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## Against Fodor

Rafael Ventura

### I. Introduction

Since Darwin introduced the notion of natural selection to account for speciation, a discomfort towards evolutionary theory has persisted. Over the past 150 years, the main focus of criticism has shifted from empirical concerns – e.g., how could natural selection account for the apparent design in nature? – to questions more conceptual in character. It thus became fashionable to inquire whether natural selection could be stated in a non-trivial or causally relevant manner. In a recent expression of this trend, Jerry Fodor has challenged the idea that predictive power could be ascribed to natural selection.<sup>1</sup> Arguing that concepts revolving around natural selection cannot be subsumed under any lawful generalization, Fodor concludes that selection should at best be understood as a historical reconstruction of past selective events. Although Fodor distances himself from creationist fancies, and he does not dispute the blatant evidence for evolutionary change, he is unwilling to grant Darwinism the full prize: for Fodor, there can be no theory of natural selection *per se* because selection does not constitute a natural kind.

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I don't think that Fodor's position can be sustained. Though it may at times be extremely hard to predict the future outcome of natural selection due to the numerous ecological intricacies involved, I want to argue that predictions in connection with selective phenomena are at least in principle possible. This will become clearer after the notion of fitness as a relational property is spelled out. Combined with this thesis, I want to reinforce a dispositional interpretation of fitness from which Fodor seems to unwillingly distance himself. On this view, the predictive force of natural selection lies not in forecasting the outcome of individual selective events, but rather in probabilistic assumptions bound up with populational thinking. Fodor's worries that selection might fail to refer to a natural kind will also vanish as soon as a one realizes that what is being selected for isn't simply an individual phenotypic trait, but a bundle of interacting trait types against a fixed environment. Natural selection will thus turn out to be more than a historical account of the causal chain of events leading to a given evolutionary configuration. By stressing these points, I hope to show that natural selection's original role as a predictive tool in evolutionary biology can be retained.

## II. Fodor's attempted coup against Darwinism

### *i. Selection for and counterfactual conditionals*

Fodor's primary purpose in *Against Darwinism* is to checkmate the Darwinian model of speciation by means of natural selection. Whereas Fodor accepts "the central Darwinist theses of the common origin and mutability of species",<sup>2</sup> he is skeptical about natural selection's capability to account for evolutionary change. The core of his criticism is filled with grudges against the concept of selection itself: if evolution is to be equated with "changes of the distribution of phenotypic traits in populations of organisms",<sup>3</sup> one should expect the notion of selection *for* phenotypic traits to be elucidated. But that is precisely what evolutionary theory can provide, Fodor assures us. Drawing on examples from the philosophy of mind (in this case, the minds of frogs), Fodor invites us to think of intentional states as adapta-

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tions – that is, as the product of selective processes. If a frog’s fly-catching behavior is thought of as an adaptation, there seem to be good reasons to assume that this particular behavior was selected for catching flies. How far one is justified in isolating an individual trait as the sole substrate on which natural selection operates is a question to be pursued below. Suffice it at this point to say that there is for Fodor a strong connection between the development of an adaptive structure and the notion of selection *for*. Since mere selection cannot distinguish an intentional trait from other coextensive mental dispositions, Fodor’s intuition is that to explain the appearance of a frog’s fly-catching mechanism as a mechanism for snapping at flies one has to resort to the idea that there is something that is being selected for.

A less misleading example will more accurately illustrate the case. Imagine a population of fruit flies under the selective pressure of predators that have a higher chance of spotting them against the dark foliage if they are light-colored. Now suppose that the fly’s genes encoding a dark body color also occasion an arrhythmic locomotive activity, so that ebony fruit flies also display a more chaotic, somewhat flamboyant behavior. In this case, the complicating question of what frogs are snapping at when they snap at flies is avoided,<sup>4</sup> though Fodor’s “disjunction problem” still holds: “[i]f you are selecting for Bs and Bs are Cs, it doesn’t follow (it needn’t be true) that you are selecting for Cs.”<sup>5</sup> Although it is clear that dark body color and arrhythmic locomotive activity will be jointly selected, it seems right to say that arrhythmic motion isn’t what is being selected for. The rather pressing point Fodor is making here is that mere selection doesn’t seem able to cope with coextensive traits. To account for body color in adaptive terms, the stronger notion of selection *for* body color (and not for arrhythmic behavior) has to be invoked.

Put this way, Fodor’s argument can easily be read as what it is intended to be: a puzzle about counterfactual conditionals. Since in counterfactual worlds the property of being ebony needn’t coincide with arrhythmic locomotion (just like flies needn’t be black dots in other possible worlds), it is reasonable to suggest that if ebony fruit flies didn’t display arrhythmic locomotive ac-

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tivity, they would—in an environment where dark colors predominate—still be selected. Analogously, if any light-colored fruit flies happened to be defective in locomotive rhythm, they would presumably not be selected.

*ii. Laws of nature, ceteris paribus clauses and Fodor's conclusion*

Trouble, however, sets in at this point. For how is the theory of natural selection to support such counterfactual conditionals? Fodor adumbrates two possible ways to do so. One might first take Darwin's original analogy with artificial selection seriously and suppose that natural selection is operated by the hands of a mindful Nature. Like a pigeon fancier of Darwin's time, who "adds [successive variations] up in a certain direction useful to him",<sup>6</sup> natural selection could be thought of as an intentional system governed by Nature's whim: "that what it selects for is whatever it has in mind in selecting."<sup>7</sup> Counterfactuals could thus be formulated according to whatever is, as Fodor puts it, in the mind of Mother Nature. If Nature aims at selecting ebony fruit flies because of their body color and not because of their arrhythmic locomotion, then in a counterfactual world only those fruit flies that exhibit a dark color would be selected.

Clearly, Fodor doesn't entertain this possibility earnestly. In fact, as he points out, one of the main theoretical strengths of adaptationism is that it "doesn't require the attributions of agency"<sup>8</sup> to mechanisms operating behind natural phenomena. Moreover, if Nature voluntarily orchestrated selective processes, there would be the rather grave question (shamanic assurances to the contrary notwithstanding) of how to gain epistemic access into her mind. So the most natural way to go is to appeal to laws of selection.<sup>9</sup> Fodor's ensuing attempt is to try to think of a law relating a certain phenotypic trait with selective advantage over another trait. That is, for such a nomic relation to hold there would have to be a trait  $F$  in virtue of which individuals carrying trait  $F$  compete successfully with individuals lacking  $F$ . In the case at hand, a law would be said to correlate ebony body color ( $F$ ) with selective success ( $G$ ) if ebony fruit flies are selected in virtue of their being ebony, such that:  $F \rightarrow G$ .

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Fodor rejects this proposal. For him, when it comes to natural selection, such “a generalization applies not to Fs *per se*, but only to Fs-in-such-and-such circumstances.”<sup>10</sup> This is what Fodor dubs the “context-sensitivity” of a trait’s fitness. There is strong evidence suggesting that for Fodor his notion of a trait’s selective success and a trait’s fitness are interchangeable terms. In fact, Fodor suspects that the concept of a trait’s fitness isn’t less problematic, “since it’s massively context sensitive whether a certain phenotypic trait is conducive to a creature’s fitness.”<sup>11</sup> So what Fodor seems to be hinting at is the intrinsic relation existing between a trait’s physical makeup and its environmental surroundings in yielding a fitness value. In fact, if a law of natural selection be expected to associate any given trait with a certain propensity to successfully compete with other traits, it would be absurd to reduce the range of such a law to a small set of fortuitous phenomena. Likewise, it would be extremely *ad hoc* to postulate a multitude of local laws to account for every individual case of selective success.

To make matters worse, Fodor emphasizes that the context-sensitivity at issue shouldn’t be mistaken for a *ceteris paribus* clause. In the latter case, the general force of a law-like statement is “obscured by the effects of unsystematic, interacting variables.”<sup>12</sup> There is, however, an underlying generalization believed to apply to all cases, other things being equal. To draw on a stock example, Newton’s law of universal gravitation expresses the relation between the gravitational force ( $F$ ), the gravitational constant ( $G$ ) and two point masses ( $m_1$  and  $m_2$ ),  $F$  being inversely proportional to the squared distance ( $d^2$ ) between  $m_1$  and  $m_2$ . A *ceteris paribus* clause ensures in this case that other variables are held constant, so that deviations to Newton’s formula are to be justified in a case-by-case basis.

Thus, the general gist of Fodor’s assault on Darwinists amounts to pinpointing the difficulties bound up with the context-sensitive nature of “being a trait that is selected for”. If selective success is indeed comparable to the notion of “getting rich”, as Fodor exhorts, there is no regular behavior to be captured by “any nomological generalizations about which traits win compe-

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titions with which others.”<sup>13</sup> On Fodor’s view, adaptationist research programs are consequently divested of any predictive power. Because there is for him no homogenous reality behind traits that are being selected for, evolutionary theory’s conceptual apparatus can on Fodor’s view be reduced to reference-empty sets: for Fodor, the term “‘a trait that’s being selection for’ pretty plausibly doesn’t pick out a natural kind.”<sup>14</sup> All that is left for evolutionary biologists is the task of articulating causally relevant events that individually explain each adaptation, given that—unlike lawful generalizations—“historical narratives are about (causal) relations among [particular] events.”<sup>15</sup> That is to say, evolutionary theories are better understood as fact-gathering narratives that in nothing resemble the projective character of the broader scientific enterprise.

### III. The case against Fodor

#### *i. Fitness’ context-sensitivity*

Immediate reactions triggered by Fodor’s caustic remarks on natural selection tended to focus on the role played by laws in adaptationist explanations. Both Godfrey-Smith (2008) and Dennett (2008) partially acquiesce to Fodor’s point and discourage the use of covering law models of explanation<sup>16</sup> in evolutionary biology. Though less emphatically, Sober (2008) sometimes seems willing to accept less traditional models of explanation too.<sup>17</sup> In what follows, I want to endorse the view that evolutionary theory’s predictive power can only be regained if some kind or other of lawful generalization is allowed for in evolutionary thinking. But before showing how this can be done, some clarification is needed. First, Fodor isn’t justified in shifting the whole explanatory burden onto the concept of selection for. Adaptations can and often are explained in terms of mere selection. That being said, Fodor’s notion of context-sensitivity will be carefully examined. After it is shown that the context-dependence of a trait’s fitness isn’t as harmful as Fodor believes, I want to draw attention to an objectionable conceptual confusion that Fodor seems to partake in.

From the outset, Fodor commits himself to the idea that a

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phenotypic trait thought to have evolved as an adaptation has to be identified with a trait that was selected for.<sup>18</sup> Since two coextensive traits will as a matter of fact be selected together, Fodor's complaint is that an evolutionary "free rider" would hardly seem able to qualify as an adaptation. I think the point is moot. To explain the appearance of a given trait, both the phenomena of pleiotropy and gene linkage might have to be taken into account.<sup>19</sup> Traits are often selected in bundles, be it either because they share a common genetic basis or because the genes encoding the relevant traits tend to be inherited together during meiosis. This is exemplified in the case of ebony fruit flies exhibiting anomalous locomotive activity. In explaining the prevalence of arrhythmic locomotion in a population of fruit flies, one might resort to the notion of *selection of*: locomotion-defective flies are frequently found in a certain type of environment because there is selection of fruit flies carrying the genes responsible for the expression of this trait. True, at times this will be only a partial explanation. One may want to inquire why precisely these flies were selected and not others. A more fine-grained explanation could involve a causal account of why these particular genes and not others are being selectively favored.<sup>20</sup> Depicting a more detailed causal network would then take us back to the question of what trait there is selective pressure for: in the hypothetical case sketched above, for ebony and not for locomotion-defective flies. But there is no reason to restrict adaptationist explanations solely to the latter kind.

Nevertheless, Fodor is right in insisting on selection *for* traits if we want to look into the causal roots of selective phenomena. If Fodor has any hope of finding laws of selection, he is also right in resorting to the selection *for* traits for the following reason. It is generally assumed that properties (and not individual terms) are subject to lawful generalizations.<sup>21</sup> There are, furthermore, strong arguments suggesting that organisms and species are better understood as historical entities – that is, not as types, but rather as tokens.<sup>22</sup> So if individuals both at the organismic and specific levels are spatiotemporally contiguous entities, defined by a common historical origin, there is forcefully no natural law in which

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they could figure. Hence, for traits to be explained not as by-products, but as genuine elements in the causal machinery of selective processes, biological entities will have to be decomposed into properties or sets of properties that are thought to causally interact, producing adaptive responses.

Crucial at this point is the question of what these phenotypic properties interact *with*. It is a mistake to suppose that an individual's trait interacts exclusively with other individuals' traits, as if a sum of phenotypic traits occurring in the same population could alone give rise to the property that Fodor loosely calls "*competing successfully with*".<sup>23</sup> Rather, in order to state what property competes successfully with what other property, one has to specify the environmental background against which selective competition is said to take place.<sup>24</sup> It should be noted that 'environment' is to be understood in this context in a broad sense, including both abiotic and biotic elements, but an organism's own genetic makeup as well.

Is this a premature surrender to Fodor's criticism on the context-sensitivity of a trait's fitness? To a certain extent, yes: as Fodor poignantly notices, "the adaptivity of a trait depends on the ecology in which its bearer is embedded."<sup>25</sup> However, this inference isn't an insurmountable obstacle. Of course, in each case the environmental variable will have to be held constant if any significant statement about a trait's survival and reproductive viability is to be made. But this doesn't prevent us from relating a change in the trait's overall frequency to a trait's relative fitness where natural selection is at work: the greater a trait's fitness, the higher the chances of its spreading throughout next generations.<sup>26</sup> Indeed, this is the force behind Darwin's initial intuitions. If resources aren't infinitely available, any heritable trait that favors an individual's survival or reproductive rate will tend to be preserved, reappearing in the next generations.<sup>27</sup> For this correlation to obtain, nothing intrinsically content-bound *about* the correlation has to be assumed.

Complications are bound to arise if we expect—consistent with our previous assumption that the environmental background to a trait's relative fitness also comprises interspecific fac-



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tors – that natural selection will also affect the very environment in which it is operating. If natural selection occasions changes in a trait’s representation in the overall population, and if a trait’s relative frequency is part of the environmental settings to selective events, then a trait’s relative fitness will be doomed to permanent fluctuation. These considerations are an exacerbation of Fodor’s original concerns that “which traits are adaptive for which phenotypes depends very much on the context.”<sup>28</sup> Yet, one needn’t succumb to the conundrum. By splitting a spatial-temporally continuous environment into time-slices, Okasha (2008) rightfully maintains that one might still ideally refer to natural selection as “an optimizing process *relative to a given environmental state*.”<sup>29</sup> Thus, even if only in relation to a circumscribed environmental state, a trait’s fitness can be restored as a viable concept. If Okasha’s proposal is taken seriously, then different traits could be assessed against transitory stages in an environment’s development, in accordance to which different fitnesses will result.

Despite empirical difficulties associated with the enterprise just outlined, this is good evidence that predictive function can at least in principle be ascribed to a trait’s fitness. That the same phenotypic constitution might acquire different fitnesses according to different environmental states or that, conversely, physically distinct traits might take on similar fitness values needn’t disturb us. *Pace* Fodor, there is no reason to fear that “‘is selected for’ can’t be a projectible predicate.”<sup>30</sup> Fodor worries that because a trait’s fitness is a compound property of an individual’s physique and the environmental setup it inhabits, nothing could be inferred from the observation that trait  $T_1$  is selected for in environment  $E_1$ . After all, trait  $T_1$  might *not* be selected for in environment  $E_2$  and a distinct trait  $T_2$  might turn out to be equally or even fitter than  $T_1$  in environment  $E_1$ . So fitness values might indeed seem to float free from physical constraints, giving the impression that they aren’t projectible. But all that is required for fitness values to be grounded on a physical basis is, arguably, a minimal degree of consistency: if a given trait be said to correlate with a certain fitness value, it is desirable that the same physical

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configuration be consistently identified with the same fitness under the same environmental conditions.<sup>31</sup> And I see no reason to suspect that the concept of fitness cannot meet this requirement. To return to the example of fruit flies, we can assume that the ascription of a certain fitness value to ebony body color will remain constant as long as an environmental state is taken as a fixed point of reference. Hence, given an environmental framework of reference, the same physical constitution will bring about a constant second-order property, such as fitness is thought to be, and the same fitness value will correctly be projected into cases where trait and environment similarly correlate.

*ii. Fodor's improbable move*

So far, I have been implicitly assuming the standard dispositional interpretation of fitness.<sup>32</sup> Although Fodor is silent on this topic, some of the fundamental problems he wants to raise shall vanish as soon as a propensity reading of fitness is spelled out. Fodor seems inattentive, for example, to the fact that the outcome of “*who wins a [trait] t1 versus [trait] t2 competition*” isn’t only “*massively context sensitive*”,<sup>33</sup> it also has to be stated in probabilistic terms. No particular event of selective success could ever be taken as evidence that a given trait is being selected for.<sup>34</sup> For similar reasons, I tend to disagree with Fodor’s pessimism about the causal role played by individual trait types. Skeptical about the explanatory power of “selection for”, as opposed to Sober’s (1984) notion of “selection of”, Fodor notes that “strictly speaking, *traits* don’t get selected at all; traits don’t either win competitions or lose them.”<sup>35</sup> Fodor’s alternative solution is to consider whole phenotypes as causally relevant for natural selection. But for Fodor that won’t do: “[u]nlike a scientist in a laboratory, natural selection can’t control for confounding variables”,<sup>36</sup> so to credit any specific trait with partial causal significance turns out to be *prima facie* impossible. However, Fodor is wrong in assuming that what holds true for individual cases can also be predicated of phenotypic trait types obeying probabilistic rules. Moreover, the fact that Fodor is willing to grant scientists in a laboratory the tools necessary for the task is indicative that we are deal-

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ing (*pace* Fodor) with empirical, not conceptual intricacies.<sup>37</sup>

Related issues on the unit-of-selection problem support this diagnosis. To answer the question of whether causally relevant properties should be located at the particle or group level, there is no resource at the biologist's hand other than controlling for variables: if individual fitness affects the differential reproduction at species level, but not properties at the particle level, then it is reasonable to suggest a causal link between group properties and selection.<sup>38</sup> Analogously, a similar model should be expected to work in a less complex scenario where concurring properties are thought of in connection with one another at the same hierarchical level.

The empirical complexities of evolutionary models tie in with a probabilistic reading of the concept of fitness for a further reason. Once it is granted that fitness is conceptually dependent solely on a trait's physical structure and the environmental background where it occurs, it is hard to see how a trait's fitness could ever be promptly measured on the basis of actual reproductive success. Rosenberg (1984) points out that if fitness values are defined by future rates of reproduction, there is no way to test the obtained result; but if fitness is defined by past reproductive rates, it would be plainly circular to use this concept in any explanatory move. So, it seems, the only viable solution to Rosenberg's dilemma is to opt, on pains of empirical vacuity, for the latter mode of measuring fitness. However, as Rosenberg recalls, this doesn't corrode the predictive utility of fitness. Since a trait's reproductive advantage is correctly thought of as a partially hereditary characteristic, a trait's fitness will be "nomologically connected to the number of [reproductive] opportunities at previous and future generations."<sup>39</sup> On the one hand, if fitness values are to perform any explanatory function, they cannot entirely forgo prognostic extrapolation; on the other hand, if fitness is to have a minimal amount of empirical relevance, it will have to partially rely on past reproductive rates. Thus, ascribing a numeric value to a trait's fitness is an empirically tricky enterprise in which a certain degree of projective thinking has to be allowed.

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With this in mind, Fodor's conflation of the notions of "a trait which wins competitions with others" and "a trait's relative fitness", which might initially have seemed harmless, should strike us as forced.<sup>40</sup> While the former term designates the unique outcome of a given selective process, the latter notion bears predictive relevance. Though past and future selective trends may generally be related, as I hinted above, there are no good grounds to unjustifiably blur the conceptual boundaries between the two terms. The example of dramatic results brought about by cataclysmic events brings this to the fore. Once environmental turmoil radically reorganizes the tension between selective pressures and adaptive responses, past reproductive and survival rates don't need to coincide with or "nomologically connect to" present fitness values. What was once selected (say, increased body size)<sup>41</sup> when predation was the main selective pressure at work may cease to be advantageous after the leading pressure shifts to harsh climatic conditions. That is, the fit between dinosaurs' average body size and the current selective scenario during the Mesozoic period may explain why reptilian clades – containing living beings *like* the ones selected – are overrepresented in fossil records of a geological era *like* that.<sup>42</sup> In contrast, the fact that increased body size was reproductively successful at the same time period simply chronicles the outcome of a unique selective battle witnessed by the Earth's biosphere. That is, in the former case there is an "ontological ascent" from particulars to trait *types*; in the latter case, trait tokens are descriptively inserted in a causally structured whole.

If this is right, Fodor's conclusion is only corroborated by his conflating the concepts of "a trait which wins competitions with others" and "a trait's relative fitness" – concepts that should be kept apart. No selective prognostic can be based merely on what trait is being selected. In order to do that, one would also have to take into account a) that a trait is always selected relative to an environmental background and b) that a trait's fitness (although correlated to the rate at which the trait is being selected for) represents no more than a probable cause for a certain selective outcome. By doing so, one naturally shifts away from the idea that

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*this* particular trait is being or was selected for towards a more general inference: given similar conditions, similar physical structures will yield similar rates of relative survival and reproductive success. It is thus reasonable to project that, under predation and provided with abundant food supply, big mammals will display higher chances of survival and reproductive success than their smaller counterparts—just like reptiles did and like other beings under similar circumstances would.

There is, furthermore, no need to suppose that predictive power automatically entails a deterministic relationship believed to hold between phenotype, environment and actual reproductive success. Once a propensity reading of fitness is adopted, the predictive utility of evolutionary theory can be sustained even if one rejects, as it is desirable, a deterministic reading of selective processes. One way of doing this is to identify natural selection with the notion of a stochastic mechanism: abdicating from the unbiased certainty with which Newtonian mechanics is able to predict the motion of mass points, evolutionary theory is better understood as a theory that relies on probabilistic thinking. I bypass here the controversy over causal and probabilistic interpretations of natural selection. The point I want to make is that at some level or other probabilistic elements have to be assumed, which nonetheless doesn't hinder us in using the theory of natural selection predictively. There is plenty of room for predictions, if we assume a probabilistic interpretation of natural selection, because "[t]he expected outcome is not generated by attending to the forces acting on the coins, but by taking into account the structure of the population being sampled."<sup>43</sup> In this case, a trait's relative fitness represents a statistical property whose variation "predicts and explains changes in relative frequencies."<sup>44</sup> Likewise, a causal interpretation of natural selection ensures that "evolutionary theory provides both source laws and consequence laws for the various forces it describes."<sup>45</sup> On this reading, weighing the forces at work in a given selective arena provides a reliable description of the underlying causal network, which will in turn enable predictions to be made. Be it because natural selection "is a mere consequence of a statistical property

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of a population – its variation in fitness”,<sup>46</sup> or because it “operates when a critical environmental factor causes one trait to be selectively favored over another trait”,<sup>47</sup> in both cases Fodor’s thesis that the context dependence of a trait’s fitness undermines Darwinism’s predictive power doesn’t hold water.

#### IV. Conclusion

In the foregoing pages I have sought to relativize Fodor’s claim that the theory of evolution by natural selection has no predictive power, endorsing at most *post hoc* explanations. One main reason for Fodor drawing this conclusion lies on his mistaken understanding of the context-sensitivity of a trait’s fitness. First, the fact that a trait’s fitness is better described as a relational property does no harm to the regularity thought to obtain in its promoting survival and reproductive success. Because a trait’s fitness is a value resulting from the interaction of both the constitution of its bearer and the environmental which it inhabits, it doesn’t follow that trait types cannot be systematically allied with certain environments to yield constant fitness values. Although the same trait may indeed exhibit distinct fitnesses in different environments, there is no reason to suppose that a trait’s fitness won’t be constant under fixed conditions.

At this point, it is telling to notice Fodor’s second unwarranted move: a trait that is selectively successful needn’t be equated with the trait whose relative fitness peaks in a given environment, as Fodor seems to assume. Since a trait will prove reproductively favorable only in the long run, Fodor’s insistence that the central explanatory notion in evolutionary theory is which trait “successfully competes” with other traits cannot be taken seriously. A trait may in fact compete successfully with other traits depending on various factors, but there is still an important sense in which a partial causal role can be ascribed to trait types.

Thus, there is no reason to despoil selection of its predictive utility. Of course, how to measure the impact on a trait’s fitness will be empirically challenging. Since the only measurable variables are past survival and reproductive rates, some degree of projective approximation will be required, if a trait’s fitness is to

be interpreted as a propensity to leave copies in next generations. But empirical intricacies shouldn't prevent biologists from modeling approximate selective scenarios. By doing so, scientists inevitably engage in both predictive and explanatory activities. Although a minor objection to Fodor's position, it should moreover be reminded that "selection for" isn't the only explanatory concept to be used in evolutionary thinking. As the case of neutral mutations, gene linkage and pleiotropy attest, the fixation of some traits can also be explained by chance events and by the selection of whole phenotypes.

### Notes

<sup>1</sup> There seems to be no consent on what Fodor (2008) is trying to get at. While Dennett (2008) and Sober (2008) make much of Fodor's claim that "the theory of natural selection can't explain the distribution of phenotypic traits in biological populations" (p.11), for Godfrey-Smith (2008) Fodor is dissatisfied with natural selection's apparent failure to accord to traditional covering law models. I tend here to side with the latter, although I think he does little justice to the fact that Fodor eventually concedes that historical explanations are "often perfectly OK" (p.16).

<sup>2</sup> Fodor (2008), p. 23.

<sup>3</sup> Fodor (2008), p. 1.

<sup>4</sup> Fodor's (2008) license to state his doubts about Darwinism in mental terms is backed by the presupposition that a "mechanism that's selected for catching flies is *not ipso facto* a mechanism

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that's selected for catching ambient black nuisances" (p. 3). I sidestep these issues because they will prove irrelevant for the present discussion. Moreover, I do not see how, in Fodor's case, any accurate inference can be made about the mental content of a frog's fly-catching mechanism.

<sup>5</sup> Fodor (2008), p. 4.

<sup>6</sup> Darwin (2009), p. 30.

<sup>7</sup> Fodor (2008), p. 6.

<sup>8</sup> Fodor (2008), p. 7.

<sup>9</sup> Noting that laws can support counterfactuals, Fodor (2008) suggests that "that's what makes laws different from mere true empirical generalizations" (p. 8). Fodor is here implicitly drawing on Dretske (1977), the idea being that because laws hold between universals and not between individual terms, nomic relations "*go beyond* the set of things in *this* world that exemplify these properties and have these magnitudes" (p. 266).

<sup>10</sup> Fodor (2008), p. 9.

<sup>11</sup> Fodor (2008), p. 9.

<sup>12</sup> Fodor (2008), p. 9.

<sup>13</sup> Fodor (2008), p. 10.

<sup>14</sup> Fodor (2008), p. 11.

<sup>15</sup> Fodor (2008), p. 15.

<sup>16</sup> With an appeal for the use of models and mechanisms in scientific explanations, Godfrey-Smith (2008) concludes that "there is no need for 'laws of selection' in Fodor's sense, either as a basis for explanations or counterfactuals, so it does not matter that they do not exist" (p. 39). Less constructively, Dennett (2008) also criticizes Fodor's "antique caricature of scientific practice, harking back to Hempel" (p. 27) as yet another expression of his self-warranted "license to dichotomize". But even if we adopt a view of scientific explanation based on models and mechanisms there seems to be no way of unconditionally avoiding law-like, regular behavior in explanations; cf. Machamer, Darden and Craver (2000) on mechanisms: "A mechanism is a series of activities of entities that bring about the finish of termination conditions in a regular way. These regularities are non-accidental and support counterfactuals to the extent that they describe activities" (p. 7).



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<sup>17</sup> Notwithstanding his extensive work on the lawfulness of natural selection, Sober (2008) surprisingly attests that biologists “don’t usually use the word ‘law’ to describe such generalizations as Fisher’s. Rather, they call them ‘models’” (p. 45).

<sup>18</sup> This is, in fact, Fodor’s (2008) first premise of his so-called “putative argument” that culminates with the conclusion that “the theory of natural selection can’t explain the distribution of phenotypic traits in biological populations” (p. 11).

<sup>19</sup> Sober (1984a), p. 101.

<sup>20</sup> Sober (2008), p. 47.

<sup>21</sup> Cf. Dretske’s (1977) view of laws as a matter of “ontological assent” (p. 263) already addressed above (p. 6, fn. 9).

<sup>22</sup> That organisms and species are not types is pointed by Hull (1978): “on the historical entity interpretation, similarity is a red herring. It is not the issue at all. What really matters is how many organisms are involved and how much the internal organization of the species involved is disrupted” (p. 348).

<sup>23</sup> Fodor (2008), p. 8. The property in question was represented by *G* in the reconstruction of Fodor’s argument I offered above (p. 6).

<sup>24</sup> Sober (2008), p. 45.

<sup>25</sup> Fodor (2008), p. 9.

<sup>26</sup> To keep Sober’s (1984a) mechanical analogy, I assume for present purposes that no evolutionary forces other than natural selection are at work.

<sup>27</sup> Darwin’s (2009) original insight is well captured by the following passage: “Owing to this struggle for life, any variation, however slight and from whatever cause proceeding, if it be in any degree profitable to an individual of any species, in its infinitely complex relations to other organic beings and to external nature, will tend to the preservation of that individual, and will generally be inherited by its offspring” (p. 61). Darwin is fully aware that a trait’s fitness can only be duly appreciated by inspection of “its infinitely complex relations to other organic beings and to external nature”.

<sup>28</sup> Fodor (2008), p. 21.

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<sup>29</sup> Okasha (2008), p. 346. Along the same lines, commenting on the view of natural selection as a hill-climbing exercise in which fitness values always increase, Okasha concludes that “natural selection does involve a kind of hill-climbing; it is just that the landscape is continually being re-configured as the population evolves” (p. 347).

<sup>30</sup> Fodor (2008), p. 18.

<sup>31</sup> This is Kim’s (1984) “consistency requirement” for weak supervenience, which, “in essence, is the prescription ‘Treat like cases alike’ in ethical contexts” (p. 62). Though not originally formulated to describe the biological concept of fitness, I follow Rosenberg (1984) in using Kim’s terminological grid for this purpose.

<sup>32</sup> Mills and Beatty (1979). For simplicity’s sake, I may freely speak both of individuals’ and of types’ fitnesses as a disposition, though the simplification is not representative of the authors’ true intentions. While doing so I keep in mind that “a notion of fitness which refers to *types* [...] is a derivative of individual fitness propensities” (p. 272).

<sup>33</sup> Fodor (2008), p. 9. To reiterate: the context-sensitivity of a trait’s fitness is unproblematic because it expresses an essentially relational property. Why its relational nature should render the concept of fitness unproblematic is clarified by Moore’s (1960) observation that “[a] relational property entails some quality in the term, though no quality in the term entails the relational property” (p. 309). That is, there is by definition no way of exhausting a relational property by inspecting one of its terms alone.

<sup>34</sup> As Dennett (2008) remarks, “Fodor’s quaint view of causation leads him to ignore the power of effects that depend on probability – he views such phenomena as not properly causal at all” (p. 28).

<sup>35</sup> Fodor (2008), p. 10.

<sup>36</sup> Fodor (2008), p. 10.

<sup>37</sup> More evidence that Fodor’s (2008) arguments do not undermine the theoretical cogency of causal-statistical models can be adduced from his remark that “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", (p. 10, fn. 19, last emphasis added).

<sup>38</sup> Elaborating on an example introduced by Eldredge and Gould, Okasha (2006) attributes the growth in body size among horses documented by fossil record to selective pressure acting on group properties: "If larger horse species are fitter than smaller ones, that is, are more likely to survive and speciate, this could explain the trend in the absence of any within-species selection for large size" (p. 110).

<sup>39</sup> Rosenberg (1984), p. 105.

<sup>40</sup> To keep Fodor's (2008) example of dinosaurs, p. 10. See above p. 7.

<sup>41</sup> Once again Fodor (2008) gives up hope of finding any lawful correlation between a trait and selective success since to explain why "[s]mall mammals won their competition with large dinosaurs" (p. 17) more than one plausible (causal) story can be told. Fodor's example is problematic because of the fragile epistemic access to geo- and cosmological events that took place millions of years ago. I assume for present purposes that non-avian dinosaurs were extinct due to a rapid change in climatic conditions.

<sup>42</sup> This reading of fitness as a causal concept is endorsed by Sober (1984b): "Fitness and selection are both causal concepts; they describe the causes of changes and not the fact that there has been differential productivity" (p. 204). An alternative to Sober's conception is presented below.

<sup>43</sup> Walsh, Lewens and Ariew (2002), p. 454.

<sup>44</sup> Walsh, Lewens and Ariew (2002), p. 462.

<sup>45</sup> Sober (1984), p. 70.

<sup>46</sup> Walsh, Lewens and Ariew (2002), p. 453.

<sup>47</sup> Barros (2008), p. 316.

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