THE ROLE OF SELECTION IN FUNCTIONAL EXPLANATIONS[*]

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Abstract: In this essay I will argue that natural selection is more important to functional explanations than it has been normally thought in some of the literature in philosophy of biology. I start by giving a brief overview of the two paradigms cases of functional explanations: *etiological functions* and *causal-role functions*. I then consider one particular attempt to conciliate both perspectives given by David Buller (1998). Buller's trial to conciliate both etiological functions and causal-role functions results in what he calls a *weak etiological theory*. I argue that Buller has not succeeded in his construal of the weak etiological theory: he underestimates the role that selective processes have in functional explanations and so his theory may not be classified as an etiologial theory. As an alternative, I consider the account of etiological functions given by Ruth Millikan (1984) and I argue that Millikan's theory is more comprehensive to assess contentious case in biology like exaptations. Finally, I conclude by analyzing where the adoption of Millikan's theory leave us. I argue, contrary to Millikan and others, that once we assume the importance of natural selection in functional explanations, there is no strong reason to resist a linguistic reform of the word function and hence that the attempts to conciliate both etiological functions and causal-role functions are misplaced.

Keywords: natural selection, functional explanations, etiological theories.

INTRODUCTION

The attribution of functional explanations in science has been the subject of much philosophical debate in the last forty years. Two main conceptions of function are taken to cover distinct explanatory projects by some philosophers (see, e.g., Godfrey-Smith 1993). On the one hand, there is the etiological conception of function formulated by Larry Wright (1973) in which functional explanations are taken to be explanations that make essential reference to the history of the object which is the target of explanation. On the other hand, there is the causal-role functional analysis proposed by Robert Cummins (1975) in which he emphasizes the importance of employing a top-down strategy in functional explanations. In other words, the function ascribed to a complex system S must be explained in terms of the functions executed by the subsystems (s_1, s_2, s_3, etc.) that form S.

David Buller (1998) has argued that a conciliation between these two explanatory projects may be given. Buller draws a distinction between the various positions taken on the literature about etiological functions: he speaks of differences of commitment in what he calls 'strong etiological theory' and 'weak etiological theory'. For Buller, the strong theory defenders commit themselves to a sort of restriction given by the fact that this theory entails that there must be selection for a trait in order for it to have a function. It is with the degree of commitment of functional explanations to selective processes that I will be concerned in this paper.

In section I I will provide an overview of the conceptions of functions discussed in the recent literature, pointing out to their main commitments and what should be satisfied in order to give a proper explanation following each strategy.

In section II I will present Buller's proposal to conciliate both conceptions of function and we shall look with more detail at what is intended in Buller's commitment to the weak etiological theory.

In section III I will analyze the consequences of Buller's assumptions at greater detail. I will argue that the weak etiological theory as presented by Buller does not satisfy one of the core concerns of the etiological theory because it fails to recognize the importance of selection in functional explanations, which prevents one from drawing the essential distinction of functions and mere effects.

In section IV we will look at some conceptual tools that will allow us to provide a defense of the strong etiological theory. I will suggest, contrary to what other authors have supposed, that the ambiguity between both notions of functions is only an apparent ambiguity and that boundary cases such as spandrels or swampmen should be reconsidered in light of the strong etiological theory of functions. For this, I shall give a brief discussion of the controversy of the concept of 'exaptation' in biology and show how it relates to the discussion of function presented here.

I. ETIOLOGIAL FUNCTIONS AND CAUSAL-ROLE FUNCTIONS

The talk of functions in scientific explanations has been the motive of a heated debate in the 1990's. Polarized by two opposed conceptions of functional explanations, the debate has lurked around the possibility of giving a unified account of both these theories. In this section I consider each theory in turn and I dedicate most of the discussion to criticize a particular trial of conciliation given by David Buller (1998).

Etiological functions

Etiological functions were first developed by Larry Wright (1973). Wright believed that functional explanations were a response to 'why' questions such that when one proposes a functional explanation, one is bringing about some prior factors about an object A to explain a certain aspect b of A.

This is best illustrated in biological cases. When we take a fairly simplified textbook account of the function of hearts, we come to something like the following sentence: the function of hearts is to pump blood. Many empirical and theoretical considerations could be drawn from this sentence, but what matters for us is that it is a straightforward answer to an important why question: when we say that the function of hearts is to pump blood, we are saying that to pump blood is the motive *because* of which hearts exist. As Wright himself puts it:

> The function of X is Z *means:*
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> (a) X is there because it does Z
> (b) Z is a consequence (or result) of X's being there. (Wright 1973, p. 161)

So, according to Wright's assumptions, explaining why X exists makes an explicit reference to what it does (and, implicitly, to why it does that) only insofar as this effect Z is also explainable because X in fact exists. The two sentences formulated by Wright can be considered the two main conditions for a thing to be functional according to the etiological theory of functions. As a matter of fact, they set two important explanatory agenda attributed to etiological theories: a functional explanation following these two criteria allows us to explain *why* a thing exists (Wright's condition 'a') and *why* it does what it does

(Wright's condition 'b'), that is, it makes clear that to do Y is rather a *function* than a mere *effect* of X.

Causal-role functions

Causal-role functions, contrary to etiological functions, are not constrained to prior facts about the object of explanation. It rather places the burden of the functional explanation on what an object X does actually. To see this more clearly, imagine that for some crazy reason we started to use forks as bookmarkers. It seems intuitive to say that in this strange case forks can be said to *function* as bookmarkers, but there is indeed no prior fact at forks history that explain why they came to have that function[1].

Cases in which we employ functional ascriptions of this sort were thoroughly described by Robert Cummins (1975). According to Cummins, functional explanations are intended to show how a particular object or biological trait does the task it actually does or would supposedly do in case it were properly displayed in certain conditions. For Cummins, explaining the function of a biological trait is not a matter of looking at its evolutionary history and telling how it came to have that function. Saying that the function of the heart is to pump blood because it was selected for that function is not a proper functional explanation in Cummins' approach. Rather, Cummins believes that a functional explanation must explain how hearts can perform their tasks, that is, how they can circulate blood.

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[1] Consider Wright's (1973) assumption (a) and (b). We could not construe an etiological explanation in this case because to mark pages on a book *is not* the motive why forks exist.

So, causal–role functional explanations (often referred to as Cummins functional explanations) are not concerned with whether hearts circulated blood before in their history. All we need to know is that they do circulate blood now and this is enough to ascribe them a function. At this point, though, it seems reasonable to ask: how is such an explanation possible, that is, how do we explain the fact that hearts circulate blood?

This question is addressed by Cummins (1975) and the answer to this is what settle the title of his paper. In other words, in order to explain how hearts can circulate blood (or, more broadly, how X can perform Y) we need to employ a *functional analysis* of a system (in this case, the heart). Functional analysis, often referred to as top-down approaches or decomposition strategies, are very common in artificial intelligence and physiology, so I will give it a very brief discussion. When we want to explain how a complex system S that realizes a function Y can do so, we can decompose the complex task that is Y into the work done by the subsystems of S. The work done by S is thus analyzed by means of the work of its subparts (s_1, s_2, s_3, …, s_n) and hence the complex job of doing Y can be explained by the operation of small parts, which in turn realize simpler jobs than S. To make the point more vividly, it is worth quoting Cummins here:

> Production [Cummins is referring here to assembly-line production] is broken down into a number of distinct tasks. Each point on the line is responsible for a certain task, and it is the function of the workers/machines at that point to complete that task. If the line has the capacity to produce the product, it has it in virtue of the fact that the workers/machines have the capacities to perform their designated tasks, and in virtue of the fact that when these tasks are performed in a certain organized way – according to a certain program – the finished product results. Here we can explain the line's capacity to produce the product – i.e., explain how it is able to produce the product – by appeal to certain capacities of the workers/machines and their organization into an assembly line. (Cummins 1975, p. 760)

These are the two conceptios of functions that I will be concerned with in this paper. I now turn to Buller's (1998) proposal to provide a unified account of functions and I try to show that he has not succeeded in his task.

II. THE CONSENSUAL USE OF FUNCTIONS AND THE 'WEAK' ETIOLOGICAL THEORY

Our considerations in the last section seem to indicate that there are two distinct concepts of functions recognized in the literature. Apparently, both serve specific explanatory projects. On the one hand, the etiological theory focuses mainly on the history of selection of a device whereas the Cummins' functions are concerned with the actual and dispositional capacities of that device. Some philosophers have argued that we can find a consensual use of these two notions[2], which seems to contrast with what we have described so far.

One of these unification proposals is that defended by David Buller (1998). Buller has proposed that what we call under the univocal label of 'etiological theories' are theories that not necessarily make the same theoretical commitments. According to Buller, there are two types

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[2] This is the case of Kitcher (1993), Griffiths (1993) and Buller (1998), for example. I will examine Buller's proposal in this section. For opposite positions in the subject, see Godfrey-Smith (1993), Millikan (1989a, 2002). Godfrey-Smith recognizes that both concepts of function serve different explanatory purposes and that a unified account would not help in this case. Millikan, on the other hand, thinks that almost all the cases (in biology) where we apply Cummins' function can be equally done with her notion of 'proper functions', which is a version of the etiological theory. For more on this, see section IV.

of etiological theories, namely, *strong etiological theories* and *weak etiological theories*[3].

      The main difference in both theories regards the importance given to the selection in determining the functional status of a device or a trait[4]. To understand this, consider a hypothetical case suggested by Buller (1998, p. 512). Imagine that a trait T plays a certain causal role in some biological process, say gamete production, to use Buller's example. For reasons not important to our discussion, consider that one of these assertions could be true in that environment: (a) mutations never occurred in this population or (b) the mutation in traits T occurred in that population, but it was not among individual in the same selective environment. In this context, if the trait T plays a causal role C that contributes to the gamete production, then playing C will increase the individuals' fitness by augmenting its capacity to produce gametes and hence increasing reproduction rates. Consequently, as long as T contributes to the surviving of an organism O, this trait should be maintained in subsequent generations. Intuitively, we could say that playing C might be said to be the function of T since it contributes to the survival of O.

      Note that there must not have been selection is this environment since (i) there was no mutation and thus no difference among T, or (ii) there were mutations, but they did not occur among individuals in the same selective environment. In other words, as Buller points out, there may not be selection of trait unless (a) T varies among a population, and (b) this variation happens within a common selective

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[3] Hereafter 'strong theory' and 'weak theory'.
[4] Given that Buller's discussion is concerned with biological cases, from now on I will restrict the analysis to biological cases and hence natural selection.

environment (Buller 1998, p. 508). Therefore, we might be in a position to conclude that selection is not necessary for a trait to be functional.

Buller claims that his theory is an etiological one, but this last conclusion seems to be at odds with one of the claims made in our earlier considerations of etiological theories, since this indicates that the essential role selection was supposed to play in determining the function of a trait is not so essential after all. If what we saw in section I about etiological theories is correct, then one tension that arises here regards the fact that the statement that selection is not a necessary condition for a functional ascription is not sufficient to provide a legitimate etiological theory. So, how are we going to reformulate the definition of etiological theory gave in section (if that is possible) in order to accommodate this new conclusion?

In fact, it makes sense to talk about *weakening* the requisites to consider a trait functional in the light of the hypothetical case presented above. But, again, one might question, how are we going to do that?

To answer this, let's consider Buller's hypothetical example with more detail. The trait T responsible for playing a causal role C that benefits gamete reproduction clearly increases the fitness of an organism, and thereby it is a beneficial trait. Also, if there is an increase in fitness, there is an equally increase in the survival rate of these organisms, which in turn will enable them to leave more offspring. Consequently, individuals in that population will have a higher probability to develop the trait T in such a way that T will be likely transmitted over the subsequent generations. These considerations seem to indicate two main points essential for a functional ascription to T on this particular case: first, T must contribute to the organism's survival and hence it must increase its *fitness*. Second, T must also be transmitted over the generations, or, in other words, it must be

hereditary. These two conditions are, according to Buller, the common assumptions made by both weak and strong etiological theories[5]. But, as one might have already noted, there is more to be said about the gamete production case than conditions (a) and (b) above. So, Buller proposes, following these considerations, that for a trait to be functional it (c) must contribute to the organism fitness, (d) must also be hereditary, but (e) it need not have been selected to perform a particular function. In Buller's words:

> A current token of a trait *T* in an organism *O* has the function of producing an effect of type *E* just in case past tokens of *T* contributed to the fitness of *O's* ancestors by producing *E*, and thereby causally contributed to the reproduction of *Ts* in *O's* lineage. (Buller 1998, p. 507)

This is Buller's *weak etiological theory*. At this point, though, one might be wondering how such thing as an increase in fitness can happen if there is no selection to measure it. That is, how can a trait be said to contribute for an organism survival if there was no variation of it among individuals in a common selective environment? How are we going to determine that this trait in fact contributed for an organism success in the environment in which it is inserted?

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[5] "Both theories entail that a trait *T* has the function of producing an effect of type *E* in an organism *O* only if *T* satisfies the following two conditions:
(ET1) T must have contributed to the fitness of O's ancestors by producing effects of type E, and

(ET2) T must be hereditary.

Since both theories are committed to these two conditions, I will take them to represent the essential commitment of the etiological theory." (Buller 1998, p. 511)

This is where Buller says causal-role functions or Cummins' functions are important[6]. In our hypothetical case, there was not selection for *T*, so, by the strong theory *T* would not be functional. However, *T* is a part of a complex organism *O* and clearly it increases *O*'s fitness by increasing gametes production. By these considerations, if we take *O* as a complex system in Cummins' sense, we can employ a top-down approach in such a way that we can determine the functions of its operating parts (of which *T* is one) so that we can ascribe an actual function *C* for *T*. By playing a Cummins' function *C*, then, *T* increases fertility, which in turn increases *O*'s overall fitness. As a consequence of this, the function *C* of *T* is determined by the causal role it plays in *O* which contributes to its overall economy, so, *C* can be said to help in executing *O* complexes tasks which directly increase its survival chances.

It follows from this that as long as *T* contributes to *O*'s fitness by playing a specific Cummins' function *C*, it tends to be transmitted over the generations and thus it becomes a hereditary trait without there being selection for that function. Consequently, *T* becomes functional in the etiological sense since it has a history of heredity among generations.

Our discussion has so far indicated two fundamental points of the weak theory: (i) being selected is not a necessary condition for a trait to be functional; and (ii) the fitness of a trait *T* is to be defined by means of the Cummins' function *C* this trait *T* plays in an organism *O*'s overall economy in surviving in a certain environment.

Buller's (1998) main claim is that his unified theory satisfies all requisites to a theory to be an etiological one. A brief overview of his

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[6] I will be referring to causal-role functions as Cummins' functions.

arguments is necessary for our further discussion. Actually, let's consider the three objections analyzed by Buller.

The first one is due to Ruth Millikan (1989a, 1989b). Millikan has argued that Cummins' analyses are too liberal in ascribing functions to a system. Consider the water-cycle[7]. If we consider this cycle as a complex system just as Cummins' suggests, we could, for instance, say that the function of clouds is to make the vegetation grow. That would be a consistent analysis since the actual causal powers of clouds can produce rain, which maintains the soil wet, which in turn favors vegetation grown.

This objection, according to Buller (1998, pp. 516-518), does not offer difficulties to the weak theory. The weak theory, argues Buller (1998, p. 516), does weaken the selection requirement, but it does not mean that selection is excluded at all. According to the weak theory, there is no selection for traits, but only selection for adapted organisms. Hence, what is subject to selection are organisms or complex systems that exhibit fitness. It follows, therefore, that the water-cycle is not to count as subject to selection since it does not exhibit fitness and thus making the vegetation grow is not considered a function of clouds since the water-cycle was not selected in the same sense that a biological organism was.

The second and third objections have to do with the same point, so let us consider them together. It could be said that the weak theory not only does not explain malfunctioning of some traits, but also

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[7] In fact, Millikan (2002) argues that Cummins' functional analysis requires background conditions, which sets a difficult for this approach, since it is not clear how we can determinate what is to count as a complex system. I will consider this objection with more detail in section III, where I present the difficulties related to a unified account of functions.

that it does not explain why that trait is there. What can be said about these two cases is similar. First of all, the weak theory, according to Buller, does not ascribe functions to actual items, but rather, it does ascribe functions to past items that contributed to the selection of an adapted organism. It is a consequence of this that the function to realize *C* is a function of *O* because its ancestors performed that function and thereby the fact that *T* does not perform its function *actually* does not change the fact that other tokens of *T* had done so in the past. Consequently, *T* can be said to malfunction even though we support a Cummins' functional analysis.

The fact that past tokens of *T* performed *C* is also very illuminating to answer the third objection. That is, we can explain why *T* is there exactly because it has performed *C* which hereditarily helped its ancestors *O* to survive and hence to be selected. Therefore, this means that *T* exists actually because it is a consequence of its past tokens playing *C* in *O*'s ancestors.

Buller's weak theory as we have presented so far seems to be in accordance with what is required by the two core statements made by Wright (1973). In the next section I will argue that this seemingly conclusion should be resisted because the conception of fitness based in a Cummins' functional analysis and the weakening of the selection's importance makes the weak theory fails to meet one of the core concerns of the etiological theory: its capacity to differentiate functional effects from mere accidents.

III. PROBLEMS WITH THE WEAK THEORY

A problem regarding our definition of the etiological theory in section I emerged in the course of our presentation of the weak theory. Roughly, what seems to strike our initial claim is that there might be

cases (the one of the increase in gametes production presented by Buller) where selection does not act upon a trait T, but yet we would not quit ascribing T a function. This has driven Buller to weaken selection as a necessary condition for functional ascription. And this weakening, in turn, required a different definition of fitness, since selection could not be anymore a measure for it.

We will see in this section that the role played by selection in functional explanations is much more important than Buller thought. In fact, the problems with the weak theory that we will be analyzing here arise right because of this weakening of the role played by selection plus a definition of fitness in terms of Cummins' functions. I take these two claims to be the central ones in differing the weak theory from the strong theory.

I shall start with the latter, that is, the definition of fitness by means of Cummins' function. As we have seen in section I, Cummins (1975) has argued that functional analysis consists in taking a determinate system S that realizes a complex task C and divides the job done by C into many S's subsystem until eventually we reach the level of physiological analysis. This is, however, a problematic statement. For how are we ever going to determinate which is the 'right' complex task to be analyzed?

To understand this, consider the case of hearts. The function of the heart is taken to be that of circulating blood. Now imagine a common case in a doctor appointment. Doctors usually use their stethoscope to hear your heartbeats in order to see whether everything is ok with your heart. As we know, this is a very common practice and a very useful one. In this context, we could, by a pure Cummins' functional analysis, attribute a complex task to hearts, that is, to produce noises, and then divide this complex task in simpler tasks

distributed all along the heart's subsystems[8]. But that would sound very counterintuitive. This is one example of Millikan's (2002) complaint about the liberality of Cummins' functions.

Millikan (1999, 2002) suggests that determining what is to count as a complex system (which she calls Cummins' system) is strictly dependent on our explanatory purposes. So, if the noises made by hearts are interesting for our explanatory project, we can employ a Cummins' functional analysis of it.

Moreover, it is not clear where to set the boundaries for a Cummins' system. Certain background conditions are needed in order to a Cummins' functional analysis to be valid. Consider your refrigerator. For it to realize its Cummins' function, namely to keep the inner part in a determinate temperature, a lot of other 'external' conditions to the analysis must be satisfied. For instance, if the refrigerator is one that runs by 220V, it must be plugged to a 220V electric source or else it will not realize its function. Also, it should be made of certain material so it can stand properly on the ground; the outside temperature needs to be an adequate one since too low temperatures would make the refrigerator useless; it must have a power button so people can use it; and so forth. These kinds of considerations lead Millikan (2002) to say that Cummins' systems are ideal systems.

As Millikan (2002) points out, in the cases of artifacts such as the refrigerator these background conditions are more or less determined by the information containing in its manuals, which are, by their turn, created by the refrigerators' designers. The problem, though,

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[8] "For no matter which effects of something you happen to name, there will be some activity of the containing system to which just those effects contribute, or some condition of the containing system which is maintained with the help of just those effects." (Cummins 1975, p. 752)

is where are we going to find the manual for the functioning of the hearts? In other words, what will determine which are the right conditions for hearts to perform their functions?

At this point, the selective history of hearts seems a reasonable candidate to answer this question. A functional explanation of why hearts were selected for blood circulation must be sensitive to the conditions in which hearts used to perform better than variations of it. That is, hearts as they actually are must have been the best alternative natural selection had in a determinate period of the evolutionary history; they must have shown the best performance among variations of hearts. Thus, we know, by looking at the selective history, what were the conditions at which the ancestral tokens of this heart were selected among other hearts. It follows from this that we can determinate at a certain degree what are the conditions necessary for hearts to work properly, namely the conditions under which its ancestral tokens were selected to realize their functions.

Selective history, both in the case of artifacts or biological devices, is therefore required to establish the background conditions upon which Cummins' systems are supposed to operate. Note that any constraints regarding our knowledge of the selective history behind a trait is a matter of an epistemological limitation and not an ontological one.

Turning back to the weak theory, it becomes problematic in face of these considerations. First of all, if our considerations are right, a determination of fitness based on a Cummins' analysis of ancestral tokens of hearts would necessarily need to mention some prior conditions under which these tokens operated or else the Cummins' analysis would be an arbitrary one, since we would not be able to establish the boundaries of our Cummins' system.

In this sense, fitness cannot be a mere analysis of the beneficial role played by a trait in a Cummins' system, for the selective history is necessary to determinate background conditions in which a Cummins' system can realize its function in an adequate manner. So, when the weak theory commits itself to a Cummins' analysis in order to determine fitness, it is not sensitive to the fact that Cummins' functions are only so when considered against a specific background[9].

So far we have seen that without the aid of selective history, the determination of a Cummins' system background becomes quite arbitrary and the notion of fitness does not make sense in this context. The difficulty with the weakening of selection was already implicit in the discussion above, but that is not all there is to be said about it.

A great deal of philosophical debate has come from the conception of 'exaptations' in biology (see, e.g., Gould and Vrba 1982 and Gould 1991). An exaptation is a function an organism *O* can play actually but that was not selected for that specific function. The classic case, I believe, is that of feathers. Feathers are traits that originated in birds ancestral before wings have developed. They were initially used to maintain the body's temperature of those animals. However, it is now known that nowadays feathers play another different function in birds (aside from temperature maintenance): they are helpful in the flying

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[9] Consider our discussion in section I. For an uninformed person, it would be at least weird to use forks as bookmarkers. But, as long as he or she gets to know my reasons (which we might consider as the context of selection), e. g. not liking the traditional bookmarkers, it might be reasonable to see the fork *functioning as* a bookmarker. Similarly, it is senseless to talk about fitness without mentioning the selective history. Millikan (2002) expresses this point plainly: "The notion that a trait can increase the fitness of an animal makes sense only in the context of natural selection where there are determinate traits that are selected against. Then there are determinate traits for the selected trait to be *more fit than.*" (Millikan 2002, p. 43)

process, more specifically, they help the bird to control flight. Faced with this, one might ask: is helping in the flying process a function of feathers or not?

Note that exaptations present a serious challenge to strong etiological theories, since many traits exhibit functions that started to be beneficial very recently and thus they might not have been subject to selection. On the other hand, exaptations seem to fit well within Cummins' functional analysis framework. For it does not matter whether feathers have evolved this way or that way, what really matters is the actual function they play. That being said, Cummins' functional analysis would not leave out cases like exaptations.

At first glance, the weak theory appears to accommodate exaptations within its framework since a Cummins' analysis of fitness is employed. However, as we have seen, talking of fitness without mentioning background conditions is as senseless as using a forks as bookmarkers without any further explanation.

Aside from these considerations, though, there is another problem with the weak theory when dealing with cases such as exaptations. Consider the case of noses and glasses. According to the etiological theory, the function of noses is not to support glasses because it was not for that function that noses were selected for. Hence, supporting glasses is a mere effect and not a function. Suppose now that a glass that significantly enhances our vision was created. It would take not much time (it is a cheap glass, so, everyone may get one) until it becomes a really helpful tool for almost all human beings in such a way that we could, just like in the case of feathers, say that the function of noses is also to support those glasses. In this case, it is important to remember that there is no variation of T since everyone may acquire the glasses.

Before going ahead, remember the weak theory's requirement that for a trait T to be functional it must: (a) contributes to the fitness of ancestors of O possessing T; (b) be hereditary; and (c) its fitness must be measured by means of Cummins' functions. Now suppose also that there is no problem with the conception of fitness in terms of Cummins' functional analysis.

In the case of these hypothetical glasses, all it is required for the weak theory to be true is that the capacity to support glasses be hereditary and that it has contributed to the fitness of its ancestors. Both (a) and (b) are satisfied by this case, since these glasses contribute to the fitness of human population (say, it allows us to detect virus contaminated superficies) and supporting glasses is also a hereditary trait since every human being has a nose which can do that. Condition (c) is satisfied inasmuch as supporting glasses contributes to the overall functioning of the system.

Now imagine the next generation to this one, name generation$_2$, where all individuals use these new glasses. According to the weak theory as formulated by Buller, this is all there is be for a trait to be functional. That is, generation$_2$ has individuals that (a) have ancestors who were favored by T (noses) performing C (supporting glasses), and (b) have hereditarily acquired the capacity C. So defined, the weak theory implies that the function of the noses in generation$_2$ is in fact to support glasses.

It would not be weird if one is uncomfortable with this conclusion since one generation seems too wispy to consider a trait functional. Millikan (2002) advises that asking for an exact moment when trait acquires an etiological function is not a question that would shed theoretical lights in our discussions. However, it seems pretty clear that on the weak theory we cannot even get a degree of determination

for this question without being arbitrary. There is no matter of fact for when we should stop considering a trait a mere effect to call it a function. This indeterminacy, nevertheless, does not mean that the question is entirely arbitrary.

The strong theory, on the other hand, seems to have conceptual tools to deal with these difficulties. The fact that selection is a necessary condition for there being a functional trait gives us more specific notions of where we should look in evolutionary history to determine when a trait becomes functional. That is, we can look at points where there were variations of T, when the T frequencies increase while other variations substantially disappear, and so forth. This does not mean, though, that all indeterminacy is gone, but only that we have in fact a more precise criterion to analyze these cases.

To close this section, a brief commentary on the concept of exaptation is needed. I have presented a critique of Buller's weak theory based on the misconceptions of the role of selection in functional explanations and the definition of fitness in terms of Cummins' functions. Instead of the weak theory, I suggested that the strong theory does not face the problems presented here. I also brought into the discussion the term 'exaptation' championed by Stephen Gould and Elizabeth Vrba (1982), a term that at first seems to render functional ascriptions as incompatible with etiological theories. Having this in mind, in the next section I will present a more precise characterization of a strong etiological theory by discussing one particular development of the theory and I will try to cast doubt on the concept of exaptations, pointing out to a different way to look for the cases this concept seeks to describe.

IV. IN DEFENSE OF THE STRONG THEORY

After pointing out problems with the weak theory and explicating the importance of selection in functional explanations, I have suggested that the strong theory is to be preferred over the weak. However, no systematic or a near systematic treatment of the strong theory was given. This is what this section is for. Briefly, I will discuss the general theory of functions developed by Ruth Millikan in her *Language, Thought and Other Biological Categories* (1984) to draw out the framework we need to face the problems posed in the end of the next section. I start by discussing some fundamental notions developed by Millikan and then I present her theory of proper functions as a viable alternative to the weak etiological theory.

REPRODUCTIVELY ESTABLISHED FAMILIES

One important concept of Millikan's definition of functions is that of *reproduction*. According to Millikan, an individual B is a reproduction of A iff:

(a) B has some properties (p_1, p_2, p_3, …, p_n) in common with A;

(b) The fact that A and B have common properties must be explainable by natural law or law in situ; and

(c) For each property p_1, p_2, p_3, …, p_n, the laws in situ which explain why B is a reproduction of A regarding p must be laws that correlate a number of *determinates* under a *determinable* under which p falls in such a way that any determinate that characterizes A must also characterize B, being the direction of causality straight from A to B. (Millikan 1984, pp. 19-23)

An example might make things clear. Consider the case of a copying machine (Millikan 1984, p. 20). We turn it on to copy same random text of only one page. Suppose that, for some strange reasons,

1/3 of this text is printed in red, 1/3 in green and the other 1/3 in blue. Seeing that the text is multicolored and given that we want our copy to be as reliable as possible, we set the machine to do a colored copy.

When the copying process is going on, there are elementary laws of physics in action on the inner mechanisms of the machine (electromagnetism laws, for instance). Advancing our description a couple levels above of that of physics we could describe these physical processes in a more familiar way (in terms of what the machine is designed to do, for instance). We could, for example, say that the function (Cummins' sense) of a determinate part of the machinery is to recognize letters tokens and their colors. For the sake of simplicity, the recognizing part would do something of the sort: "If input color of token x equals color y (where x equals letter token recognized and y equals the color of the token recognized), then print token x_1 with color y_1 (where x_1 and y_1 equals x and y respectively)". This last conditional describes underlying physical processes responsible for copying the red token of 'a' in a simpler way, establishing then a relation of causal regularity between the document to be copied (A) and the copied document (B). This is a natural law in situ.

Given these considerations, we might now see if the copied document satisfies the three conditions above. Given that B was a perfect copy of A, it instantly follows from this that (a) is satisfied. Also, considering that the machine work in a similar way as described above, we have a law in situ which explains why B has properties p_1, p_2, p_3, …, p_n in common with A. Therefore, (b) is satisfied. But what about (c)? What is a determinate and what is a determinable?

Millikan (1984) defines these terms as follows:

> A property is "determinate" relative to some "determinable" property under which both it and a set of properties contrary to it fall. Thus *red* (along with its contraries *green, yellow,* etc.) is a determinate property relative to *colored; scarlet* is a determinate property relative either to *red* or to *colored.* (Millikan 1984, pp. 20-21, her emphasis)

Having (c) in mind, we could say that in our case, p_1 is red, p_2 is green and p_3 is blue. In this context, (c) is also satisfied, given that the natural law in situ described above correlates three determinates (red, green and blue) subsumed under one determinable (color) in such a way that if we change the red part of the text to yellow, the correspondent copy of this document would equally change.

This special way of characterizing the term 'reproduction' leads us to one key notion in Millikan's theory of function: the notion of *reproductively established families*. Millikan (1984) names the properties p_1, p_2, p_3, …, p_n above as "reproductively established properties". Hence, if *B* is a reproduction of *A* in the sense that it has the same or similar reproductively established properties, it follows that *B* belongs to the same reproductively established family of A (Millikan 1984, p. 23).

It is important, though, to distinguish two ways in which a thing may be a reproduction of another thing. Millikan (1984) talks of *first-order reproductively established families* and *higher-order reproductively establish families*. Devices belonging to first-order reproductively established families are those that are direct copies of a unique or various similar descendants. For example, when a parrot says 'hello' because he has heard someone speaking so, he is actually copying the token of the word 'hello' emitted by this person. In this sense, the reproduction of the token 'hello' uttered by the parrot is a *direct* copy of the token 'hello' uttered by some English speaker.

Contrast the case of the parrot with the case of biological devices produced by natural selection. Most of human beings hearts are indeed very similar, but they are not a copy of each other. For example, if John develops a certain heart disease (which is not a genetic disease), it does not follow that his children will necessarily develop this same disease. This happens because John's children do not have hearts which are copies of John's heart. His children's hearts are instead a result of the subsequent development of the transmitted genes under determinate conditions[10], being these genes the direct copies of one another, but not of hearts themselves. Thus, if John's disease has nothing to do with his genes, it follows that genetically speaking his children will not be born with his disease.

One last important point of Millikan's theory is that it accommodates cases where there is malfunction of members of a reproductively established family. This, we shall remember, is one important characteristic of an etiological theory of function. Consider this further formulation added by Millikan (1984):

> If anything x (a) has been produced by a device a direct proper function of which is to produce a member or members of a higher-order reproductively established family R, and (b) is in some respects

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[10] Millikan (1984) has a technical term to describe these conditions: 'Normal conditions' (with capital 'N'). Roughly, Normal conditions are conditions in which a device *historically* realizes its function properly. Consider: "A Normal explanation is an explanation of how a particular reproductively established family has historically performed a particular proper function" (Millikan 1984, p. 33). By the same token, Normal conditions are those conditions in which a reproductively established has historically performed a particular proper function. It is important to remember that 'Normal conditions' is not understood as 'average conditions', that is the motive of the capital 'N'. A trait T may, in most of the cases, perform a function F in conditions C but yet these conditions C may not be the Normal conditions N for which T was *selected* for to realize F (Millikan 1984, p. 33).

> like Normal members of *R* because (c) it has been produced in accordance with an explanation that approximates in some (undefined) degree to a Normal explanation for production of members of *R,* then *x* is a member of *R*. (Millikan 1984, p. 25)

That is, if *x* is a reproduction of items of a reproductively established family *R*, but *x* malfunctions in some respect (say, the heart does not circulate blood for some other reason rather than genetic problems) and if *x* is similar to members of *R* and was produced under approximate Normal conditions in which members of *R* are produced, then *x* is a member of *R* even though it does not realize the proper function assigned to members of *R*.

PROPER FUNCTIONS, ADAPTED FUNCTIONS AND DERIVED FUNCTIONS

The specific formulation of 'reproduction' given by Millikan (1984) allows us to formulate the strong etiological theory in a more precise sense. What we have been calling etiological functions can now be understood under the label of proper function, a term dubbed by Millikan (1984).

A trait X belonging to an organism O has the proper function of Y iff:

(a) O is a member of a reproductively established family R and X is a reproductively established property of R individuals;

(b) O ancestors had X and X realized Y;

(c) Y causally contributed to O's survival among other organisms with variations of X or organisms who did not have X;

(d) X was selected for Y; and

(e) Y is a consequence of X being there.

It is clear from this formulation that proper functions correspond to the etiological theory we outlined above. At this point, though, it might be reasonable to question what makes Millikan's theory special regarding other etiological theories since proper functions are actually only etiological functions[11].

After defining proper functions, Millikan (1984) further discusses two distinctions that can be made among these, that is, adapted proper functions and derived proper functions. An example will make things clear. Consider the case of chameleons' capacity to change its skin color according to the surrounding environment[12]. It is a direct proper function of the chameleons' skin cells to change color according to the surrounding environment in order to mislead predators. In other words, that is what those cells were selected to do.

Now consider a particular case in which a chameleon sits on something green and brown. Giving that the chameleon's cells responsible for color change are working properly, the chameleon will eventually change its skin to green and brown. But, as one may rightly point out, it is not the function of these skin cells to make the skin color turn to green and brown, since it might be the case that no other chameleon has ever sat on something green and brown. Thus, changing skin color to green and brown is not a direct proper function of the skin cells in the sense that it was not for this particular capacity that

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[11] Millikan's (1984) main concern when presenting her theory of functions is with intentionality, but the framework developed by her pretty much applies to any usage of functions.
[12] This example is directly drawn from Millikan (1984, pp. 39-41).

they were selected for. Rather, to say that the function of the skin cells is to change skin color to green and brown is to say that the skin cells have a proper function adapted to a given context. Hence, albeit changing skin's color to green and brown may have never happened before, it is still a proper function of these cells to do so. This kind of functional ascriptions relates to, according to Millikan's (1984) definition, an adapted proper function of these devices.

At this point, however, it might seem contradictory to say that such a novel relation (turning skin to green and brown) be in fact a proper function since it is clear that it was not selected to realize it in particular, that is, it was not selected for changing skin color to green and brown.

This is only an apparent problem, though. These functional ascriptions are genuine proper functional ascription because they realize the abstract character of the functional selected effect related to the reproductively established family R of which these skin cells are members. That is, the direct proper function of chameleon's skin pattern is to enable the chameleon to change its skin color whatever be the color of the environment in which it is inserted. The direct proper function makes no reference to particular cases, it only establishes the abstract relation between individual and environment that must be respected when these cells function properly.

As we have seen, adapted proper functions respect such abstract relations in a given context and that is why they are called adapted. But, at the same time, they are not direct proper functions; they are, on the other hand, only derived from the capacity the token of those skin cells have as belonging to a reproductively established family R which members have direct proper functions to change-color-according-to-surrounding-environment. Hence, the function those skin

cells have to change skin's color to green and brown is a derived proper function from the direct proper function played by the members of the reproductively established family of which the particular chameleon in question is a member.

EXAPTATIONS, PROPER FUNCTIONS AND THE IMPORTANCE OF CUMMINS' FUNCTIONS

Millikan (1999, 2002) has offered an account of the controversial case of exaptations raised in the last section based on her theory of proper functions. In this part, I will suggest, along with Millikan (2002), that what is labeled as exaptations do not offer difficulties for the strong etiological theory. This will depend intimately on the conceptual tools offered by Millikan (1984) that we analyzed on the previous parts of this section.

Exaptations as we have presented are taken to be the functional role some trait T plays that was not selected for realizing this function. Consider the case of the function of dog's mouth[13]. The fact that dogs can carry newspaper to their owners by using their mouth is clearly not a function mouths were selected for, since dogs' mouth were not selected for that function. Consider now that most of the dogs in the world are in fact trained to pick up newspapers and bring them to their owners. By a Cummins' analysis, we would be able to ascribe the function 'to carry newspaper' to dogs' mouth without caring about selective history, since it is pretty clear that their mouths, at least actually, serve that function. Consequently, dogs' mouth would have a function that would not have been selected for and thereby it would not be an adaptation. Carrying newspaper, as long as we consider Cummins' analysis, is thus an exaptation.

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[13] Millikan (2002, p. 39).

These difficulties offered to the strong theory, as one might have noted, can be addressed properly having Millikan's theory of function in hand. The case of the dogs' mouth is very similar to the chameleon's skin cells. We could say that one of the proper functions of dogs' mouth is to make dogs' stand in an abstract relation to its environment, that is, the relation dog-carrying-object. Thus, carrying newspapers is just a derived proper function from the direct proper functions mouths have that are supposed to produce an abstract relation between dogs and environment, that is, the relation of dog-carrying-object. At the same time, carrying newspapers is also an adapted proper function given that it is just a function in a specific context, that is, in places where there are newspapers.

One important point to note here is that derived proper functions also specify an abstract relation between dogs and environment, that is, dog-carrying-determinate-object-with-this-and-that-characteristics. Consequently, we could have a new adapted proper functions (and hence a derived derived proper functions) that derivatively reproduce that abstract relation expressed by the derived proper function of which it is a derivation. This derivative relation could go further on until dogs' mouth are seen as realizing extravagant tasks that are not even close to the direct proper function of mouths. The function of dogs' mouths on these cases would seem so new and different from that of which they were selected for that we would be encouraged to call them exaptations rather than adaptations.

This pathway should be resisted, though. There is no limitation to the length of the 'relation chain' between direct proper functions and its subsequent derived functions in such a way that it becomes a problem for the evolutionary theorist to drawn this chain. However, it should be clear that this problem is an epistemological problem rather than an ontological one. The fact that we cannot draw this chain does

not imply that ontologically speaking this chain does not exist. In fact, we may never be able to draw such a complete chain regarding the use of noses to support glasses or of our capacity to read, for instance. But, again, this is an epistemological problem that has nothing to do with the nature of the explanation we are advocating. So, the advantages of such analysis should not be resisted merely on the grounds that it is counterintuitive to think that way.

It follows from these considerations that the concept of exaptation is not after all a useful one, since Millikan's important classification of proper functions gives us a framework to think of the most recent behaviors as legitimate adaptations once they are conformed with direct proper functions of traits T[14].

Our considerations of Millikan's theory of functions indicates that the ambiguity in the notion of function is just an apparent ambiguity and not a matter of ontological difference, that is, there are not two excluding concepts of functions in the market. If our analysis on the role of selection is right and if Millikan's theory of functions is on the right track, then what was thought to be Cummins' functions are after all only proper functions (derived and adapted ones). So, if our analysis is correct, then it is the case that there are not two concepts of functions to fight against each other.

One might be wondering what could be said of cases such as that of swampman (Davidson 1987). If those are real possibilities, it would follow from our considerations that they do not have functions

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[14] Millikan (2002) talks of a possible use of Cummins' functions and the concept of exaptation. I will not address this question here, but I believe Millikan's use of the term is very restricted and those cases should not be considered functional in light of the strong theory we have analyzed here. See Millikan (2002), section 11.

since they were not subject of a selective process. Cases of swampman were originally designed to clarify some points in philosophy of mind. Functionalists views of mind have that a mental state can be defined by its causal-role function in both its relation to input-output transitions and to other relevant mental states. The upshot of swampman cases is that a situation where a lightning bolt strikes a tree in a swamp and originates a physical replica of, say, Barack Obama, is perfectly conceivable. By the functionalist view of the mind, we would have to say the Swamp Barack Obama is conscious, since he is functionally identical to the real Barack Obama. Now suppose that besides giving origin to Swamp Barack Obama, this lightning bolt also generates a very rare cosmic accident such that the real Barack Obama is replaced by the Swamp Barack Obama right after the lightning bolt strikes the tree. If functionalism is true, then the Swamp Barack Obama would continue to act just like the real Barack Obama. That, according to Davidson (1987), is very implausible, for the Swampkind just introduced lacks the causal history of the real Barack Obama.

Well, how this relates to our discussion about the strong theory? As Davidson (1987) has pointed out, the absence of the causal history in the Swampkind precludes it from having a mind, for it lacks the evolutionary background that is associated with the real Barack Obama, a legitimate member of the human species and hence a part of the evolutionary history of that species. Now, if we are inclined to hold that the function of biological organs are defined by their etiological functions, then we would have to deny that the Swamp Barack Obama, although having a physically identical heart to the real Barack Obama, does have a heart. The reason is that the biological kind 'hearts' are individuated because of their evolutionary history. Hence, if the Swamp Barack Obama does not have an evolutionary history, then it follows that it also does not have a heart (and neither a kidney, a stomach, etc.),

although it has a physical organ that physically resembles a human heart.

These considerations would allegedly lead to very counterintuitive consequences of the strong theory. I agree that things just so formulated sketch a very weird scenario, but I think that we should be wary of drawing extreme conclusions from cases like Swampman. The reason why I believe this is that the possibility of Swampman are so counterintuitive as the conclusions that it supports. We may reasonably ask to what extent such a cosmic accident is possible, and if so, whether a physical replica of a human being like Barack Obama would act exactly like the real Barack Obama. We have strong reasons to believe that human adult minds are a result a very complex development process throughout an individual's life. It is a well-known fact, for example, that individuals that for some reason did not develop language skills in earlier development stages have extremely limited capacities to acquire those skills after that specific period. So, without this developmental history, which stems from a very complex evolutionary background, it seems very unlikely that the Swamp Barack Obama would be capable of acting in the same way as the real Barack Obama. By the same considerations, one should be wary of calling a Swamp heart a real heart, for it seems very unlikely to suppose that a brand new heart originating from a lightning bolt strike would function just like a real heart that has been subject to a very complex developmental process and inherits a similarly complex (or perhaps even more complex) evolutionary history.

Another reason why we should resist Swampman cases is because it introduces a rather mysterious element into our general conception of the world. Assuming that such a Swampman case is possible merely as a contingent event would be similar to assume that biological species were created by the will of some deity. Inasmuch as

we believe that something is a direct product of the will of some volitional agent, there is not any other explanation available to the origin of that entity rather than that it is a product of a volitional act, which is, by its turn, a contingent act. So, if we assume that Swampman cases are possible merely on a contingent basis, then the introduction of a mysterious element in the causal chain of the world is lurking around. However, if we assume that there is a causal explanation for that fact (as we do when considering evolutionary explanations about the origin of species), perhaps one that involves considerations about theoretical relationships that are unimaginable for us today, then the Swampman has, in fact, a causal history, and this causal history is quite different from the one that biological beings with minds and hearts have. From this point, I think it is safe to say that we should be at least suspicious to hold with much confidence that Swampman have the same ontological status as human beings.

Those considerations are, indeed, not conclusive, but so isn't the case Swampman makes against the strong theory. Either way we are in danger of making a slippery move from what is nomologically possible to what is only logically possible. The transition from the latter case to the former case is a matter of controversy and I cannot address it here, but what should be clear is that Swampman cases alone are no strong reason to harm the strong theory.

Same considerations could be made of Gould and Lewontin's (1979) spandrels. These do not have functions since they were not selected for anything, they are mere consequences of some other things being there. To say that a biological spandrel has a function is only to say that it causally works in a regular way given determinate conditions, but this is not enough to say that it has genuine functions in the strong etiological sense, in spite of all the counterintuitiveness of this claim. It would have a function only if it was a result of a selective process or if

it somehow played a proper function derived from other biological devices of which it is a consequence.

Finally, if our analysis is right, it would follow from it that (a) selection plays a central role in functional explanations, which makes (b) the relevant cases of Cummins' function only proper functions, and hence (c) there is only an apparent ambiguity in the notion of function.

FUNCTIONAL ANALYSIS

One unclear point about the controversy between strong etiological theories and exaptations is the following: is the proposal presented here committed to what some philosophers have called conceptual analysis? That is, do our conclusions here indicate that we should find a consensual use to the word 'function'?

I believe that functional analysis is implied when one takes the importance of selection for functional explanations. In fact, the conclusion offered here contrasts that of those philosophers who hold that there cannot be a unified account of functions. To understand this, consider the closing passage of Godfrey-Smith's 1993 paper:

> Lastly, it might be asked: on my view, what reason is there to use the word "function" for both Wright and Cummins functions? What do the concepts have in common that justifies this usage? My reply is: there is no strong reason for using the same word. Both types of function are "explanatorily important properties of components of systems," but this is a very broad category. I doubt if linguistic reform is possible here, as both types of functional ascription are deeply embedded in biological usage. At least let philosophers do the right thing, when we analyze functional characterization: let no philosopher join what science has put asunder. (Godfrey-Smith 1993)

Faced with this, the question I suggest we ask is: why should not we look for a linguistic reform? I believe that this is one of the ways

the dialogue between philosophy of science and science can be fruitful. It certainly has not been so explicit that the proposal offered here is sympathetic for a linguistic reform, but I take my claims to point out for something of the sort. So, when we say that "Cummins' functions are after all only proper functions", what is implied is that we may give a functional explanation of processes heretofore taken to be paradigmatic uses of Cummins' functions (exaptations, for example) using proper function, although this might be untenable from a practical point of view[15].

We should also recognize that Cummins' functions and proper functions are not extensional. In Millikan's (2002) discussion of co-opted traits that give rise to new proper functions, it is clear that in this case a reduction of Cummins' functions to proper functions is not possible right because proper function and Cummins' functions are not extensional, so this might be a case in biology where Cummins' function play some explanatory role which is not covered by proper functions. However, as I hope to have demonstrated along this section, selection plays an essential role in functional attributions, so, those new functions would not be functions (under the definition presented here) until they are subject to selection. The point is that we could use another term to describe the causal powers of co-opted traits. So, it is reasonable to ask: if we are disposed to deny functions to a swampman because it does not have a selective history, why should we take these new traits to be functional?

Fred Dretske (1996) gives an illustrative example on this point. In Dretske's example, we are asked to imagine a case in which some object of ours simply disappears and it is replaced for a physical identical object resulting from lightning bolts striking somewhere.

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[15] See below for more on this.

Dretske speaks of a wedding ring. Imagine that your wedding ring suddenly disappears but it is replaced with a physical replica of it generated by a lightning bolt somewhere near your bedroom. Given that you see this spectacular phenomenon, Drestke argues that there is no reason why we should still continue to consider that ring as *your* wedding ring, since it is not the ring you were using in the day you got married. What makes that previous ring *your* wedding ring is the history behind it. Any physical copy of it simply would not be that ring since it does not have the same history as *your* ring does.

Another important thing to have clear on this discussion is the relation between the concept of function used in science and the use of the term in our everyday talk. That is, when it comes to the relation between the formal definition proposed here and the ordinary use of function, I think that the everyday use of functions does not need to reflect the constraints posed by the formal definition. An analogy here seems appropriate. The Portuguese word '*tempo*', which is literally translated by 'time', has an ambiguous use in the everyday talk. It can mean, on the one hand, "time" like in "What time is it?", and it can mean, on the other hand, 'weather' like in "The weather is not so good for a soccer match". Both usages have a similarity in the sense that they are somewhat related to how we describe some phenomena regarding the sun (a certain position of the sun indicates that a certain amount of time has passed or the absence of the sun and the presence of clouds might indicate that it will rain), but it would be misleading to maintain this ambiguity in a scientific context. In the case of physics, Portuguese speakers keep using the word '*tempo*', which is related to the first common usage, that is, '*tempo*' is used in a quantitative sense like in "a = $\Delta V/\Delta t$". As for the case of meteorology, Portuguese speakers have a quite new word to refer to 'weather', that is, they use the word '*clima*' instead of '*tempo*', usage which has more to do with the qualitative aspect of the word '*tempo*'.

By the same token, saying that some actual disposition of a device which does not have a selective history has a function should not be banished from our everyday talk. But I believe we must have things clearer in science. And what I expect my argumentation to make clear is that the word function must be used in science strictly for cases where there is a selective history to be told.

One objection that might be raised at this point regards the broader use that Cummins' function have in scientific practice[16]. According to this objection, one cannot just propose an elimination of Cummins' functions based merely on considerations of biological cases. Given that the causal-role conception of functions plays an essential role in other scientific domains such as psychology and artificial intelligence, we would not be allowed to conclude merely from philosophical considerations about biological cases that a linguistic reform could take place.

One thing to bear in mind at this point is that the linguistic reform I am suggesting here is a kind of minimalist reform. That is to say, in other words, that we should seek for new and quite different terms to refer to both etiological functions and Cummins' functions. By suggesting a minimalist reform of this sort, I do not mean to suggest that we should eliminate the practical dimension associated with the use of causal-role or Cummins' conception of functions. One might still use the word 'function' for causal-role functional statements, but that should be accompanied by a quite different term used to refer to etiological functions. In philosophy of mind, some have used the term 'teleofunctions' or 'teleofunctional' to refer to the kind of functional statements that are concerned with teleological considerations, and this might be taken as an attempt to differentiate both kinds of

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[16] Thanks to an anonymous referee for calling attention to this point.

explanations, since teleofunctionalist conceptions of the mind would allegedly solve problems that are posed for classical functionalist views (which are by their turn characterized in terms of Cummins' functions).

Now, since I have been arguing that Cummins' functions *are* etiological functions, a linguistic reform of this sort might suggest that I am supporting a distinction of things that are not distinct after all. As contradictory as it might seem at first glance, there is here only a seemingly tension. Let me explain. In fact, the linguistic reform is not intended to support an ontological distinction, but rather to preserve the practical dimension of the use of Cummins' functions and yet to recognize the reductive relation between these two kinds. As I have argued, the reduction of Cummins' functions to etiological functions depend on a fundamental distinction between direct proper functions and derived proper functions. Moreover, as the case of the dog that carries the newspaper shows, one might be inclined to support an ontological distinction between functional statements, since a long chain of derived proper functions may, at some point, seem to detach from its original direct proper function and originate a new function. This, as we have seen, is not due to real ontological differences, but rather to our limited epistemological capacities that conceal us from seeing this long causal or relational chain. It is indeed true that if one had to formulate functional explanations by taking into account this long causal or relational chain in every single situation, then functional statements in science would most likely be rendered impossible. But that is not, I believe, a strong reason for concealing us from drawing the ontological reduction I am proposing here, since the minimalist linguistic reform advocated here is intended to avoid such difficulties.

Also, it is worth noting that this ontological reduction followed by a linguistic reform might be important in some theoretical cases. This is best viewed in instrumentalist conceptions of intentionality in

philosophy of mind. One might hold that intentional capacities are a result from some sort of social skill we have learned through our evolutionary history and that intentional states, although highly valuable for social interaction, are not ontologically distinct from brain states[17]. In this sense, one may preserve intentional terms (such as belief, desires, etc.) and yet hold that they are nothing else but brain states. The reason for supporting this line of reasoning is that implementing the language of neuroscience in our everyday life would almost likely turn our life more complicated and would possibly render many of our everyday practices impossible. A linguistic distinction, in this case, proves to be useful both for practical reasons and cognitive reasons, since one may preserve the practical aspects of the intentional language and still recognize that they are ultimately brain states. By the same token, applying a clear-cut linguistic distinction between both kinds of functional explanations allow us to preserve their practical dimension without suggesting that there are two ontologically distinguished kinds of function on the market.

To conclude, I believe that my conclusion is pretty similar to Godfrey-Smith's (1993), that is, I do not see any strong reason to use the word 'function' for both Cummins and proper functions, but I disagree with him when he says that philosophers should not attempt to join both usages only because it is deeply entrenched in the scientific practice. For I believe science may have some conceptual definitions that are sometimes misleading, and in my view that is exactly where the work of philosophers is important. So, if what I propose is coherent, there should no more be an ambiguity of the notion 'function' within science.

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[17] See, e.g., Dennett (1987).

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