trait to occupy; and that *always* depends on what else is going on in the neighborhood. Is it good to be a square peg? Not if the local holes are mostly round.

I want to emphasize that my point *isn't* just that, if there are laws about which traits win which competitions, they must be `ceteris paribus' laws. To the contrary, I take it to be true quite generally that special science laws hold only `all else equal'. If that's so, it's not a complaint against the putative laws of selection that they do too. I think, however, that the present considerations go much deeper.

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[17] That's to say: it's supposed to apply to Fs *in virtue of their being Fs.*

[18] Some biologists have claimed to descry `trends' in evolution: that is, traits which will be selected for in almost any ecological situation. But if there are such, they must be very coarse grained (increased size and increased complexity are typical candidate.) And, as Mayr remarks "almost all trends are not consistently linear, but change their direction sooner or later, sometimes repeatedly, and they may even totally reverse their direction" (p. 218)

To a first approximation, the claim that, *ceteris paribus* Fs cause Gs says something like: `*given independently justified idealizations.* Fs cause Gs.'[19] The intuition in such cases is that, underlying the observed variance, there is a bona fide, reliable, counterfactual-supporting relation between being F and causing Gs, the operation of which is obscured by the effects of unsystematic, interacting variables. The underlying generalization comes into view when the appropriate idealizations are enforced. By contrast (so I claim) there just aren't any nomological generalizations about which traits win competitions with which others. It's simply isn't true, for example, that being big is in general better for fitness than being small except when there are effects of interacting variables; or that flying slow and high is in general better for fitness than flying fast and low except when there are effects of interacting variables; or that being monogamous is in general better for fitness than being polygamous *etc* except when there are effects of interacting variables… etc. It's not that the underlying generalizations are there but imperceptible in the ambient noise. It's rather that there's just nothing to choose between (eg.) the generalization that being small is better for fitness than being big and the generalization that being big is better for fitness than being small. Witness the fact that the world contains vastly many creatures of both kinds.[20] I don't doubt that there are explanations of why competitions between creatures with different traits come out the way they do; but such explanations don't work by subsuming the facts they explain under general laws about the relative fitness of the traits.[21] (I'll say something, pretty soon now, about how I think they actually do work.)

Nor is that by any means the whole story about the context dependence of *being a trait that's selected for*. In fact, strictly speaking, *traits* don't get selected at all; traits don't either win competitions or loose them. What wins or looses competitions are *the creatures that have the traits.* That's to say that what's selected is

[19] It's crucial that the idealizations are independently justified; otherwise `ceteris paribus Fs cause Gs' collapses into `Fs cause Gs except when they don't.'

[20] Compare Sober (1993): "Evolutionary biology has developed… a system of models that describe the consequences for fitness of various traits.84) I think that this is simply untrue; what it has developed is a system of models that explain why, de facto, certain-traits-affect-fitness-in-certain-circumstances. Sober offers the example "in which heterozygote superiority explains a balanced polymorphism (ibid)." But this example cuts the wrong way since `heterozygote superiority' is itself thoroughly context dependent; sometimes it's conducive to fitness, sometimes it's not.

[21] It bears emphasis that assuming the laws in question to be probabilistic wouldn't help. The problem is that `it's probable that…' is itself extensional and is thus unable to reconstruct the intensonlity of 'selection for….' If being F is probably conducive to fitness, and F are G are coextensive, then being G is equally probably conducive to fitness; total gain, no yardage. Compare Sober (1993): "Evolutionary biology has developed… a system of models that describe the consequences for fitness of various traits.84) I think that this is simply untrue; what it has developed is a system of models that explain why, de facto, certain-traits-affect-fitness-in-certain-circumstances. Sober offers the example "in which heterozygote superiority explains a balanced polymorphism (ibid)." But this example cuts the wrong way since `heterozygote superiority' is itself thoroughly context dependent; sometimes it's conducive to fitness, sometimes it's not.

whole phenotypes; and, quite possibly, whether a trait is fitness-enhancing depends a lot on what phenotype it's embedded in. That too is practically a truism; but it's one that game-theoretic models of evolution (for example) have a bad habit of ignoring. `What would happen if a population of ts were to invade a populations of not-ts?' That depends a lot on what *other* differences distinguish the *t*s from the not-*t*s. `Yes, but *all else equal* what will happen if a population of *t*s invades a population of not-*t*s?' Since `all else' never is equal, the question doesn't seriously arise. Unlike a scientist in a laboratory, natural selection can't control for confounding variables; it has no access to the method of differences.[22]

`Come *on.* Do you seriously mean to say that we can *never* explain why a population of creatures that are *t*-inter-alia wins competitions with populations that aren't *t*?'. No; but I do think that such explanations are inevitably post-hoc. (More on this presently.) Evolutionary processes can select for a given phenotypic trait only insofar as its interactions with other phenotypic traits can be discounted.[23] How far is that? It's likely to depend on the details. To put this another way, *`being a trait that's selected for'* pretty plausibly doesn't pick out a natural kind. And, to put *that* another way, it seems unlikely that there are laws about traits-that-are-selected-for *as such.* [24]

The alleged conclusion: Contrary to Darwinism, the theory of natural selection can't explain the distribution of phenotypic traits in biological populations.

[22] To employ the `method of differences' is to find out whether it's their being F that explains why Fs cause Gs or whether it's their having some other property that's confounded with their being F. One does this by examining situations in which , as far as one can tell, all the (relevant) ceteris paribus conditions are satisfied. Typically such situations don't occur outside the experimental laboratory.

[23] Just as, by the way, `reinforcement' can select a mental state only if it can `see through' the performance mechanisms that mediate the behavioral expressions of the state. `What is selected for' is a lot like `what is reinforced'; if you're both a behaviorist and a Darwinian, you're likely to think of selection as Mother Nature's way of reinforcing a trait. In any case, since both contexts are intensional , neither supports direct inferences from effects to their causes.

[24] You might suppose that, at very least, there are laws about whether a trait is fitness-enhancing (hence selected for, all else equal) in a creature's modal (/normal/whatever) environment. But environments are just like ecological niches; their individuation is intensional; what a creature's environment is depends on which creature it is. Suppose gazelles and slugs live side by side. Still, it may be that tigers count as part of the environment for the one but not for the other; hence that a trait that tigers and slugs share may be selected for in the one but irrelevant to fitness in the other.

The putative argument:

(i) To do so would require a notion of `selection for' a trait. `Selects for….' (unlike `selects…`) is opaque to substitution of co-referring expressions at the `…' position.

(ii) If T1 and T2 are coextensive traits, the distinction between selection for T1 and selection for T2 depends on counterfactuals about which of them. The truth makers for such counterfactuals must be either (a) the intensions of the agent that affects the selection, or (b) laws that determine how the relative fitness of having the traits *would be* selected in a *possible* world where the *actual* coextension doesn't hold.[24]

(iii) But:

Not (a) because there is no agent of natural selection.

Not (b) because considerations of contextual sensitivity make it unlikely that there are laws or relative fitness (`laws of selection').

QED

24. Might anything else be relevant? It's been suggested to me a couple of times that an example of Elliot Sober's (1993) provides a third option. Roughly, Sober imagines a mixed batch of pebbles, some square and some round, that is run through a filter all the holes of which are round. Suppose that all the round pebbles are red and all the square ones are some other color. Then the filter will pass only red objects even though, as Sober points out, there is a strong intuition that what this device sorts *for* is not color but shape.

One might claim, in the case imagined, that what underlies these intuitions is, as usual, the counterfactuals (green round marbles would have gone through had there been any) but that the counterfactuals are covered by an, as it were, a *very local* law whose domain is restricted to this kind of machine. But that strikes my ear as forced, and it's anyhow unnecessary: Rather, what grounds the counterfactuals in Sober's example is *the structure of the mechanism*; given how it works, it lets the round pebbles through but no others; one's intuitions about which trait is *selected for* follow not from what laws of selection per from mechanics. Notice, for example, that whereas competition plays a central role in the explanation of every bona fide Darwinian selection, it plays no role at all in explaining how Sober's machine sorts for round marbles. Sober's machine would work exactly the same way even if there were only one marble for it to sort.

I think that Sober's treatment of the `selection of/ selection for' distinction is muddled by his not attending to the distnction between `causes' and `causally explains'. Sober says that "'selection of' pertains to the *effects* of a selection process, whereas 'selection for' describes its *causes* [sic]"(1993 p. 100.)" But, in fact, the salient difference is that `selects' is extensional, but `selects for' isn't. If you *select* elephants and elephants live in Africa, you thereby select things that live in Africa. By contrast, you *select for* such-and-suches *in virtue of their being Fs* (where, typically, *being F* is a property in virtue of which the things selected fall under a law). So, it's untenditious (for present purposes) that elephants were selected, and it *may* be that they were selected for having big ears; but, almost certainly, they weren't selected for living in Africa. That would still be so *even if `lives in Africa* and *has big ears* were coextensive properties.

Pace Sober, the of/for distinction quite clearly *isn't* about causes versus effects; for, though it's true that elephants' having big ears is the effect of big ears having been selected for, it's equally true that big ears having been having been selected for was the cause of elephants' having big ears.

As we're about to see, this sorts of argument applies to adaptationist explanations across-the-board; but it applies in spades when what's at issue is selection for *intentional* states. That's because, unlike any others, intentional states have unintended consequences, and natural selection can't see the difference between a consequence that is intended and a consequence that isn't.[25] `Jack and Jill/ Went up the hill/ To fetch a pail of water/ Jack fell down/ And broke his crown/ And thus decreased his fitness.' *We* can see that what was detrimental to Jack's fitness was neither his intention to fetch water, nor his intention to climb a hill in order to do so. It was the falling down that was bad for him, and that wasn't part of the intention on which he acted. Since we can see all that, we're prepared to conclude that, although Jack's action brought him to grief, evolution shouldn't count its having done so as a reason for selecting against mental states whose intentional objects are climbings of hills or fetchings of water. Jack's climbing the hill eventuated in the damage to his crown; but it wasn't, as one says, `intentional under that description.'

But to suppose that the processes of evolution can see that the actual outcome of Jack's action was incidental to its intentional object is precisely to beg the questions that's are now at issue. We can understand went wrong with Jack because we have the concept of `the maxim of an act', and it's clear to us that the maxim of Jack's act was something like `when thirsty, fetch water' and nothing at all like `when thirsty, fall down/ and break your crown'. But, recall that (putting aside the loose talk about what evolution can `see`) the adaptationist's aim was to explain how the fitness of an intentional state varies *as a function of its content.* So, if he's to avoid circularity, he can't take for granted either that intentional states with distinct effects on fitness are ipso facto distinct in content or that states that are distinct in content are ipso

[25] I should emphasize that the phenomenon of unintended consequences is proprietary to intentional states and is unavoidable whenever they're at issue. So, for example, intending the world to end and not caring about global warning are patently distinct mental states; accordingly selecting for one must be distinguished from selecting for the other even *if their effects on fitness turn out to be exactly the same.* Any attempt to apply the notion of selection to intentional states will have to face this sort of issue. We all know where the best laid plans of mice and men oft gang.

facto distinct in their effects on fitness. Jack's crown got broken and Jill's didn't. It remains *entirely possible* that they both acted with the very same end in view,

I hope it's clear that I've thus far been running two kinds of arguments in tandem; two kinds of arguments that happen to converge in the case of issues about evolutionary psychology. The first concerns the goals of evolutionary psychology in particular; it's that data about effects on fitness can't, even in principle, distinguish the selection of any given intentional state from the selection of any other intentional state with the same outcomes. What's making the trouble here is the intensionality of the mental: Beliefs, desires and the like are individuated not by the consequences of having them but by their contents, and these come apart when (or to the extent that) the actual effects of being in such a state aren't the effects intended. This is a dilemma if only differences in their *actual* effects can distinguish the mental state that is selected for from indefinitely many coextensive alternatives; but there's a way out on the assumption that mental states can be distinct in virtue (solely) of differences in their *counterfactual* effects. I think some such thought is implicit in quite a lot of the selectionist literature. It would be plausible were there a plausible account of the truth-makers for these counterfactuals.[26] But there isn't.

To repeat, this kind of worry arises because of the intensionality of mental traits. But there is another and independent kind of worry that derives from the intensionality of the notion of *selecting for* a trait, mental or otherwise: That's because which trait a phenotype is selected for depends on which phenotype would be *selected-tout-court* in appropriate counterfactual situations. Selection for propositional attitudes has both of these worries at once.

Because `selection for' is intensional, so too are a galaxy of adaptationist concepts that are defined in terms of it including, notably, that of a `problem of adaptation'[27]; the very same configuration of the

[26] The analogy to the famililar polemics about behaviorism is exact. You can't distinguish mental states by their consequences for reinforcement because mental states that differ in content may cause the same behaviors and vice versa. The sort of behaviorism that Ryle made familiar wanted to identify mental states with dispositions precisely because dispositions are individuated by (inter alia) their counterfactual outcomes.

[27] Sometimes the notion of a `problem of adaptation` is taken for granted and other notions in the adaptationist cluster are introduced in terms of it. This changes which shell the pea is under, but it does nothing useful. Thus Sterelny and Griffiths (199, p. 32) "Organisms in a population vary. Some variants will be better suited to dealing with the problems presented by their environment than others. These variants are more likely to survive to reproduce…."

This sounds harmless, but whether it's true depends on how problems-with-the-environment are individuated; a problem is an intentional object, just like an adaptation or a trait that's selected for. Tree-climbing animals have a problem about getting up trees; borrowing animals don't. But why is that? Why isn't burrowing a moles' way of solving a tree-tree climbing problem; viz by ignoring it. One is, of course, inclined to say: `It's because moles don't even *try* to get up trees'. Quite so, but `try to' is intentional too.

environment may present a problem of adaptation to one kind of creature but not to another even though the creatures live side by side.[28] And, just as one would expect, the intensionality of ecological problems makes their individuation deeply obscure. In familiar cases, solutions are defined by the problems that they solve. The order of metaphysical dependence is that keys solve the problem of finding something to open locks, not that locks solve the problem of finding something to be opened by keys. In adaptationist theory, by contrast, there's a sort of topsy-turvey: Which problem a creature's phenotype is the solution to depends on which traits of the phenotype was selected for solving it. But that there are spiders, who would have guessed that *how to spin webs to catch flies* is an ecological problem? Or that there are creatures whose fitness is a consequence of their having solved it? Because selection for is intentional, a range of questions of which a theory of adaptation ought to be responsive are, in fact, answered post hoc.[29] (More on this presently).

The long and short is that the intensionality of the attitudes and the intensionality of selection for both raise problems of individuation, but the first kind of problem is much less of a worry than the second. A reasonable biologist might be willing to live without a selectionist evolutionary psychology so long as there's no implied threat to adaptationism per se. When the weather gets rough, there's an understandable temptation to lighten ship by throwing the psychologists overboard. But, in fact, doing so doesn't help; the intentionality of *selection for* makes trouble for adaptationism *as such*, and it would continue to do so even if, in our panic, we were to adopt some sort of behaviorism, or neurological reductionism, thereby making intentional psychology disappear.

Exasperation may now urge the following response. `Why shouldn't I think that that's all just epistemology pretending to be metaphysics? And for you, of all people [that is, for me of all people] to conflate epistemology with metaphysics really is the last straw. Isn't that the very mistake that you're forever warning everyone against? Aren't you always saying: `what it is is one thing; how we know what it is is quite another?' Aren't you, in short, ashamed of yourself?

`What you've offered,' (exasperation continues) `isn't grounds for claiming that there are no laws of selection. At most it's grounds for claiming that, if there are such laws, then, because of their context dependence, they must be very complicated; perhaps, even, they're not within our capacity to formulate.

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[28] Likewise the concept of an `ecological niche' about which more later.

[29] There are examples wherever you look. So, it's often suggested that the reason there are so many diseases of old age is that creatures can't compete for representation in the gene pool once they become infertile. But then, why didn't selection increase the length of the fertile period?

But nothing of metaphysical interest follows from that. In particular, nothing follows as to the status of counterfactuals about which phenotypes would, and which ones wouldn't, be selected in possible worlds other than our own. Laws that are too complex for us to formulate can support counterfactuals all the same.

`After all, do you really want to say that adaptationist explanations aren't *ever* any good; that selection histories never explain phenotypic traits, psychological or otherwise? Surely you're aware that the textbooks simply team with good examples to the contrary. These textbook explanations purport to, and often clearly do, give reasons why phenotypes are the way they are; why there are lots of populations of T1s, but few or no populations of T2s. Well, there can't be such explanations unless there are laws about the relative selectability of various traits. Since you can't have the explanations without the laws, and since the illumination that the explanations often provide isn't subject to serious doubt, it would seem that if you don't like laws of selection, you will somehow have to lump them.

`To recapitulate,' exasperation concludes, `if there are successful adaptationist explanations, there must be laws of selection; and if there are laws of selection they will ipso facto serve to vindicate the counterfactuals that distinguish selection tout court from selection for. And, on your own [my own] account, it's precisely the distinction between selection and selection for that a viable evolutionary psychology requires to vindicate it's claim that cognitive traits are selected for their intentional contents. So tell me again: what, precisely, is your case against the possibility of an evolutionary psychology?"

Thus the voice of exasperation, and I think there's a lot in what it says. Certainly I have no objection to the form of its argument: If there are few or no examples of laws of selection on offer, that could be because there are few or no such laws; or it could be that, there are lots and lots of them but we aren't smart enough to find them out. And it's quite true that I disapprove, vehemently, of arguments that purport to draw metaphysical conclusions from epistemological premises. Still more vehemently do I disapprove of ignoring what otherwise seems to be successful science on the grounds of merely philosophical scruples.

On the other hand, it's crucial in the present case not just that there are bona fide successful adaptationist explanations, but also that such explanations are bona fide nomological. If they aren't, then the success of the explanations is not a reason to think that there are laws of selection. In fact, I'm inclined to think that explanations of phenotypes in terms of their selection histories generally aren't nomological and that they don't claim or even aspire to be. What they are is precisely what they seem on the face of them; they're historical explanations. Very roughly, historical explanations offer not covering laws but plausible narratives; narratives which (purport to) articulate the causal chain of events leading to the event that is the explanandum. Covering law explanations are about (necessary) relations among properties; historical

narratives are about (causal) relations among events[30.] That's why the former support counterfactuals, but the latter don't.[31, 32]

Historical explanations are as far as I know, often perfectly ok. Certainly they are sometimes thoroughly persuasive, so perhaps they are sometimes true. But, prima facie at least, historical explanations don't seek to subsume events under laws. `She fell because she slipped on a banana peel.' Very likely she did; but there's no law ---there's not even a statistical law--- that has `banana peel' in its antecedent and `slipped and fell' in its consequent.[33] Likewise, Napoleon lost at Waterloo because it had been raining for days, and the ground was too muddy for cavalry to charge. So, anyhow, I'm told; and who am I to say otherwise? But it doesn't begin to follow that there are laws that connect the amount of mud on the ground with the outcomes of battles.

I suppose, metaphysical naturalists (of whom I am one) have to say that what happened at Waterloo must have fallen under some covering laws or other. No doubt, for example, it instantiated (inter alia) laws of the mechanics of middle-sized objects. But it doesn't follow that there are laws about mud so described, or about battles so described, still less about causal connections between them so described; which is what

[30] I hope you find the examples I'll offer plausible, but nothing much turns on them. The basic point is that `…causes… is transparent and `…is nomicallly connected to… ' isn't. That being so, there are bound to be true causal histories that aren't couched in a projectible vocabulary. It's arguable that every cause-effect pair must instantiate a law *under some description or other*. But not under every *true* description; or even under every true description that is scientifically illuminating. (This general idea is owing to Davidson, 19xx).

[31] Here's an entirely typical instance of the kind; it's drawn from a discussion of the evolution of neural specialization: "The flatworm's rudimentary division of neural labor, was the first step toward an avalanche of specialization that has given rise to the complex neural systems of vertebrates… First, shortly after vertebrates came on the scene, intercellular communication got a whole lot better, with the evolution of glial cells …. William Richardson 's research group has speculated that glial cells evolved as modification of motor neurons…. They further suggested that such glial cells could have immediately conveyed a large adaptive advantage by making it possible for prey to more rapidly escape their predators. It could, of course, have been the other way around…. (Marcus, ll6-117)" There are hypotheses about the *sequence* of phylogenetic alterations, and about the selectional advantages that each step in the sequence might have conferred. But there's nothing that sounds remotely like an attempt to frame a covering law. It isn't claimed, for example, that it's nomologically necessary that neurons always (or generally) milinate in the environment of selection; or that phenotypes with milinated neurons always (or generally) win competitions with phenotypes that lack them. What *is* suggested is that, as a matter of fact, there were such competitions, that the milenated phenotypes won, and that it is intelligible (in retrospect) why they did so. What's on offer as an explanation is a historical narrative, not a Hempelian deduction from generalizations to their instances.

[32] Strictly speaking, of course, historical narratives aren't even of the right *form* to provide support for counterfactuals; you need *quantified* propositions to do that; eg. propositions that quantify over both actual and possible states of affairs. Indeed, it's *because* quantified propositions can support counterfactuals and causal narratives can't that there's a problem about induction: How can premises about what did happen justify conclusions about what always happens (and hence about what would happened if … .) Didn't Hume say something of that sort?

[33] I've borrowed this example from Steven Schiffer, who uses it to argue that intentional explanations themselves aren't of the covering-law sort.

would be required if `he lost because of the mud' is to be an instance of a covering-law explanation. It likewise doesn't follow, and it isn't remotely plausible, that whatever explains why Napoleon lost at Waterloo likewise explains why Nelson won at Trafalgar; i.e. that there are laws about the outcomes of battles as such, of which Nelson's victory and Wellington's are both instances. `Is a battle' doesn't pick out a natural kind; it's not (in Nelson Goodman's illuminating term) `projectible`.[34]

Likewise, I suppose that when a t1 creature competes with a t2 creature, *some laws or other* must govern the causal interactions between them. The question, however, is whether they are laws *about competitions;* or, indeed, whether they are even laws of biology. I don't imagine Darwin would be pleased if it turned out that, thought there is indeed an explanation of the mutability of species, it exploits not the concepts of competition, selection and the like, but in (as it might be) the vocabulary of quantum mechanics.[35]

It's of a piece with their not appealing to covering laws that historical-narrative explanations so often seem to be post hoc. The reason they so often seem to be is that they usually are. Given that we already know who won, we can tell a pretty plausible story (of the too-much-mud-on-the-ground variety) about why it wasn't Napoleon. But, what with their being no covering law to cite, I doubt that Napoleon or Wellington or anybody else could have predicted the outcome prior to the event. The trouble is that there would have been a plausible story to explain the outcome whoever had won; prediction and retrodiction are famous for exhibiting this asymmetry. That being so, there are generally lots of reasonable historical

[34] In the course of events, Miss Peahen was attracted to Mr. Peacock because of his conspicuous tail. It doesn't follow that there are laws about conspicuous tails, or about creatures with conspicuous tails, *as such*, i.e. laws that apply, ceteris paribus, both to peacocks with conspicuous tails and to mice with conspicuous tails and to conspicuous tails that have become detached…etc. It's a mistake to think that historical narratives convert into covering law explanations *salve* their vocabulary. The vocabulary of nomological generalizations is *ipso facto* projectible', but the vocabulary of historical explanations needn't be and usually isn't.

[35] The issues here runs exactly parallel to ones that are familiar from the philosophy of psychology. What's required in order to vindicate belief/desire psychology is that the laws that govern behavior are laws *about beliefs and desires as such;* neurological laws, or quantum mechanical laws, wouldn't suffice. Compare Bunzl (2004, p.7): "… just because historians don't explain events by reliance on laws of history, it does not follow that some explanations don't draw on laws of specific disciplines… the assassination of Archduke Ferdinand involved a shot that was `governed by the generalizations of physics.":. Quite so; but since the covering laws of physics don't apply to events *qua assassination*, the explanations they afford abstract from precisely the aspects of events that historians care about. What was interesting about Ferdinand was that he was assassinated, not that his mass was increased by the weight of a bullet. (For discussion, see Fodor, (Special Sciences) and (Reply to Kim.) Bunzl remarks, at one point that "implicit in every causal assertion, there is a set of counterfactual implications. (13)" But that's very misleading; what supports counterfactuals aren't singular causal truths per se but the laws that subsume them. The nice lady's throwing the switch caused the lights to go on. But it's not implied that if the nice lady hadn't thrown the switch the lights wouldn't have lit; or that if the nice lady had thrown some other switch they would have; or that if a nasty lady had thrown the switch… etc. What sustain the counterfactuals is the corresponding nomological generalizations about the circuit: If circuit hadn't been closed, the lights wouldn't have gone on.

throwing the switch caused the lights to go on . supports the counterfactuals aren't causal relations per se but the laws that subsume the relations.

accounts of the same event, and there need be nothing to choose between them. Did Wellington really win because of the mud? Or was it because the Prussian mercenaries turned up just in the nick of time? Or was it simply that Napoleon had lost his touch? (And while you're at it, what, exactly, caused the Reformation?)[36]

It's not in dispute that competitions between creatures with different phenotypes often differ in their outcomes; and, of course, in each case, there must be some explanation or other of why the winner won and the looser didn't. But there's no reason at all to suppose that such explanations typically invoke laws that apply to the creatures in virtue of their phenotypic traits. That being so, there need be nothing to choose between claims about the corresponding counterfactuals. Small mammals won their competition with large dinosaurs. But did they do so because of their smallness? That depends (inter alia) on whether they would have won even if there hadn't been a meteor. I can tell you a plausible story about why they might have: Small animals are able to snitch dinosaur eggs to eat when the dinosaurs aren't looking, (which is bad for the diinosaurs' fitness.) On the other hand, I can tell you a plausible story about why they mightn't have won: lacking the meteor, there wouldn't have been selection for tolerance to climate change, which the mammals had but the dinosaurs didn't. (Notice that, according to the latter story, it wasn't the smallness or quickness of the mammals that was selected for, but the range of temperatures they were able to tolerate.)[37] So, which of the counterfactuals do our evolutionary narratives about the extinction of dinosaurs support? Neither? Both? And, likewise, what intentional content did evolution select for when it selected creatures that protect their young? Was it an altruistic interest in their offspring or a selfish interest in their genes? Well….

The moral is that the fact that a phenotype is selected doesn't determine which (if any) of its traits it was selected *for*. Quite generally, if you want to infer from the one to the other, you have two choices (and, as far as I can see, only two.) You can try attributing intentions to the agent of selection (hence Mother Nature); or you can try to find a covering law that connects its having some or other of its phenotypic traits with a creature's having been selected. The former tactic is hopeless; there simply isn't any Mother Nature,

[36] Dray (1964) acknowledges a "grain of truth" in the view that "… if two historians make different selections out of what is known… there is no need to conclude that either of them writes a *false* account. Nor, for that matter, need we strictly speaking, regard them as *contradicting* one another. It is therefore somewhat misleading even to say that they offer different answers to the same question. Their answers are better regarded as providing `contributions' to the history of the subject in review (31)" Drays book provides useful discussions of the sorts of differences between historical narratives and covering law explanations that I've been emphasizing here.

[37] Nor, however, do the complications end with this. One of the reasons for their relative resistance to climate change (compared to the dinosaurs) was that the mammals had relatively small surface areas (i.e. the mammals were *smaller* than the dinosaurs. So maybe the mammals won because they were smaller than the dinosaurs after all? It's precisely this kind of instability of the counterfactuals that suggests that there just aren't any laws that connect a creature's size *as such* to its success in competitions. (NB: it strongly suggests that; but, of course, it doesn't prove it.)

and natural selection has nothing in mind when it prefers some creatures to others; natural selection has nothing in mind *at all*. But the second tactic seems hopeless too, given the extreme context sensitivity of selection processes. Whether a trait is conducive to fitness appears to be just about arbitrarily dependent on which sort of creature it's a trait of and what sort of ecology the creature inhabits. If that's so, then there can't be laws of selection, and `is selected for' can't be a projectible predicate.

There is, however, a model of adaptationist explanation that seems to fit the facts pretty well. If it's otherwise viable, it suggests that such explanations, though they aren't nomic, have perfectly respectable precedents. Adaptationist explanations are species of historical narratives. If so, then everything can be saved from the wreckage except the notion of selection for, since historical narratives don't support counterfactuals, it's likely that selecting for can't be salvaged. That's all right; much spilled ink to the contrary notwithstanding, there very likely isn't any such thing.

The curious case of the ecological niche.

I remarked earlier on that the intensionality of *selection for* makes the individuation of a whole galaxy of adaptationist constructs problematic; the example of choice was the `evolutionary problem' to which a phenotypic trait is said to be a solution. Before we close up shop, I want to look at another example of the same kind; the concept of an ecological niche. It's bears separate attention because it is often to the fore in informal accounts of the adaptationist program. In effect, `ecological niche' and `law of selection' are more or less interdefined: there is an ecological niche for a phenotype with trait T iff it's a law that, all else equal, phenotypes with T are at selectional advantage with respect to otherwise identical phenotypes that lack T. That being so, the claim that there are such things as ecological niches and the claim that there are such things as laws of selection must sink or swim together, and one might expect to find individuation problems about the second to parallel the individuation problems about the first.[38] Still, one might consider,

[38] As indeed one does. Here's an example sufficiently spectacular to merit quoting at length: "Every species is adopted to a rather restricted selection of properties of the environment. This specific set of environmental properties provides a species with the required living conditions called it's *niche*. There are two ways to define a niche. The classical way is to consider nature to consist of thousands and millions of potential niches occupied by the various species adopted to them. In this interpretation, the niche is a property of he environment. Some ecologists, however, consider the niche to be a property of the species that occupies it. For them the niche is the outward projection of the species that occupies it…. The large Sunda Islands… each have about 28 species of woodpeckers. Even though the tropical rain forest of New
Guinea … is remarkably similar; there is not a single woodpecker. Does this mean that there is no woodpecker niche in New Guinea? Definitely not. It would be quite misleading …to say that there are no woodpecker niches in New Guinea. Actually, the open niches are virtually calling for them, but woodpeckers are notoriously poor in crossing… large water gaps…. (Mayr, 2001 p.152)

This does strike me as remarkably confused even by the prevailing standards. The niche that's waiting to be occupied is all of a kind with the notorious headache that lurks in the parlor, waiting for someone to have it.

taking the former notion as prior in the order of explanation. The idea is to explain why there are relatively many phenotypes with a certain trait by claiming that there is an ecological niche that is available to creatures that have that trait but unavailable (all else equal) to creatures that don't. Perhaps that way of grounding the adaptationist program might avoid, at least in the short run, the worries about laws of selection that I've been complaining about.

One way to put the argument to here is that you can't infer from `xs have (trait) T and `xs were selected' to `xs were selected for having trait T'; not even if xs do in fact have t and that xs were in fact selected. Unsurprisingly, the same sort of point applies to the individuation of ecological niches. So, for example, you might have supposed that if an ecological niche is actually occupied, then it is ipso facto a possible ecological niche. If there is an ecological arrangement in which large tails are fitness enhancing (in peacocks), then there is an ecological niche for large-tailed peacocks. But, on second thought, no. :`Is an ecological niche for…' is intensional in exactly the same way, and for exactly the same reasons as `is selected for…': from the fact that large tailed peacocks predominate in a population, it doesn't follow that large tailedness was selected for in peacocks, or that there is or ever was an ecological niche for large tailed peacocks. To get those sorts of conclusions, we need a premise that specifies what sort of creature the niche is a niche *for*;. For example, the premise 'large tailed peacocks were selected *as such';* and notice, 'selected as such` is intensional, just like `selected for'. That F xs are selected as such and all and only the F xs are G xs, does *not* imply that G xs are selected as such. So, we're back where we started; the detour through ecological niches leads nowhere.[39]

But perhaps you don't like that sort of argument. So be it. I shall try to convince you (or at least to quasi-convince you) that it's independently implausible that there could be an unvacuous account of what makes a possible-or-actual state of affairs a possible-or-actual ecological niche for a possible-or-actual kind of creature. Why not? Well, for one thing, because there are *so many* environmental arrangements that have turned out to be niches, and (prima facie) the properties that make them so are wildly heterogeneous and context dependent, just like the properties that are selected-for.

[39] As usual, not attending to the intensionality of one's explanatory constructs eventuates in all sorts of silliness. Thus Jarrel Diamond (in his Introduction to Ernst Mayr's *What Evolution Is* Basic Books, 2001) wonders (rhetorically) 'How can one explain the remarkable adaptation of every species to its chosen niche? ' (p. x). Likewise Sober (1993) p.186: "The exquisite fit of organisms to their environments … is one of the central phenomena that the theory of evolution by natural selection attempts to explain." (See also passages from Dobzhansky quoted on p 49 of Sterelny and Griffiths.) But, so long as `degree of fitness' and `the organism's environment' are specified post-hoc, there's nothing here to wonder at except a *tautology*. If a certain creature fails to occupy a certain niche exactly, then it just *follows* that that isn't exactly the niche that the creature occupies. Imagine a research program directed to explaining why each creature fits so precisely into the corresponding hole in space. Would the NSF be well-advised to fund it? Or imagine Scrooge before his tragic capitulation: `The chap who is living in the gutter on scraps from the tables of the rich has *nothing to complain* of , for he is *perfectly adapted* to living in exactly the way that he does; viz in the gutter on scraps from the tables of the rich.' This would be a joke if it were funny.

Consider a couple of numbers. There are, by current estimates some nine or ten million species; and it must be approximately true that each one of these has its proprietary niche. I don't know what the total number of species past, present and future is, but I suppose it must be pretty big. Accordingly, there must be (/or have been/or be going to be) at least as many ecological niches as that. And that doesn't begin to cover the case since it leaves out the relations between counterfactual but nomologically possible creatures and the ecological arrangements that are their (counterfactual but nomologically possible) ecological niches. Now I ask you, as among friends, does it strike you as plausible that an empirical theory could specify all those niches, actual and counterfactual, other than post hoc? I.e that all those actual and counterfactual environmental arrangements are instances of a small and compact list of lawful generalizations?

It's a commonplace of macrobiology that there are remarkably ways in which creatures contrive to make their living. Some creatures are viable because they can live in near-boiling water; some creatures are viable because they can live in near-freezing water; some creatures are viable because they have protective coloration; some creatures are viable because they are practically transparent; some creatures are viable because they his tragic capituation: "The chap who is living in the gutter on used scraps of food has nothing are so big; some creatures are viable because they are so small; some species that live in water are viable because they can breathe under water, some species that live in water are viable because they can hold their breath for a very long time; some species are viable because the webs they spin, though delicate, are very fine grained and so can catch small things; some species are viable because the webs they spin are very coarse grained but very strong, and so can catch big things. And there are species that are viable because they parasitize species that are viable for some other reason. And there are species that are viable because they parasitze them.[40] And there are species (the Victorians couldn't stomach them) that are viable because they lay their eggs in the bodies of paralyzed hosts, which latter the larvae, having hatched, then proceed to digest from inside out. And so on, and so on. And, of course some species are viable simply because, de facto, because there happen not to be predators in the part of the woods in which they live and have their being.[41]. Well, if there is to be an empirical theory of niches, all such facts must fall under a relatively small and counterfactual supporting set of generalizations; indeed, the generalizations they fall under, must be laws. Seriously, what do you suppose are the chances of that?

[40] "Big fleas have little fleas/ upon their backs to bite'em/ And little fleas have lesser fleas/ and so on ad infinitum."

[41] Adaptationists should find this sort of case particularly embarrassing since it suggests that it's sometimes *a mere accident* that a certain environmental arrangement constitutes a niche.

In similar spirit, let me remind you of some facts about size. The smallest animal[42] (a kind of insect) is said to be about 1.7 millimeters long. The largest animal (the blue whale, probably the largest animal ever) is 80-90 feet long. Intermediate creatures are scattered throughout the size continuum, and there must be many cases in which its size counts a lot for a creature's fitness and many cases in which it doesn't. If the notion of an ecological niche is to be other than post hoc, there must be, for each kind of creature (actual or nomologically possible) whose size affects its fitness, a generalization that specifies the ecological arrangements in which it does so. (Please bear in mind, once again, that these generalization must be counterfactual supporting and the types that they refer to must all be natural kinds.) And there are, of course, many,. many parameters other than size along which creatures do or could differ in ways that do or could affect their viability; and, almost certainly, the effects on fitness of some of these parameters interact with the effects of size on fitness, and the effects of others of them don't. If there is to be a theory of niches, there will have to be a law that specifies which parameters interact with size and which don't, and what the consequences of each such interaction would be for the fitness of each relevant phenotype. Are you still convinced that there could be a substantive account of what makes an environment a niche?

The basic difficulty is, of course, the same one that also makes the notion of `selection for' so problematic: which traits are adaptive for which phenotypes depends very much on the context , including the context consisting of whatever other traits belong to the phenotypic. There are many, many traits that are fitness enhancing in some circumstances or other but not across the board. For each such trait, and for each phenotype that includes the trait, the laws of adaptation must determine the environmental arrangements in which having it (does or would) promote the creature's viability (and hence constitute a niche for that phenotype); and the environmental arrangements in which having the trait would decrease the creature's viability; and the arrangements in which it wouldn't matter one way or the other. Do you really, really, in your heart of hearts, think that there could be such a theory? Perhaps it would be well to repeat that my worry isn't that if there were such a theory it would have to be frightfully complex. The worry is that, prima facie anyhow, ecological niches wouldn't seem to be natural kinds. `Cambridge properties' aside, all they have in common is that some kind of creature or other, does or would, flourish in each.

Rhetorical conclusion.

42 Protozoa not included; I gather protozoa have recently become not-animals (just as Pluto has recently become a not-planet.) I also haven't included viruses, bacteria, plants, all of which have been not-animals for a long time. If you do include them, the situation looks orders of magnitude worse.

An analogy (in fact, I think, quite a close one): For each person who is rich, there must be something or other that explains his being so: heredity, inheritance, cupidity, acuity, mendacity, grinding the faces of the poor, being in the right place at the right time, having friends in high places, sheer brute luck, highway robbery, or whatever. Which things conduce to getting rich is, of course, highly context dependent: It's because of differences in context that none of us now has a chance of getting rich in (for example) the way that Genghis Kahn did; or in the (not unsimilar) way that Andrew Carnegie did; or in the (quite different) way that Andrew Carnegie's heirs did; or in the (again quite different) way that Liberace did; and so forth. Likewise, the extreme context sensitivity of the ways of getting rich make it most unlikely that there could be a theory of *getting rich per se*; all those how-to-get-rich books to the contrary notwithstanding. In particular, it's most unlikely that there are generalizations that are lawful (hence counterfactual supporting, not ad hoc, and not vacuous)[43] that specify the various situations in which it is possible to get rich and the properties in virtue of which, if one had them, one would get rich in those situations.[44] This is, please notice, fully compatible with there being convincing stories that explain, case by case, what it was about a guy in virtue of which he got as rich as he did in the circumstances that prevailed when and where he did so.

I think adaptationist explanations of the evolution of heritable traits are really a lot like that. When they work it's because they provide plausible historical narratives, not because they cite covering laws. In particular, pace Darwinists, adaptionism *doesn't* articulate the mechanisms of the selection of heritable phenotypic traits; it couldn't because there aren't any mechanisms of the selection of heritable phenotypic traits (as such.) All there are is the many, many different ways in which various creatures manage to flourish in the many, many environmental situations in which the do so. Diamond (op cit, p.x) remarks that Darwin didn't just present "a well-thought-out theory of evolution. Most importantly, he also proposed a theory of causation, the theory of natural selection." Well, if I'm right, that's exactly what Darwin *didn't* do; a `theory of causation' is exactly what the theory of natural selection *isn't.*

From the viewpoint of the philosopher of science, perhaps the bottom line of all this is the importance of keeping clear the difference between historical explanations and covering law explanations. Just as there is nothing obviously wrong with historical explanations, there is likewise nothing obviously wrong with covering law explanations. Roughly, they start with a world in which the initial conditions and

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[43] `Everybody gets rich in contexts in which he accumulates riches' doesn't count (though it is, of course, perfectly true.) Likewise `selection favors a creature that inhabits an ecological niche.'

[44] Notice the immediate temptation to provide caveats ad hoc: `Well, maybe there is a theory of how to get rich in 12th century Mongolia; and a *different* theory of how to get rich in the Wild West, and a still different theory of how to get rich in 20th century Manhattan.' Maybe. On the other hand, maybe `ways to get rich' just doesn't name a natural kind.

the natural laws are specified, and they deduce predictions about what situations will transpire in that world. By definition, the explanation of an event by reference to a covering law requires that that the event have some projectible property; that is, some property in virtue of which it falls under the law that covers it. Nothing has a covering law explanation unless it belongs to a natural kind. (I take what I've just said to be a string of truisms.) Covering law explanations have had a good press in philosophy, and rightly so. Whether or not they are the very paradigms of scientific explanation, it's pretty clear as a matter of fact that many scientific explanations are or incorporate appeals to covering laws.

But nor is there anything wrong with explanations that consist of historical narratives. Roughly, a historical narrative starts with an event observed to have occurred, for which it seeks to provide an empirically sufficient cause; (it was for want of a shoe that the horse was lost.) So historical narratives are inherently post hoc (though not, of course, inherently ad hoc.) The causally sufficient conditions that historical narratives invoke belong, in familiar ways, to chains of such conditions which (assuming determinism) can go back as far as you choose; (it was for want of a nail that the shoe was lost.). How far back such an explanation ought to go depends, as one laughingly says, on pragmatic factors: what is being explained and to whom, and to what end.

By contrast, many paradigm scientific theories are, I think, best understood as historical narratives; Consider, inter alia: theories about lunar geography, theories about why the dinosaur became extinct, theories about the origin of the Grand Canyon, or of the Solar System or, come to think of it, of the universe. All these projects (and, surely many others) are post hoc searches for chains of sufficient causal conditions whose satisfaction would explain the occurrence of the event in question. If I'm right, theories about how heritable traits evolve are also of this kind.

That's really just to say that the various mechanisms of adaptation don't themselves constitute a natural kind for purposes of evolutionary explanation; not, at least, if the model for explanation is subsumption under nomologically necessary generalizations. But if there are no nomologically necessary generalizations about the mechanisms of adaptation as such, then the theory of Natural Selection reduces to a banal a truth: `If a kind of creature flourishes in a kind of situation, then there must be something about such creatures, (or about such situations, or about both) in virtue of which it does so.' Well, *of course* there must. Even Creationists agree with that.

None of this should, however, lighten the heart of anybody in Kansas; not even a little. In particular, I've provided not the slightest reason to doubt the central Darwinist theses of the common origin and mutability of species. Nor have I offered the slightest reason to doubt that we and chimpanzees had (relatively) recent common ancestors. Nor I do suppose that the intentions of a designer, intelligent or

otherwise, are among the causally sufficient conditions that good historical narratives would appeal to in order to explain why a certain kind of creature has the phenotypic traits it does (saving, of course, cases like Granny and her zinnias.) It is, in short, one thing to wonder whether evolution happens; it's quite another thing to wonder whether adaptation is the mechanism by which evolution happens. Well, evolution happens; the evidence that it does is overwhelming. I blush to have to say that so late in the day; but these are bitter times.[45]

[45] I discover (why am I not surprised?) that if you really want to annoy your friends and relations, you should write a paper attacking evolutionary adaptationism. Among those who attempted to dissuade me, I'm particularly indebted to David Buller, Georges Rey and Louise Antony; this paper would have been much worse except for their comments on earlier drafts. Also, I'm grateful to students in the NYU Graduate Philosophy Department where I taught (very badly) a course on evolutionary psychology in which some of this material was presented.