

Biological Function: Dispositionalism Reassessed

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Abstract

A number of arguments against selection-dispositional accounts of function have been put forward in the past few decades. It has notably been charged that such theories (1) fail to account for an explanatory force allegedly inherent in function attributions, (2) problematically require the current existence of trait-variants in the population, (3) fail to grant functions to so-called ‘malformed’ traits when they ought to, (4) are excessively liberal in the function attributions that they license and finally (5) do not enable us to draw sufficiently fine-grained functional distinctions. In this paper it is found that, upon closer inspection, all five criticisms fail.

Introduction. There are essentially two ways of developing an account of biological function that appeals to the notion of natural selection: one can either appeal to facts concerning *history* of selection, or one can appeal to *current dispositions* for selection. Both approaches are represented in the contemporary literature on functions. According to ‘selection-historical’ (SH) accounts, very roughly, it is a function of trait t to bring about outcome F iff there was selection of ancestral precursors of t for their ability to bring about F . For example:

It is the/a proper function of [a trait t] of an organism (o) to do that which items of [t]’s type did to contribute to the inclusive fitness of o ’s ancestors, and which caused the genotype, of which [t] is the phenotypic expression, to be selected by natural selection. ([26, 174])

According to ‘selection-dispositional’ (SD), accounts, again very roughly, it is a function of trait t to bring about F iff t ’s disposition to bring about F positively contributes to the biological fitness of its bearer. In this spirit, one finds for example:

Something has a (biological) function just when it confers a survival enhancing propensity on a creature that possesses it. ([2])¹

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¹Admittedly, this quote does not specify what exactly the function consists in. Nor does it speak of fitness, alluding, rather, to ‘survival enhancing’ propensities. Regarding the former point, the remainder of the article cited makes clear that the specification of the function proceeds along the lines suggested above. Regarding the second point, as noted in ([22, 39]), the authors elsewhere appear to treat ‘survival-enhancement’ as roughly analogous to ‘selection-enhancement’.

SD accounts have been around for some time now, having enjoyed a certain popularity in the sixties and seventies, with the works of Canfield ([6]), Ruse ([34], [35]) or again Wimsatt ([40]). They have been the subject of a more recent revival of interest. Various incarnations of the view have been defended by Bigelow & Pargetter ([2]), Horan ([16]), Proust ([33]), as well as perhaps Walsh ([39]) and Boorse ([4]).²

Recent support for the dispositionalist position does not however come anywhere near to constituting a majority view and it seems fair to say that SD accounts of function have largely fallen from grace since their inception. This is largely due to the increasing popularity of the SH approach, whose proponents have done what appears to have been a convincing job of discrediting its dispositionalist counterpart. Amongst recent defenders of the SH approach one can find notably Millikan ([20], [22], [21], [23]), Neander ([27], [26]), Griffiths ([12]), Godfrey-Smith ([11]) and Schwartz ([37]), to name but a few.

In this paper I would like to take a closer look at some of the criticisms leveled against SD accounts. In particular, I would like to address five prominent charges, namely the charges that SD theories of function (1) fail to account for an explanatory force allegedly inherent in function attributions, (2) problematically require the current existence of trait-variants in the population, (3) fail to grant functions to so-called ‘malformed’ traits when they ought to, (4) are excessively liberal in the function attributions that they license and finally (5) do not enable us to draw sufficiently fine-grained functional distinctions. All five criticisms are, in my view, unfounded.

Before launching into the discussion proper, a brief preliminary clarification is in order. It is typically assumed in the literature that SD and SH accounts are in *competition*, having as their target one same analysandum.³ But the dispositionalist could of course conceivably try to deny this. She could then argue that existing criticisms of her view fail to have bite because the data that they rest on pertains to a *further* concept of biological function that the SD account does not set out to explicate. This, however, will not be the line of response that I shall be taking. First of all, the ‘rival theories’ assumption seems currently justified. Occamist considerations recommend, of course, that this option be the default one: *sensus non sunt multiplicandi preter necessitatem*. And furthermore, there are no compelling defeating grounds that urge us to think otherwise. Secondly, the assumption is one that is shared by the those whose arguments I shall be discussing in this paper. My rebuttal will be all the more effective to the extent that it can proceed on their terms.

‘SD accounts fail to account for an explanatory force inherent in function attributions.’ A number of writers have argued that there is something essentially explanatory about function ascriptions (e.g. ([43, 155]); further references in ([7, 741])). To state that a function of *t* is to bring about *F*, so the claim goes, is *ipso facto* to provide an explanation of the existence of *t*, or perhaps rather, of its prevalence in the population.

²Boorse’s article attempts to defend dispositional accounts in general against criticisms from the selection-historical camp. He himself appears to be torn between a positive value account and a ‘goal-directedness’ account in the style of ([25]). See also his earlier reply ([3]) to Wright’s general criticism of ahistorical accounts ([43]).

³It would follow from this, of course, that shortcomings of the one provide, *ceteris paribus*, grounds to favour the other.

If true, this claim would appear to yield a clear advantage for the SH account: after all, on this view, to attribute a function to *t* is to provide a partial explanation of its existence or population-level prevalence, via the past action of natural selection.

Now what is undeniably true is that requests for an explanation of the presence of a particular trait in an organism, or its prevalence in a population, are often met by the citing of a function in the present tense. For example:

- (1) a. Why do snakes have forked tongues?
 - b. Because it is a function of the snake's forked tongue to enable chemosensory tropotaxis.⁴

This would appear, at least *prima facie*, to provide some degree of support for the SH account. However, note that following response seems equally acceptable:

- (1) c. Because, in ancestral snake populations, it was a function of forked tongues to enable chemosensory tropotaxis.

It is difficult to see how SH-theorists would go about handling the felicity of (1c): if functional facts are indeed historical facts, their relevance to the explanation of *future* evolutionary trajectories is hard to understand. At best we have a stalemate.⁵

In fact, upon reflection, it would appear that it is SD accounts that have the upper hand here. The only one plausible explanation for the apparent adequacy of both (1b) and (1c) is surely the following. The SD account is essentially correct, functions are indeed to be identified with fitness-enhancing propensities, and strictly speaking it is (1c) that is the correct answer to (1a). However, we do typically use the (timeless) present tense construction exemplified by (1b) because, as a matter of fact, the forked tongues of snakes still have the relevant function, and to use the past tense would conversationally implicate the contrary. This conversational phenomenon—that is, the use of the present tense to avoid a misleading implicature—is in fact fairly common. Consider for instance the following exchange, in which two friends discuss a conversation that occurred the previous day:

- (2) a. Why did Gavin go cold on Yann yesterday?
 - b. It was Yann's same-sex civil union comment...Gavin doesn't approve of these practises.

But of course it patently is not because Gavin disapproves of same-sex civil unions *now*, that Yann's comments upset him *yesterday*. So strictly speaking, the correct answer to (2a) would be the past-tensed:

- (2) c. It was Yann's same-sex civil union comment... Yesterday, Gavin didn't approve of these practises.

⁴I read of this hypothetical function in ([41, 22–23]).

⁵Of course, their relevance to the *prediction* of future trajectories is not difficult to make sense of. As has been pointed out to me, one could still maintain that, to the extent that (i) a trait has been selected for in the past and (ii) the future will relevantly resemble the past, one might be entitled to expect future selection of the trait. This much is of course true, but irrelevant to the difficulty at hand: predictive relevance does not entail explanatory relevance.

But (2c), though technically correct, would be conversationally misleading: it would falsely imply that Gavin no longer disapproves today. Hence the use of the present-tensed (2b). These considerations carry over, of course, to responses to (1a).

‘SD accounts problematically require the current existence of trait-variants in the population.’ SD accounts of function, at least as presented in the introduction, appeal to *t*’s disposition to bring about *F* providing a ‘positive contributions to fitness’. This idea of ‘positive contribution to fitness’ obviously involves a comparison with a certain contrast case. But, if so, which contrast case is the appropriate one to consider? The issue is raised notably by Millikan, in an article critical of Bigelow and Pargetter’s ([2]) SD account of function:

... exactly in this sort of context, counterfactuals are notoriously indeterminate in truth-value. If a given individual with a certain trait were not to have it, what would that individual have instead? There is no such thing, for example, as being not monogamous. Is the individual then to be celibate? Or homosexual? Or polygamous? If polygamous, how many wives does he juggle? How does he employ them?... Suppose that you didn’t have a nose. Well, would you have gills instead? Or maybe a trunk? Or just two holes? A closed flap over the two holes so that you must breathe over your mouth? ([22, 40])^{6, 7}

After ruling out consideration of what would have been the case had the relevant trait been absent, Millikan then claims that the suitable contrast cases to be considered by the SD-theorist must be trait-variants within the current population of the bearer of the trait. This choice, according to her, is mandated by the linguistic practises current in contemporary evolutionary biology:

The notion of superior fitness, as actually used in evolutionary biology, is never taken to attach to any trait in a vacuum or absolutely but rather is understood only relative to alternative traits actually found in the population. ([22, 40])

But comparison with actual variants, she says, leads to the SD account to sanction counterintuitive functional judgments. Indeed, functional traits are surely not *required*, as a matter of conceptual necessity, to be represented in the population alongside various alternatives. For instance, the current human population’s not including (‘genetically’) noseless individuals would presumably not preclude noses having various functions. Millikan concludes that a selection-historical account, which makes reference to selection over *historically* present variants, provides the only sensible approach to the problem.⁸

⁶Worries about the specification of the relevant contrast case in SD accounts are not new. ([40, 51–55]) devotes a considerable amount of space to this issue.

⁷These arguments are repeated in ([23, 134–135]).

⁸One must assume that she takes noselessness to have constituted a historically present variant, or else her own account would fail to grant the relevant functions. Note that both ([1]) and ([5]) argue, from within the selection-historicalist camp, that Millikan’s requirement that a functional trait’s ancestral precursors must have co-existed with other populational variants is overly restrictive. ([11, 287–289]), however, disagrees.

But whilst Millikan's discussion certainly brings to the foreground an important issue, it falls way short, however, of delivering the intended conclusion. First of all, one wonders what exactly justifies the claim made in the first quote that the relevant counterfactuals are 'indeterminate' 'in this sort of context'. The invalidity of the so-called law of subjunctive conditional excluded middle (SCEM) is rather controversial, to say the least,⁹ and *a fortiori*, the claim that it fails *in the particular kinds of cases considered* would require even further work to establish.

It is not clear, however, that this line of argument was required in the first place. Even if the relevant counterfactuals *do* have determinate truth-values, a suitable account of function clearly ought to square with the theoretical interests of practicing biologists, as Millikan correctly points out. And it is far from clear that the aforementioned kinds of counterfactual-involving fitness comparisons are necessarily, if ever at all, high on the agenda.

Unfortunately, Millikan's assessment of the kind of comparisons that *do* matter to biologists betrays a strikingly narrow-minded view of the field. It is of course true that evolutionary biologists are often in the business of making fitness-comparisons between contemporaneous variants in a given population time-slice. They may, for instance, be in possession of data charting past population structure and wish to account for the relative rates of proliferation of various competing variants. But actual variants need not be the only variants of interest. The relative prospects of counterfactual variants is clearly relevant, for instance, in the field of evolutionary epidemiology. Here, researchers may for instance be interested in providing predictions of the evolutionary dynamics of a certain population of pathogens under uncertain future population compositions (they may, for example, be interested in the future profile of the meningitis population following a particular vaccination campaign). Similar concerns may be shared in the field of genetic engineering, where it would clearly be of interest to be able to evaluate the robustness of particular manipulations of population-composition with respect to the emergence of potential future variants. So it is simply untrue to claim that biologists, writ large, focus exclusively on measures of comparative fitness between co-existing variants.

Still, one must grant that all this would appear to leave the SD-theorist with an embarrassment of riches: if the appropriate contrast situation to consider (i) is not necessarily the situation that would have obtained had the actual one not done so, and (ii) needn't consist in the possession an alternative trait represented in the current population, then what *is* the appropriate contrast situation?

Taking cue from an increasingly influential view in the philosophy of causation,¹⁰ an obvious, and not entirely implausible, response on the SD theorist's part would be to give up on the view that function ascriptions assert the obtaining of a binary relation between a trait and an outcome. Rather, the suggestion would go, function ascriptions are essentially *contrastive* and assert the obtaining of a *quaternary* relation between a trait *t*, a contrasting trait *t**, an outcome *F* and a contrasting outcome *F**. This structure may be implicit, to be recovered from conversational context by the hearer. It may be

⁹For the classic defense of its invalidity, see ([17, sec. 3.4]). For the—again classic—defense of the contrary view, see ([38]). Note that Lewis himself, in rejecting SCEM, notoriously admitted that the principle does seem to square with commonly-held intuitions.

¹⁰See for instance ([13], [15], [14]), ([19]), ([29]), ([36]).

partly explicit, with the speaker relying on prosodic stress to guide the hearer towards the correct interpretation. For example:

- (3) The function of *egg* rejection in hosts of the Eurasian Cuckoo is to reduce the risk of false positives.

Finally—and perhaps more rarely, due to the fact that combination of context and prosodic stress should generally suffice to disambiguate the statement—the contrastive structure may be entirely explicit:

- (4) The function of egg rather than chick rejection in hosts of the Eurasian Cuckoo is to reduce the risk of false positives associated with the latter.¹¹

It would then follow that there is no issue of finding some overarching semantic principle to select an appropriate contrast case. The relevant contrast case would simply be the one intended by the speaker: it may be the situation that would have obtained had the trait in question not been possessed, it may be the possession of some competing trait represented in the population, but then again, it may be some different alternative altogether.

‘SD accounts fail to grant functions to so-called ‘malformed’ traits when they ought to.’ Another common criticism of SD approaches is that they have difficulty in dealing with what one might want to call ‘defective’ members of biological categories, such as glaucomic eyes, cirrhotic livers, collapsed lungs and withered hands.¹² The critic maintains that these items have functions despite lacking the relevant capacity to perform them, in clear contradiction to the SD account.

This position on the functions of ‘defective’ traits is deeply engrained in the literature. So deeply, in fact, that nobody appears to have noticed that it runs squarely counter to biologists’ actual linguistic practises. In biochemistry, for instance, a denatured protein is said to ‘lose its function’ when it loses its fitness-enhancing capacity. Again, in pathology, one might speak of a loss of neurological function due to brain ischaemia, or a loss of kidney function due to amyloid build-up.¹³ In genetics, one speaks of a ‘loss of function’ or ‘gain of function’ mutation to designate a mutation resulting in, respectively, the disappearance or appearance of a selectively beneficial capacity. This, incidentally, clearly contradicts Neander’s claim that “according to current usage, a new mutation has no function” ([27, 465]).

Lack of fitness-enhancing dispositions arguably also underlies our intuitions concerning that lack of current function of so-called ‘vestigial traits’ such as, in the human species, the appendix, or the fused vertebra that form the coccyx. ([12]), ([11]) and ([37]) do attempt to do justice to these intuitions from within the SH framework by requiring of functional traits that their ancestral counterparts have been subject to

¹¹See [31].

¹²See ([27, 466], [26, 182]), ([32, 18–21]), ([43, 146]) and ([21, 20–26]).

¹³In criticism of the SD view, ([18, 72]) writes, of a heart that loses its capacity to pump, that “if function is defined only in terms of actual contributions to demands, we would have to say... that it has lost its function”. Yes we would have to say this but, as a matter of fact, in spite what Manning appears to suggest, we do.

selection in the *recent* past. However, note that the term ‘vestigial trait’, as used in biology, appears to have little to do with the issue of whether or not a trait was recently selected for. Indeed, one would speak, in developmental pathology, of developmental mishap resulting in the development a ‘vestigial’ penis, ovary, or ocular system.¹⁴

In view of all this, one must of course wonder what exactly it is that led the critic to endorse the position that she did. From the vantage-point of the SD account, one could provide two plausible diagnoses. The first might be a failure to take into account the non-monotonicity of the inference from (1) a type-level functional generalisation such as ‘normally, it is a function of *A*’s to bring about *F*’ and (2) an assertion of type-membership such as ‘*t* is an *A*’, to (3) a token-level function ascription ‘it is a function of *t* to bring about *F*’. The inference can be defeated by the addition of further premises such as, most obviously, the fact that *t* is not disposed to bring about *F*. The second reason may lie in the fact that, even if the SD account is correct, one would still expect on pragmatic grounds that a query as to the function of a defective device to be met by a citing of the function that it was selected for to have. Indeed, many of the defective devices discussed exhibit various properties (pertaining to complexity, internal structure, component materials and so on) that we typically know are correlated with the enhancing of the fitness of some organism or other. These properties provide defeasible grounds to assume that the device in question has a function of some sort. On the basis of these properties, and in the absence of any further data, someone may wish to enquire as to what specific functions the device may have. Now consider a respondent in the possession of further information to the effect that although the device was selected for on the basis of its disposition to bring about *F*, it has no current fitness-enhancing properties. This respondent has two salient conversational options: (i) to reply that the device has no function, (ii) to reply that it was selected to perform function *F*. Option (i) would explicitly answer the question but leave the enquirer yearning for an explanation of the function-indicating properties of the device. Option (ii), on the other hand would (a) answer the question implicitly, by conversationally implicating that the device has no function, whilst at the same time (b) providing an account of the device’s possession of function-indicating properties. (ii) is clearly the better conversational option of the two.¹⁵

‘SD accounts are excessively liberal in the function attributions that they license.’

Another common charge made against SD theories is that they are overly liberal in their attributions. For example, the impressive frill display of a frill-necked lizard might, in the presence of a particularly soft-hearted hunter, perform the useful task of singling out the lizard as a particularly magnificent beast whose death would be a great loss

¹⁴As an aside, note that the SH account would also appear to run into trouble with the distinction between functional and dysfunctional traits. Presumably a functional trait is a trait that has one or more functions. There is no obvious sensible alternative way to interpret the term. This, the SH account will interpret as being a trait that owes its presence, or current prevalence in the population, to the effects of its ancestors. Again, an afunctional trait will be understood as a trait *not* owing its presence, or current prevalence in the population, the effects of the dispositions of its ancestors. But what to make of the notion of dysfunctional trait? Indeed, a dysfunctional trait cannot be a trait which owes its absence to the effects of the dispositions of its ancestors (since, by hypothesis, it is present).

¹⁵Note that I am not alone in thinking that it is a mistake to say that items can have functions which they fail to be disposed to perform. Boorse holds the same view ([4, 88–90]).

to the biosphere. We would nevertheless be reticent to grant the frill a function of inducing awe and restraint in hunters (this example is inspired by ([10, 266])). It is often claimed that an appeal to selection-historical facts yields a more conservative proposal. The bringing about of the aforementioned events will presumably not have played a role in the selectional history of the bearer of the relevant propensity, and so will not qualify as a function. This argument for bringing in etiologies goes as far back as Wright's critique of the early SD accounts of Canfield and Ruse ([42, 512–513], [43, 147]) and has been repeated by ([22, 37–38]).

However, as has been rightly pointed out in ([2, 191]) and ([4, 71–72]), selection-historical analyses are just as vulnerable to these counterexamples as their dispositionalist counterparts are. Say for instance that some 150 years ago a frill-necked lizard encountered a hunter in the bush and that, at that precise moment, it exhibited a timely display of its frill. Say furthermore that, upon witnessing this rather wonderful sight, the compassionate hunter decided to spare the animal's life and returned home empty handed. The attribution of the relevant function to the frill is surely no more compelling when the relevant etiologies are granted.¹⁶

The correct way to deal with this case is to note that the circumstances under which the trait in question makes this kind of contribution to fitness are exceedingly rare. As a result of this, the disposition to bring about the relevant outcome has a low expected contribution to fitness and the corresponding function is relatively unimportant. Citing such an incredibly insignificant function would generally be considered conversationally inappropriate, in much of the same way as would be citing the risk of getting hit on the head by a falling window cleaner as one of the dangers of urban pedestrianism.¹⁷

This point is also relevant to two further alleged counterexamples to SD accounts. There is first of all the oft-cited case of the convenient way that noses allow for the wearing of spectacles. Supporting spectacles, so the contention goes, is a useful thing that noses do but it is not one of their functions. However, note that conferring the ability to wear spectacles, while a useful feature of noses, is not a *very* useful feature. The conferring of an ability to sport a pair of spectacles is not, on the whole, across all owners of noses, something which increases fitness drastically (although perhaps it does do so for a handful of extremely poor-sighted people who do not have access to contact lenses). Furthermore, there are a number of further, more noteworthy, fitness-enhancing consequences of having a nose. The ability to support spectacles is simply typically not a salient function of noses in relation to other functions that noses perform, hence the typical conversational infelicity of citing the provision of ability as the—or even a—function of noses.

Issues of low expected contribution to fitness are also central to a case presented by Enc and Adams. They ask us to consider at time t_1 a species of butterflies with a specific wing-coloration pattern. These butterflies are preyed upon by a certain type of bird prevalent in the environment. “By sheer chance”, a particular kind of new species of butterflies arises at a later time t_2 . These butterflies have the same markings

¹⁶To be exact, Millikan would require not only the sparing of some ancestral lizard equipped with a frill, but the killing of a frill-deprived conspecific of that lizard ([22]). This additional requirement, however, makes absolutely no difference to the implausibility of the function assignment.

¹⁷A similar assignment of the blame can be found in ([40, 50]), ([3, 80], [4, 71]), ([39, 567]), and ([2, 252]). The diagnoses given differ somewhat in form, both between each other and in relation to mine.

but are poisonous to the predators. The birds develop an aversion to the wing colourings, enabling both species of butterflies to avoid predation. Adams and Enç “do not think that the [SD] account has any safeguards that would prevent our saying that at t_1 , the particular colour pattern of the first species had a function—it certainly conferred a survival-enhancing propensity to that species, that is, it increased the probability of that species surviving past t_2 .” ([9, 379]). The reply is straightforward: at t_1 the coloration conferred a survival-enhancing propensity to the extent that the conditions for wing coloration to lead to an event that increased the long-term survival chances of the relevant replicators were probable. At t_1 the chances were extremely small of there appearing identically-coloured, albeit poisonous butterflies, in sufficient number to render the potential predators averse to preying on bearers of the particular wing coloration. Indeed, as Adams and Enç set the scenario up, the new species arises in t_2 “by sheer chance”. It is therefore not surprising then that we should withhold function-attribution at t_1 .

‘SD accounts do not enable us to draw sufficiently fine-grained functional distinctions.’ The final argument which I would like to discuss is due to Sandra Mitchell. At the heart of her article ‘Function, Fitness, and Disposition’ ([24]), is a thought experiment designed to show that intuitions can grant different functions to systems with identical propensities, provided that the evolutionary histories differ, thus favouring the SH account over its SD counterpart.

She asks us to consider the well-known case of Monarch butterfly wing-colouring-mimicry by the Viceroy Butterfly. The Monarch butterfly is a common, brightly-coloured North American butterfly. Its conspicuousness might *prima facie* seem to be evolutionary disadvantage, by putting it at greater risk of predation. The Monarch caterpillar’s diet, however, turns out to be rich in cardenolides, rendering both the caterpillar itself and the butterfly it subsequently turns into unpalatable to predators. The bright coloration thus enables the Monarch to ward off any prospective consumers by advertising its unsuitability as a potential foodstuff. Now, as it happens, another species of butterfly—the Viceroy butterfly—which is itself perfectly palatable, has hopped onto the bandwagon, mimicking the Monarch’s coloration patterns and thus landing itself a useful means of avoiding predation. Mitchell argues that although (i) the markings of the two species confer identical fitness enhancing propensities, (ii) they nevertheless have distinct functions:

Two types of organism, the Monarch butterfly and the Viceroy butterfly, are structurally similar or indistinguishable... the morphological structures have the same future consequences... The function of conspicuous coloration in the Monarch is to warn the predator of its unpalatability. The function of the Viceroy coloration is to mimic the model and deceive the predator into presuming it is unpalatable and thus avoid predation. The same structure has two functions, one to warn and one to deceive. ([24, 405])

I would like to suggest that (ii) is correct, that we do have functional differences, but that (i) is false, that is, that the difference in function can be spelled out in terms of fitness-enhancing propensities. Here is how this is done.

One immediate benefit of the markings is the propensity that it confers to put off predators (*F*). This is the ‘instant gratification’ aspect of markings, and it is something which Monarch and Viceroy markings alike manage to do. But note, however, that this achievement is contingent on the obtaining of a further environmental condition, namely on its being the case that predators avoid hunting butterflies with the relevant markings (*C*).

But now we can clearly see that Monarchs differ from Viceroy butterflies in that their markings are disposed to bring about *another* beneficial state of affairs, besides *F*: *C* itself. Indeed, the Monarch’s markings have a further property which the Viceroy’s lack: they are a reliable guide to unpalatability. They thus increase the probability that *C* will hold, thereby enabling *F* to be brought about more frequently. The markings of the Viceroy butterflies perform no such function. Indeed, the Viceroy butterflies are reliant on the fact that Monarchs will be doing the job for them. So Monarch and Viceroy markings do, *contra* Mitchell, have different fitness-enhancing propensities after all and the argument simply fails to go through.

Concluding remarks. It would appear that the SD account of function may have been a little too hastily dismissed. There are to my knowledge, at this point in time, no convincing arguments against the view.

This should come as good news to those who believe that natural selection is the key to understanding biological function. Indeed, the SH approach itself has its fair share of problems. I have already suggested that (1) the SH account faces the problem of accounting for the fact that citations of *ancestral* functions appears to constitute perfectly legitimate answers to requests for explanations of *current* trait distributions. Furthermore, I have pointed to (2) linguistic evidence to the effect that, contrary to what is claimed by SH-theorists, intuitions indicate that loss of ability to perform a function entails a loss of the function itself.

More importantly, (3) many commentators simply find the historicalist commitment of the SH account hard to swallow. According to the SH view, a molecule-for-molecule replica of oneself, situated in the very same environment, but which nevertheless was not the product of natural selection would be endowed, for instance, with kidneys, eyes, and a hormonal system that would lack any biological function whatsoever. It has been complained, notably by ([2, 188]) and ([3, 74]) that this is a counterintuitive result.

The problem becomes particularly acute for the increasing number of philosophers of mind who see in biological function a key to providing a naturalistic account of mental content: conjoining a biofunctional account of content with a selection-historical account of biological function leads to denying a fully-blown mental life to such replicas. This counterintuitive consequence of the SH account is sometimes met by claiming that its cost is offset by a number of further advantages of the SH view ([28]). But these, however, include most notably its ability to grant functions to defective members of biological categories as well as its ability to account for an inherent explanatory force of function ascriptions. And if the arguments in this paper are correct, these ‘advantages’ are illusory.¹⁸

¹⁸Other attempts to defend the SH view against this type of criticism include ([8]) and ([30, 92]).

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