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FUNCTION IN BIOLOGY: ETIOLOGICAL AND ORGANIZATIONAL PERSPECTIVES

Función en Biología: perspectivas etiológicas y organizacionales

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ABSTRACT

In this paper, we argue for a taxonomy of approaches to function based on different epistemological perspectives assumed with regard to the treatment of this central concept in the life sciences. We distinguish between etiological and organizational perspectives on function, analyzing distinct theories: Wright's selectionist etiological approach and Godfrey-Smith's modern history theory of functions, in the case of the etiological perspective; and Cummins' functional analysis and Collier's interactivist approach to function, among organizational accounts. We explain differences and similarities between these theories and the broader perspectives on function, arguing for a particular way of understanding the consensus without unity in debates about function. While explaining the accounts of function, we also deal with the relationship between this concept and other important biological concepts, such as adaptation, selection, complexity, and autonomy. We also advance an argument for the limits and prospects of the explanatory role of function in evolution. By arguing that changes in functionality are always grounded on changes in systems' organization, we show that function can never explain the origins of traits. Nevertheless, it can explain the spread of traits in populations, but only when we are dealing with functionally novel traits. Finally, we stress that organizational accounts of function are needed to understand how new functions appear by means of changes in systems' organization.

Key words: etiology, explanation, function, organization, teleology

RESUMEN

En este artículo, argumentamos a favor de una taxonomía de abordajes del concepto función basada en diferentes perspectivas epistemológicas de acuerdo al tratamiento de este concepto central en las ciencias de la vida. Distinguimos entre perspectivas etiológicas y organizacionales sobre la noción de función, analizando teorías distintas: la teoría etiológica seleccionista de Wright y la teoría de la historia moderna de Godfrey-Smith, en el caso de la perspectiva etiológica; y el análisis funcional de Cummins y el abordaje interactivista de Collier, entre los abordajes organizacionales. Explicamos las diferencias y similitudes entre estas teorías y entre las perspectivas más amplias sobre función, argumentando a favor de una manera particular de comprender el consenso sin unidad en los debates sobre función. Al explicar los abordajes sobre función, examinamos también las relaciones entre este concepto y otros conceptos biológicos importantes, como adaptación, selección, complejidad y autonomía. Además presentamos un argumento sobre los límites y las posibilidades del papel explicativo de función en evolución. Al argumentar que los cambios en funcionalidad están siempre basados en cambios en la organización de los sistemas, mostramos que la función jamás pode explicar los origines de rasgos. Sin embargo, ella puede explicar el aumento de frecuencia (*spread*) de rasgos en poblaciones, solo cuando estamos considerando rasgos funcionalmente nuevos. Finalmente, destacamos que abordajes organizacionales de la función son necesarios para comprender como nuevas funciones aparecen por medio de cambios en la organización de sistemas.

Palabras clave: etiología, explicación, función, organización, teleología.

CONSENSUS WITHOUT UNITY

Since Larry Wright's seminal work, "Functions" (1973), etiological approaches have played a distinguished role in discussions about this central biological concept. Another important and highly influential work was Robert Cummins' "Functional analysis", written in 1968, but only published, without amendments, in 1975 (Ariew *et al*., 2002, p. 417-418).[1] Due to the widespread influence of these works, many philosophers of biology consider Wright's and Cummins' approaches as the two main poles in the debates about function in recent philosophy of biology (Salmon, 1990; Godfrey-Smith, 1993; Hull, 2002; Chediak, 2006).

Nevertheless, several other philosophers and scientists also tackled functional ascriptions/ explanations in biology. While it is true that many, if not most of them took Wright's and Cummins' works as grounds for developing their own analyses, it is time to consider, in our view, that the best taxonomy of approaches to function should not be built by appealing to the authors of those influent papers, but, rather, to the epistemological perspectives assumed with regard to the treatment of the concept. Our main goal in this paper is to argue for this position, by distinguishing between etiological and organizational perspectives on function.

To be fair, one cannot lose from sight that Wright's and Cummins' works contributed to make the concept of function more important and even central in the debates about explanation in biology. But many authors, such as Millikan, 1998 [1989], Neander, 1998 [1991], Bigelow and Pargetter, 1998 [1987], Godfrey-Smith, 1998 [1994], and Collier (Collier, 2000; Collier, 2004a) among others, developed analyses of function that dealt with a diversity of problems related to this concept, contributing to either broadening or limiting the scope of functional ascriptions /explanations. Even though they have built upon Wright's and Cummins' works, the similarities between their approaches come to a significant extent from sharing a particular perspective on function, either etiological or organizational. Thus, despite the influence of those founding works, it is on their shared perspectives that we should find the criteria for a taxonomy of approaches to function. It is the case, however, that more diversity is found among organizational than among etiological approaches to functions, since the latter have been largely built as modifications of Wright's account.

Etiological and organizational approaches to function lead to distinct conceptions about the nature and goals of functional explanations. Some philosophers attempted, however, to unify these approaches in a single theoretical structure. The most well-known of these proposals is

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[1] Cummins also presents his account of function in the first two chapters of his book The Nature of the Psychological Explanation (Cummins, 1983).

Kitcher's (Kitcher, 1998, [1993]). As a response to Kitcher, Godfrey-Smith, 1993, built his thesis of a consensus without unity about function.

Kitcher argues that a unity can be found in approaches to function by means of an analysis of both functional ascriptions throughout the history of biology and current uses of function in biological and non biological contexts. This purported unity would be found "in the notion that the function of an entity S is what S is designed to do" (Kitcher, 1998 [1993], p. 479, Emphases in the original). The synthesis proposed by Kitcher would be accomplished through a combination of Wright's and Cummins' approaches linked to each other by the concept of project or design.[2] Kitcher's claim has a normative nature: uses of function should be connected with a source of design, being the two main sources human intentions and natural selection.

Godfrey-Smith, 1993, argues that Kitcher's proposal can only lead to a "false unity". His consensus without unity amounts to the recognition that there are two central approaches about function, each with its own domain of application. In his view, these approaches cannot (and *ipso facto* should not) be reconciled in any single, monolithic view, supposedly applicable to each and every biological phenomenon. He grounds his confidence in the unfeasibility of this synthesis on the idea that these two theoretical accounts look at the natural world from different perspectives, drawing attention to some aspects while naturally neglecting others.

While Wright emphasizes the historical nature of the living world, associating it with the origins of traits, taken to be explained by their functions, Cummins focuses on the complexity of systems and the relationship between systemic capacities and capacities of a system's parts. Cummins and Wright situate functional ascription within two distinct explanatory modes, both of which are legitimate parts of our current scientific perspective. We accept the existence of natural and artificial selection, and the explanatory power of selective histories as a ground to understand the attributes of many things. And we also accept the existence of complexly organized systems and the explanatory power of the analysis of their global capacities in terms of the capacities of their parts. We are dealing with two legitimate and different scientific explanatory modes when discussing Wright's and Cummins', or, as we prefer, etiological and organizational approaches. Godfrey-Smith convincingly argues that there is no single explanatory project that might encompass these two modes. Or at least, in a somewhat weaker argument, we should resist such attempt of unification in the interests of maintaining an accurate understanding of these distinct scientific explanatory strategies.[3]

In order to support this pluralist thesis, we can also appeal to Mayr's (Mayr, 1982; Mayr, 1988) distinction between evolutionary and functional biology, or Jacob's (Jacob, 1983)

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[2] Kitcher's appeal to the concept of design has no connection with the idea of a supernatural or, for that matter, natural designer. As he himself clarifies: "design is not always to be understood [...] in terms of background intentions, however; one of Darwin's important discoveries is that we can think of design without a designer" (Kitcher, 1998 [1993], p. 480). However, the priority given to the concept of design by Kitcher, but also by other authors such as Dawkins, 1996, and Dennett, 1995, may be misleading. As Godfrey-Smith (Godfrey-Smith, 1999; Godfrey-Smith, 2001) suggests, this emphasis on design may follow not from biological evidence, but from an intellectual project of defending a secular worldview. Thus, it may lead us away from theoretical and empirical problems about evolution towards a metaphysical discussion, entangling ourselves in debates about design and the role of a supposed designer, which may be interesting for some people, such as intelligent designer advocates, but not so interesting to science itself. We consider, thus, that it would be particularly consequential to deflate the importance of discussions about design in evolutionary biology.

[3] Godfrey-Smith's argument that these two approaches amount to distinct explanatory projects is in agreement with an observation made by Cummins, 2002, who maintains that the selectionist etiological accounts (such as Wright's) and his functional analysis have different explananda.

distinction between tomist or reductionist and integrationist or evolutionist biology.[4] As Caponi, 2002, emphasizes, these distinctions play an important role in the treatment of several issues raised by philosophers of biology, including the role of teleology (and, consequently, function) in the life sciences.

Organizational approaches, such as Cummins', capture well the uses of function in functional biology, which is mainly focused on explaining how living systems operate by appealing to proximate causes and mechanisms. It is through functional analysis that we typically explain global capacities of systems by appealing to capacities of their parts, interacting with each other as explained in models of mechanisms (e.g., Craver and Bechtel, 2006). In Cummins' account, these capacities of the parts are treated as their functions.

In turn, etiological accounts, such as Wright's, might grasp uses of function in evolutionary biology, which explains why living systems operate the way they do, i.e., why they show the kinds of mechanisms they show, by appealing to ultimate, evolutionary causes. Wright articulates a philosophical analysis of how we can appeal to function to explain the existence of a biological trait or cultural artifact. In the case of biological structures, he considers how ultimate or evolutionary causes –particularly, natural selection– can account for the existence of a structure in some living system based on its function. Despite the criticisms that can be raised against Wright's approach –for instance, due to its selectionist nature (see below)–, it is quite clear that it can find its adequate room in the context of evolutionary biology. And the same is arguably true of any other etiological account of function.

If we acknowledge that there are two distinct albeit interrelated fields constituting biology and, moreover, that etiological and organizational accounts of function fits better with one or the other field, the argument against an unification of these approaches will be reinforced. In these terms, we can conclude that there are (at least) two modes of functional explanation in biology. Or, as Caponi, 2002, suggests, there are two teleologies, one in each field of biology, related to functional explanation and selectionist explanation.

ETIOLOGICAL PERSPECTIVES ON FUNCTION

Wright's selectionist etiological approach. Wright, 1973, developed an explicitly causal approach to functions, framed in terms of what he called a "consequenceetiology". Even though he proposed the first formal model of the etiological account, we can say that this approach was implicit in much of the selectionist thought of the 1960s and 1970s.

For Wright, previous analyses of functional ascriptions failed because they did not capture the genuinely explanatory power of functional ascriptions:

> Merely saying of something, X, that it has a certain function, is to offer an important kind of explanation of X. The failure to consider this, or at least take it seriously, is, I think, responsible for the systematic failure of these analyses to provide an accurate account of functions (Wright, 1973, p. 154).

In his view, this failure was due to the fact that those philosophical analyses did not propose any distinction between function and accident. This distinction plays an important role not only

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[4] We do not think that functional biology can be indeed (explanatorily or methodologically) reductionist, to the extent that the spatiotemporal organization of the components' relationships within complexly organized systems should be taken into account to explain how living systems operate (El-Hani and Emmeche, 2000). But this is not the space to pursue this argument further. Below, we will preferentially use Mayr's terminology.

in the elucidation of function, but also in scientific practice, as shown by works proposing a distinction between functional and accidental traits in evolutionary biology (e.g., Williams, 1996 [1966]; Gould and Vrba, 1982).

Wright stresses that functional ascriptions provide explanations, just as goal ascriptions do. He insists, furthermore, that functional ascriptions should explain in a strong sense, since to use a weaker interpretation of the meaning of function is to run afoul of the function-accident distinction. The fundamental nature of this distinction becomes clear when we consider a question such as "what is liver good for?" –qualified by Wright as an "anemic" question–, which cannot be translated into the stronger question "Why do animals have livers?" (Wright, 1973, p. 155). Notice that the latter question requires an explanation of the existence of a certain state of affairs in a particular, circumscribed domain. This explanation should count as the ascription of a function to the liver, while the first question admits many different answers, which do not depend on any distinction between function and accident to be acceptable. Some of these answers have nothing to do with the function of the liver, but are related, rather, to utilities of the item that are accidents from an evolutionary point of view. From a historical perspective, it is an accident, for instance, that livers are good to eat with onions. Nevertheless, this is not an absurd answer. To differentiate functions from accidents, we need to consider that the fact that livers are good to eat with onions does not illuminate, in any sense, the etiology of livers. The notion of utility (present in expressions like 'it serves for' or 'it is useful for' or 'it is good for') is not adequate to deal with the uses of function in strictly biological, non intentional systems, even though it plays a role in the explanation of intentional behavior. Often, the utility of some biological item cannot account for its etiology precisely because utility is a human-oriented notion, and, thus, cannot be the reason that explains why some living systems evolved so as to exhibit some biological items, say, why animals have livers.

From Wright's point of view, functional ascriptions/explanations should be etiological, in the sense that they are related to the causal background that gave origin to the trait or behavior under consideration. The fact that it is an accident that livers are good to eat with onions is what tells us that this is not a function of the liver in the intended sense. Since functional ascriptions/explanations should concern the causal scenario which gave origin to the trait or behavior at stake, they are treated by Wright as causal explanations in an extended sense. In order to show what makes them different from ordinary causal explanations, he appeals once more to the function-accident distinction. For him, all counter examples to a functional ascription that are based on accidents can be avoided "if we include as part of the analysis something about how X came to be there (wherever): namely, that it is there because it does Z – with an etiological 'because'." (*ibid.*, p. 156). This leads to the first clause in his well-known formula to capture the explanatory power of functional ascriptions: X is there because it does Z.

Wright emphasizes, however, that this first clause, which shows the etiological character of functional ascriptions, is not enough. After all, the causal/functional distinction is a distinction between etiologies. Thus, it is necessary to introduce a clause distinguishing functional from non functional etiologies, in order to save the genuinely explanatory power of functional ascriptions. Wright's second clause states that the consequence of doing Z in the past is the very reason for the occurrence of Z in the present. It is the very nature of the etiology, thus, that entails the existence of etiologies that are specifically functional. When we say that the function of X is to do Z, we are saying that X is there because it does Z. Moreover, we are explaining how X got there, and it is this feature that qualifies the explanation as etiological.

We can now formulate Wright's account in full: "The function of X is Z means that (a) X is there because it does Z, (b) Z is a consequence (or result) of X's being there." (*Ibid*., p. 161, emphasis in the original).

Wright holds that his analysis is highly recommended because it elucidates the concept of natural selection, even though it is not limited to this particular kind of selection. Indeed, Wright's approach is strongly selectionist, something that is not really surprising, since he built it by taking as a starting point an adaptationist understanding of the Darwinist explanation of evolution, which was highly accepted in the 1970s. The selective advantage of showing a given behavior or trait in the past, related to the realization of its function, is, at least in part, a cause of the instantiation of the behavior or trait by current organisms of a lineage. Therefore, the function –in this account– is the very reason, the raison d'etre of the behavior or trait.

This approach to function offers us a short and elegant formula to account for functional ascriptions/explanations in evolutionary biology, in contrast with approaches developed by previous philosophers. Another feature that makes this approach quite appealing is the fact that it provides a helpful distinction between function and accident. This distinction is an advance in philosophy of biology that cannot be neglected. Sometimes, critical appraisals that show –correctly– some flaws in Wright's approach become so fierce that they end up losing from sight the importance of the function-accident distinction.

Even though Wright's account clarified some important issues in the debates about function, this does not mean that there are no problems with it. Later etiological theories were developed, at least in part, to address problems in the original account.

Godfrey-Smith's modern history theory of functions. Godfrey-Smith, 1998 [1994], developed a selectionist etiological theory about functions, in the same line of Wright's, but trying to overcome objections raised against this philosopher's account. Although he does not overcome all the difficulties faced by this account (Nunes-Neto and El-Hani, 2009), he noticed an important aspect of the evolution of traits that was neglected by Wright. He saw a putative flaw in the lack of a clear indication of how far we have to go in the past to reconstruct an etiology of a current organismal trait, whose existence we want to explain. In order to face this problem, Godfrey-Smith restricted the search for the etiology to the recent past in the evolutionary history of a given trait.

In order to illustrate Godfrey-Smith's theory, we take the case of feathers, which appeared in dinosaurs, before the evolution of birds. According to the most accepted models, they were initially selected for the function of thermal isolation in the dinosaur ancestors of birds, which were not able to fly (Chiappe, 2009). However, they were subsequently coopted for flight, and this resulted in the posterior selection for changes in other traits of the feathers and the birds themselves, particularly in their skeletons and neuromotor systems. Following Gould and Vrba 1982, we can say that feathers were originally adaptations for thermal regulation in ancestral dinosaurs and were exapted for flight in birds (see Sepulveda and El-Hani, 2008). As the existence of feathers is – according to the etiological approach – explained by natural selection for thermal isolation, not only feathers are an adaptation to thermal regulation, but also we can ascribe function to them only in relation to thermal isolation, not in relation to flight. To be used as a device to fly is just an effect of feathers, since they preexisted flight and were coopted to play a role in this process later in the evolution of the lineage of birds. We are dealing here with the distinction between function and accidental effect, an important contribution of the etiological approach, as a counterpart of the distinction between adaptation and exaptation.

From Godfrey-Smith's perspective, to explain why current birds have feathers, we need to refer only to the recent, modern history, in which the reason why this item has been maintained in this lineage is related to their contribution to flight. As a consequence, we would have to exclude from the etiological explanation a historical narrative that goes deeper in the past, delving into a time in which feathers arose and were maintained in dinosaur populations because of their thermoregulatory effects. For, if we wish to understand the current presence of feathers in birds, we have to restrict the temporal gap to the recent history of feathers, addressing only the advantages of having these items that are related to their contribution to the activity of flight.[5] This perspective is at first consistent with the distinction between exaptation and adaptation proposed by Gould and Vrba, 1982, but it is not in agreement with these authors' idea that function is to be ascribed to adaptations, not exaptations.

Godfrey-Smith's proposal, however, is not fundamentally different from Wright's. Therefore, it reinforces rather than challenges the idea that Wright's approach, although not being the only etiological selectionist account of function, is the central one. As we said above, in contrast with organizational, less diversity is found among etiological approaches, because they have been mostly built as modifications of Wright's account. The same goes for other etiological approaches to function, which we will not review here for the sake of space, such as Millikan's (Millikan, 1998 [1989]), Neander's (Neander, 1998 [1991]), and Bigelow and Pargetter's (Bigelow and Pargetter, 1998 [1987]). The substantial similarity between etiological theories of function is a consequence of the fact that they are all based on the same premise (introduced by Wright himself), namely, that functional ascriptions in biology should explain the presence or existence of organismal items.

Cummins, 2002, argues that this premise is the Achilles' heel of etiological approaches. In his original 1975 paper, we already find him writing that to appeal to function to explain the presence of an item "...is an act of desperation born of thinking there is no other explanatory use of functional characterization in science" (p. 747). In his more recent paper on the issue, he argues that etiological selectionist approaches do not capture the legitimate role of functional explanations in scientific practice since biological items do not exist because of their functions, but because they are produced by developmental histories. And, even if we weaken the etiological account, trying not to explain the existence of traits anymore, but rather their spread in populations, we cannot find yet a legitimate role to functional explanation, since traits typically do not spread –Cummins argues– because of their functions, but because of the differential efficiency in the performance of a function. It is only in the cases in which functional novelties are introduced in some subpopulation that the target of selection and the target of functional ascription coincide and, then, we can appeal to function to explain the spread (but never the origins and, thus, the presence) of a trait. Nunes-Neto and El-Hani, 2009, discuss Cummins' criticism of the etiological accounts and conclude that there is an important room for them in our theories about functional explanation, only hesitantly acknowledged by Cummins, even though the domain of application of those approaches is much more restricted than Wright and his followers thought. Etiological approaches cannot explain the existence of traits. They can only explain their spread, and, even in this case, only of functionally novel traits. Nevertheless, the emergence of functional novelties plays an important role in evolutionary explanations, and, thus, etiological approaches still have a relevant place in biology and its philosophy. This also means that Godfrey-Smith's consensus-without-unity thesis is to be preserved, despite Cummins' (Cummins, 2002, p. 157) appeal that etiological accounts be extinct.

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[5] At first, it might seem that Godfrey-Smith can be said to assume the untested (indeed, untestable) claim that feathers would not be preserved in a descendant monophyletic lineage of organisms which would correspond to birds in the topology of the animal phylogenetic tree, but, hypothetically, would not be able to fly. We may envision, thus, a thought experiment which casts doubt on this assumption, if we consider that birds are, just as their dinosaur ancestors, warm-blooded animals, and, thus, feathers could be preserved in them by natural selection for efficient thermal regulation. However, Godfrey-Smith's arguments are not framed in terms of a possible-worlds structure. He is dealing with our real world, as we experience it, and, in this world, it is entirely acceptable that the reason why birds currently have feathers lies in flight-related advantages.

ORGANIZATIONAL PERSPECTIVES ON FUNCTION

Cummins' functional analysis. In this section, we will briefly examine Cummins' (Cummins, 1975) approach to function, named by him "functional analysis". First, we should notice that, for him, functional ascriptions/explanations can be offered without reference to evolutionary considerations. Differently from selectionist etiological accounts, this is a non historical approach. It addresses functional ascriptions from a systemic, organizational perspective, in terms of complex capacities and dispositions. As an example, for Cummins, if x functions as a pump in a systems, or if the function of x in a system s is to pump, then, x should be able –or specifically, it should have the disposition– to pump in s. Thus, functional-ascription statements imply dispositional statements: to ascribe a function to something is, at least in part, to ascribe a disposition to it.

For Cummins, there are two complementary strategies to explain a disposition: (i) the subsumption or instantiation strategy, and (ii) the analytical strategy. In the instantiation strategy, a particular disposition of a given object is explained through its subsumption to a lawful dispositional regularity. For instance, we can explain in this manner the disposition of a piece of metal to expand with increasing temperature. In this case, the explanation takes place by appealing to a regularity about the thermal expansion of bodies (say, the law of linear expansion), alongside with initial conditions and propositions about the object at stake, such as its coefficient of linear expansion, the temperature variation to which it was exposed, etc. In other words, the particular case is subsumed into the lawful regularity, which explains, together with the initial conditions, the instantiation of the specific disposition of the object.[6]

It is not surprising that this strategy is successfully employed in the physical and chemical sciences, while in biology it cannot be applied to all its subdisciplines or domains with the same strength. Since this is not the space to discuss the considerable literature on the nature of biological generalizations (e.g., Beatty, 1995; Brandon, 1997; Waters, 1998; Mitchell, 2003), we will only call attention to the fact that to explain by appealing to laws is far from being trivial in most domains of biology. If we use Mayr's distinction between evolutionary and functional biology, we can say that there is much more room to apply the subsumption strategy in some fields of functional biology closer to chemistry and physics, such as biochemistry and biophysics. Even in functional biology, however, explanations typically appeal to functions. In evolutionary biology, in turn, the instantiation strategy seems largely artificial, as a consequence of the narrative, historical nature of a substantial portion of evolutionary explanation.

Instead of deriving a dispositional regularity that specifies a disposition *d* (in system *a*) from the facts of the instantiation of *d* (in *a*), the analytical strategy proceeds by analyzing the disposition d present in a into a series of other dispositions, *d1, d2,* ..., d_n, exhibited by components of *a,* such that the programmed manifestations of di results in a manifestation of *d.* According to Cummins, the analytical and the instantiation strategies will fit together into a unified account of functions if the analyzing dispositions (d_i) can be made to yield to the instantiation strategy.

In his 1975 paper, after presenting the analytical strategy, Cummins proposes a shift in terminology: "when the analytical account is in the offing one is apt to speak of capacities (or abilities) rather than of dispositions" (Cummins, 1975, p. 759). This is basically an appeal to familiarity, justified by Cummins as a manner of putting a more "familiar face" on the analytical strategy, since we often explain capacities by analyzing them. He offers an example of the

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[6] This is an application of the Hempel-Oppenheim, 1948, deductive-nomological model of scientific explanation. It suffers, thus, of the same shortcomings of this model (for a review, see Salmon, 1990).

application of the strategy, taking assembly-line production as a case in point. Production in an assembly-line is broken down into a series of distinct tasks. Each point of the line is responsible for a given task, and it is the function of the components at that point to accomplish the task. If the tasks of the component parts are realized in a properly organized way, then the final product will follow as a result. This brings home the lesson intended by Cummins: the function of a component of a system is whatever it does that contributes to the realization of a given capacity of the system as a whole, more precisely, the capacity that we are trying to explain.

Cummins formalizes the analytical strategy as follows. *x* functions as a Φ in s (or: the function of *x* in s is Φ) relative to an analytic account *A* of *s*'s capacity to Ψ, just in case x is capable of Φ-ing in s and *A* appropriately and adequately accounts for s's capacity to Ψ by, in part, appealing to the capacity of x to Φ in s (*ibid*., p. 762).

This formalization makes it explicit an important feature of Cummins' approach, namely, that it is pragmatically formulated as being dependent on the research question one is trying to answer. For each systemic capacity one is attempting to account for, different decompositions of the system into component parts and different localizations of activities that count as the parts' function can be explanatorily more powerful.[7] Or, to put it differently, the same system can be modeled differently – in terms of structure and function – depending on which of their capacities we are trying to explain.

The explanatory interest of a functional analysis is roughly proportional to "(i) the extent to which the analyzing capacities are less sophisticated than the analyzed capacities, (ii) the extent to which the analyzing capacities are different in type from the analyzed capacities, and (iii) the relative sophistication of the program appealed to, i.e., the relative complexity of the organization of component parts/processes that is attributed to the system" (*ibid*., p. 764). These requirements entail that the appropriateness of the use of functional explanations is a matter of degree. The higher the difference in type and sophistication between analyzed and analyzing capacities, the more adequate will be the use of function. But when the above requirements are not fulfilled, say, when the difference in type and sophistication between analyzed and analyzing capacities is small, the instantiation strategy is more adequate.

This allows us to understand how the two explanatory strategies can be linked. We explain nomologically, i.e., by subsumption when there is no use for function. In other words, scientists often explain complex capacities of systems by analyzing them into the component parts' capacities (using the analytical strategy), until the parts' capacities are better explained through a nomological explanation (using the instantiation strategy). In a series of explanations, moving towards lower-level phenomena, a capacity that, in one explanation, is the *explanans*, and in the other is the *explanandum*, can be the linking element between the analytical and the instantiation strategies. It is interesting to notice how this approach allows us to conceive of a non reductionist explanatory integration between different fields –each focusing on some specific level of organization–, such as sociology, neurosciences, organismal physiology, biochemistry, biophysics, etc.

The fact that in Cummins' approach the appropriate use of functional explanation is seen as a matter of degree is yet another difference in relation to Wright's, who makes no distinctions whatsoever about the degree in which functional statements may be adequate. In Wright's view, there is a clear-cut distinction between what is functional and what is not. More importantly, that his account leads to a clear distinction between function and accident is widely seen as one of its positive features. For example, if you use a dictionary to adjust the height of the monitor of

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[7] About decomposition and localization, and their challenges, see Bechtel and Richardson, 1993.

your computer, it is intuitive to think that this is just an accidental use, not a function of the dictionary. Wright's account brings this difference home, by claiming that to adjust the height of the monitor is not the function of the dictionary because it is not the reason why the dictionary exists. This aspect is related to two criticisms raised against Cummins' approach, the "too liberal objection" and, precisely, the lack of a proper function/accident distinction. To continue with our example, notice that Cummins' functional analysis allows us to say, in principle, that to adjust the height of the monitor is a function of the dictionary in the described situation, since this capacity of the dictionary can be said to contribute to explain a higher-level capacity, namely, the efficient usage of the computer by the system user/computer/dictionary.

The "too liberal objection" focus on two allegedly inadequate outcomes of Cummins' theory (Wouters, 2003): (i) to regard as functions effects that are not intuitively conceived in this manner –as in our example of the dictionary, or in Millikan's (Millikan, 1998 [1989]) example of producing rain as a Cummins-function of clouds–; (ii) to ascribe functions to parts of systems that we do not think intuitively as having functions – as, for instance, when one tries to ascribe to the nose bridge the role of supporting glasses. These counterexamples do not devalue, in our view, the legitimacy of functional analysis (Nunes-Neto and El-Hani, 2009). First, consider that Cummins did not intend, from the very start, to support an all-or-nothing distinction with regard to functionality. He explicitly argues that functional explanations will be more or less appropriate, depending on features of the proposed analytical account. Anyway, Cummins might be wrong about that, and, despite his original intentions, the liberality could be, indeed, a flaw of his theory. We suspect, however, that one can also see this as a positive feature, if we consider that this supposed flaw may be the very reason why functional analysis can be broadly used, in the most diverse sciences. If this is true, something may be wrong with Kitcher's (Kitcher, 1998 [1993]) attempt of a reductio ad absurdum of Cummins' theory when he claims that any complex system can be submitted to functional analysis. This is precisely what Cummins' theory aims at, a general account of functional explanation as an approach to understand the behavior of each and every complex system.

The objection that Cummins' approach is too liberal is closely related to another criticism, namely, that it fails to discriminate between functions and dysfunctions, or functions and accidents (Kitcher, 1998 [1993]). We think that this problem can be solved if we take in due account the pragmatic nature of the analytical strategy. We appeal to a functional analysis of a system's capacity if we first consider that it is worth explaining that capacity, and, secondly, we build a decomposition/localization model in order to ascribe capacities (i.e., functions) to the parts of a system that contribute to a disposition of the containing system. Thus, if we say that the nose bridge has the function of supporting glasses, we will have to establish first that this is a systemic capacity worth explaining. We think that just a rather weak argument can be made to support this explanatory task as worth investigating. And, if for some reason one builds a good argument for it, and the function of supporting glasses is ascribed to the nose bridge, there is no problem in doing so, since this function will only be ascribed in relation to a decomposition/localization model related to the explanation of the systemic capacity of using glasses. Functional ascription to the nose bridge will not generalize. There is no problem, also, with the fact that nose bridges are not adaptations to support glasses. No adaptationist charge follows because, when we are trying to investigate if nose bridges are adaptations, we are dealing with an entirely different explanatory task, when compared to the explanation of the capacity of using glasses.

The same argument applies to another case discussed by Kitcher, the ascription of Cummins-functions to mutant DNA sequences involved in the formation of tumors. This would show, he argues, that Cummins-functions are not genuine functions. But what are genuine functions? This argument has the flaw of taking Kitcher's own understanding of function as a ground to judge

Cummins' different rendering of the same term (Nunes-Neto and El-Hani, 2009). It is indeed the case that functions can be ascribed to mutant DNA sequences in Cummins' approach if one convincingly shows that tumor formation is a systemic capacity worth explaining[8], and, in a decomposition/localization model, those DNA sequences are established as relevant parts with relevant activities with regard to the systemic capacity at stake.

Notice that there are criteria that can be formulated within Cummins' theory to discriminate between inadequate and adequate functional ascriptions. They are, however, dependent on the explanatory tasks chosen (and justified) and the modeling approach followed. Not surprisingly, some functional ascriptions that we can make in this manner may be counter-intuitive, but our intuitions are not often –or, at least, not always– good advisors in the search for scientific explanations. For instance, the function of producing rain may be indeed ascribed to clouds in a functional analysis of the global climate system, no matter if it looks counter-intuitive to a philosopher or scientist (Nunes-Neto and El-Hani, 2006; Nunes-Neto, 2008). The survival or death of such functional ascription does not depend on its intuitive or counter-intuitive nature, but on the success of the explanatory models within which that ascription has been proposed.

An important feature to distinguish organizational from etiological accounts is clear in Cummins' works: he is not trying to answer a why-is-it-there question. His functional analysis addresses a how-does-it-work question (Cummins, 2002, p. 158). Furthermore, in Cummins' approach, differently from the etiological theories, functional ascription does not coincide with functional explanation: they have targets at different levels of organization. While performing a functional ascription, we ascribe function to a component of the system, while in functional explanation the target is a capacity of the containing system. Cummins' approach differs from the etiological theories not only in its *explanandum*, but also in its *explanans*. This gives additional reason to reject a synthesis between these approaches to function.

Finally, we should consider Cummins' argument that his account is not teleological. In his 2002 paper, he initially treats teleology in a general sense as "the idea that some things can and should be explained by appeal to their purpose or goal or function" (*ibid*., p. 157). But, then, he simply conflates teleology with the etiological selectionist accounts, in our view with no proper justification, just appealing to a plausibility criterion he does not elaborate at all: "More plausibly, teleological explanation seeks to account for the existence or presence of a biological trait, or structure or behavior by appeal to its function" (id. *ibid*., emphasis in the original). If we keep in mind the more general sense of teleology, unsoundly cast aside by Cummins, we will be able to see that functional analysis still has the form of a teleological explanation. A teleological explanation is an explanation in which it is said that an event happens because it is the kind of event required to bring about a given end (Taylor, 1964). In these terms, we can argue that in functional analysis we appeal to function as that activity of a system's component that takes place in order to bring about a systemic purpose or end, namely, the realization of a given systemic capacity. If we are right, Cummins' functional analysis can be seen as a formalization of the "intra-organic teleology" discussed by Claude Bernard (Caponi, 2003).

If one accepts this argument, the issue of functional explanation cannot be framed as Cummins does: "There are two sub-populations of functional explanation roaming the earth: Teleological Explanation, and Functional Analysis. The two are in competition. [...] I hope to

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[8] We have doubts that a convincing argument can be indeed put forward to this effect. Probably, the systemic capacities to be explained are of the tumoral cells themselves, who behave as if they were single-celled organisms, rather than parts of a multicellular organism as a whole.

help select the latter, and nudge the former to a well-deserved extinction" (Cummins, 2002, p. 157). We already argued that there is (at least some) room to etiological accounts among our philosophical treatments of function. Here, we advance another thesis, namely, that the opposition is not between a teleological and a non teleological approach, but –*contra* Cummins– between an externalist teleological account, dealing with the relationship between organism and environment, and an internalist, intra-organic teleological account.

Collier's interactivist approach to function. Collier (Collier, 2000; Collier, 2004a) proposes an interactivist approach to the individuation and stability of organisms as a basis to reconcile etiological and organizational accounts of function. But, in our view his position is best seen as a formulation of the organizational perspective different from Cummins', not as a synthesis in the sense pursued by Kitcher.

He explains the etiological perspective in usual terms as a view according to which the function of something is that it was selected to do. The fact that the etiological account is externalist, conceiving function as externally imposed, either by intentions in design or outcomes in selection, is stressed by Collier. But he builds a case against this focus only on external factors, since it leads one to neglect any but idiosyncratic organizational requirements involved in the internal dynamics of the biological systems and their interactions with the environment, treating organisms as "black boxes" and looking only at consequences. This is not appropriate, since organizational requirements typically play a central role in the functionality of traits.

The organizational view is also explained by Collier in standard terms as individuating function by organizational role, treating the functionality of a component part as its contribution to the functionality of a containing system. Thus, the organizational approach is internalist, focusing on how component parts constrained in their dynamics by internal organization contribute to the overall functionality of a containing system. Function is internally produced, being determined by internal relations that contribute to overall telos, namely, to maintain the system's organization and autonomy.

Initially, Collier recognizes that these approaches are incompatible with each other, at least as usually formulated, since they have different criteria for functions (Collier, 2004a). But he thinks it is possible to build a synthesis between them. We understand his project as one of formulating the organizational approach in a less extreme form, which might make it possible to accommodate an etiological perspective, and, thus, to consider both external and internal requirements for the functionality of a system. The idea is to offer a way to unify their best aspects in a single approach, while eliminating or mitigating their disadvantages,[9] but Collier does so by taking the organizational perspective as the basis. In the end, he offers an interesting way of formulating the organizational approach, which attempts to accommodate some aspects of the etiological accounts and is likely to avoid some problems found in Cummins' theory, as we will see below.

He justifies the need of a unified account of function in somewhat weak terms: even though in most cases we can decide which account to use on pragmatic grounds, it would be "useful" to have one integrated theory (Collier, 2004a, p. 228). It is important to take this into account since the failure to integrate etiological and organizational perspectives is not a critical problem for his theory: it might be good to have a unified account of function, but it is not a fatal flaw if we don't. There is no problem in living with a consensus without unity if we have pragmatic criteria to opt for one approach or the other.

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[9] For a discussion of what would be the advantages and disadvantages of each approach to function, we refer the reader to Collier, 2004a.

Interestingly, Collier does not include Cummins among the advocates of the organizational approach, regarding him rather as allied with the etiological approach due to his "mechanical" model of function: "Robert Cummins, though he also uses internal organization as criterion for function, also has a fairly mechanical model of function that is compatible with the etiological account" (Collier, 2004a, p. 229). Among the proponents of organizational approaches, Collier mentions Rosen, 1991, and Maturana and Varela, 1980.

We do not agree with Collier in this respect, since Cummins' approach is very different, indeed irreconcilable with the etiological theories. They address different questions and answer them in rather distinct ways, as we argued above. Furthermore, Cummins can be read –and in our view is correctly read– precisely in the same terms used by Collier to explain the organizational approach, namely, as trying to address the ascription of function to a system's part as resulting from its contribution to the functional capacities of a containing system. For instance, Collier's explanation of how we ascribe function to the heart in the organizational approach is quite similar to Cummins' own rendering of the very same case. Collier writes:

> ... the heart is functional because it plays the role of a pump in the blood circulatory system of an organism thereby supplying nutrients and removing wastes as required for the survival of the organism (Collier, 2004a, p. 229).
>
> Cummins in turn writes:
>
> It is appropriate to say that the heart functions as a pump against the background of an analysis of the circulatory system's capacity to transport food, oxygen, wastes, and so on, which appeals to the fact that the heart is capable of pumping (Cummins, 1975, p. 762).
>
> We can see no relevant difference between these two statements. We consider, thus, that Cummins is more properly located among advocates of the organizational approach.

We can see why Collier claims to have built a synthetic account combining the advantages of etiological and organizational approaches: his account intends to combine internal, closed, organizational and external, open, interactive aspects related to functionality. Nevertheless, etiological approaches are more than just externalist. They are attempts to answer a why-is-it-there question in terms of function, by claiming that biological items are there in living systems because of the functions they play, which made they survive selection processes. We cannot see in Collier's approach an attempt to provide an answer to a why-is-it-there question, i.e., we do not see him assuming the same *explanandum* as the etiological approaches, but just the typical *explanandum* of the organizational accounts. The *explanandum* in Collier's theory is the relationship between functionality, autonomy, and closure. In sharp contrast with etiological accounts, Collier, 2000, argues that function can be determined, in his account, by examining contributions to autonomy, irrespective of selection. He is clearly walking on a different terrain in relation to advocates of the etiological approach.

Collier's theory of function interestingly combines organizational and processual aspects. He stresses that he is speaking in terms of process organization or organized processes. He also focuses

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[10] Collier, 2000, p. 290, stipulates that "a process is closed if and only if it requires no inputs at the level at which it is defined". For this reason, when analyzing fermentation and respiration as candidates for closed processes, he concludes that they cannot be, since they need, for instance, glucose as an external input at the same level as the process itself occurs. Therefore, there is no sense in which they can be autonomous. There is no autonomy without (interactive) closure, according to his theory.

on the necessity of coming to grips with closure as a condition for the autonomy of functional systems. Organized processes related to functionality should be closed, in the sense of their integration and self-maintaining organization,[10] but also open, in terms of their interaction with other systems that constitute their environment. We should refer, then, to interactive closure. But, since there is closure involved, we should refer to some global entity or containing system (conceived as a set of integrated, organized processes) that functionality is for. Collier emphasizes, thus, the requirement of building "a notion of individuation or unity based in an open system organizational process closure in which the process closure results in the viability of the very system it individuates" (Collier, 2004a, p. 231). For convenience, he refers to this concept as "autonomy".

Collier stresses that autonomy is inherently dynamical, being entirely based on interacting processes, embedded in an organization that constitutes the integrity of the autonomous system. Thus, autonomous systems should be dynamical entities, showing a dynamic unity designated by him as "cohesion". Cohesion is conceived as the logical closure of the relations among elements of a system that avoids its disruption by internal and external forces. Cohesion is a matter of degree, not an all-or-nothing property. It should allow the individuation of the dynamical entity, however, by forcing us to look for dynamical closure in order to conceive how cohesion both unifies a given entity and distinguishes it from other dynamical entities. Cohesion both unifies and individuates because it is stronger within the system than in relation to any other system. Collier takes it to be the unique dynamical criterion for identity, describing it as "the dividing glue" (Collier, 2004b).

To ascertain, then, that a system is autonomous, even if it is open to the external environment, we only need to verify if the internal organizational closure is greater than the interactive closure. Collier summarizes the requirements of autonomy, according to his theory, as follows:

> In summary, autonomy requires non-equilibrium conditions, internal dynamical differentiation, hierarchical and interactive process organization, incomplete closure, openness to the world, and openness to infrastructural inputs from the material basis of the organization (Collier, 2004a, p. 233).

Autonomy requires a complex organization, since system integrity is preserved through typically complex and self-reinforcing cycles of interaction between internal component processes and systems, and with the environment. Autonomous systems are often hierarchically organized, and, in this case, the system frequently shows a modular hierarchical structure. Modules are explained in Collier's theory as organized processes which are more cohesive within themselves than in relation to other elements of the system. Therefore, they can be individuated and this is what it means, indeed, to say that they are modules and the containing system has a modular structure. This has important consequences to the construction of adequate decomposition/localization models during functional analysis, since in this case the models will be more appropriate and powerful if they deal with modular structure, not with parts at a level below that of the modules. For instance, if a cell has a modular structure, a decomposition/localization model that takes single molecules or even single metabolic pathways as parts may be utterly inadequate. Modular analysis will be needed to propose a model capable of accounting for the functionality of the cell, and the parts may be far from obvious, even counterintuitive.

Autonomy is a self sustaining form of cohesion, in which the components of the system contribute to the maintenance of autonomy. We can see, thus, that autonomy depends on the functionality of these component processes, or, to put it differently, that function ultimately serves autonomy (Collier, 2000). As Collier writes, "functions are for the sake of autonomous entities, and functionality is part of the process based organizational closure that constitutes autonomy"

(Collier, 2004a, p. 234). In his account, function is seen as "… a product of the interlocking processes that underlie the organization that constitutes autonomy" (Collier, 2000, p. 290). He also argues that the most basic form of function is contribution to survival, from which all other forms of function derive (Collier, 2006; Collier, 2008).

Even though he does not elaborate on this point, it is clear that the ascription of function to some component of a system will depend, in Collier's theory, just as in Cummins', on locating what is the contribution of that component to a higher-level capacity, and, ultimately, to the self-preservation of an autonomous entity. Nevertheless, he also stresses the possibility of diffuse functionality, and, thus, of situations in which functional localization and ascription is not possible:

> It is common to talk of the function of some component or process, but the autonomy account of functionality allows that functionality can be diffuse, and not easily localized, so talk of specific function is not always warranted even though there may be functionality. Proper functions certainly exist, but they are possible only when the functional subsystem has become stabilized and modularized […]. Many aspects of function, however, are not so highly modularized, and all we can talk about is functionality, rather than the proper function of some part or aspect of the organism" (Collier, 2004a, p. 234).

Not surprisingly, in the current state of knowledge this applies more strongly to mental phenomena, in particular if they involve consciousness.

Since autonomous systems are hierarchical, there is a hierarchy of functional roles in the system: lower-level functions can contribute to higher-level functions, which can contribute to ever higher-level functions, and so on, until we reach the level of functions that are for the sake of the autonomous entities. Consequently, autonomous entities are usually, if not always, emergent, and functionality cannot be reduced to purely mechanical processes at the substrate of the organization. Nevertheless, if we put Collier's theory in dialogue with Cummins', we will be able to claim that at some level non functional explanation will appear as we go down a series of functional analyses. And when we reach a level at which it is not worth anymore to explain functionally, we can use the subsumption strategy, appealing to the instantiation of lawful dispositional regularities.

Collier explicitly considers the restrictions that follow from his concept of autonomy to the assumption that a kind of organized system might be autonomous. Since his notion of functionality is strictly connected with the individuation of autonomous systems, which show a special sort of process closure, it follows that his approach has the resources to avoid the "too liberal objection". Closure places severe restrictions, he argues, on what can qualify as autonomous, with consequences for ascriptions of functionality and partship, as well to explanations of parts of a system (and, thus, to decomposition models) and functional processes (and, thus, to localization models). Moreover, to identify the focus of explanation, it is fundamental to look at closure conditions, especially of cohesion. If one does not pay attention to closure conditions, Collier, 2004a, p. 234, writes, "It is surprisingly easy […] to misidentify what entity a function serves, and thus to misexplain the function".

In fact, when we strive for explaining the functionality of a system, the system showing cohesion can sometimes be far from obvious. Collier discusses this point by considering cases in which closure conditions are satisfied not only by organisms, but also by lineages and autonomous modules within organisms. We think that it is exactly the attention Collier gives to process closure –something we do not find in Cummins' approach– that provides his account with more resources to steer clear of the liberality objection. As he writes:

> Closure suggests where to look to complete explanations in terms of required elements, the place of these elements, and which element the function is for. They also both restrict and suggest appropriate questions to ask functionally and materially about the target system. In fact considerations of closure may well determine that the target system is not the appropriate system to target at all" (Collier, 2004a, p. 234, emphasis in the original).

He argues, for instance, that neither a rock nor a gas in a box can be autonomous entities. They are not active, because to be active, a system needs to do work, and, in turn, to do work, it should be displaced from thermodynamic equilibrium. Moreover, rocks or gases in a box are not autonomous because they cannot alter their own state to answer to processes beyond their boundaries, and, thus, cannot adapt to conditions surrounding them. In these terms, we can employ Collier's theory –to use an example provided by Kitcher, 1998 [1993], to illustrate the problem of liberality– to show why we cannot ascribe any function to a particular arrangements of rocks in contributing to the enlargement of a downstream river delta. After all, neither the river nor the rocks are autonomous entities in the sense explained by Collier.

But whether or not a given system is autonomous cannot be settled by logical or philosophical argument only. We cannot decide a priori what systems are autonomous. This is a scientific question that should be empirically addressed. This leaves room for counter-intuitive function ascriptions, depending on whether one successfully shows that a given system is autonomous and, thus, exhibits functionality. Collier pays particular attention, thus, to the problem of correctly ascribing and individuating functions, and we see promise in his theory with regard to the construction of a sound distinction between function and accident within an organizational account.

We also think that Collier's theory accommodates cases like that of ascribing functions to items involved in pathological processes, such as the example of a function-ascription statement concerning mutant DNA sequences in tumoral cells. We argued above that in Cummins' approach it is the case that functions can be ascribed to mutant DNA sequences if one takes tumor formation as a systemic capacity (of tumoral cells) worth explaining. This comes from the fact that Cummins does not include in his account any requirement concerning the relationship between the analyzed capacities and the self-preservation of the containing systems. Collier, in turn, explicitly links functionality with its contribution to the self-preservation of autonomous entities. Therefore, one cannot ascribe function to mutant DNA sequences related to tumor formation in his theory, since pathological processes do not contribute to the self-preservation of the system, but, quite to the contrary, destabilize it, being capable of leading to its demise. Moreover, a further reason why the counter-example of the tumoral cells does not apply to Collier's approach lies in the fact that functionality is regarded in this theory as a feature of autonomous systems and, obviously, a tumor is not autonomous.

Finally, differently from Cummins, Collier explicitly conceives his approach as being teleological. Functionality can be thought, in his view, as an end directed activity, since function is internally produced in the system as component parts are constrained to contribute to the organization and stability of the system as an overall telos. In fact, he considers an advantage of both etiological and organizational approaches that they account for teleology of function by reference to some ultimate end, and, moreover, by also considering how function is embodied in material systems.

RESTATING THE CONSENSUS WITHOUT UNITY IN THE DEBATES ABOUT FUNCTION

We argue in this paper that the consensus without unity in the debates about function is properly formulated in terms of two philosophical positions, an etiological and an organizational approach. The etiological accounts are more similar to each other, being all modifications of Wright's original formulation. Among organizational accounts, in turn, we find more diversity. An interesting consequence is that there is much work to be done to build an organizational account that combine the best features of distinct approaches, such as Cummins' and Collier's. This is, however, work to be done elsewhere. Our intention is to advance in future work about this topic by combining an organizational approach to function inspired by Cummins and Collier with philosophical treatments of explanation by modeling mechanisms in biology (e.g., Craver, 2001; Glennan, 2002; Bechtel, 2007), and, also, with a physicalist account of emergence and downward determination (El-Hani and Queiroz, 2005; Vieira and El-Hani, 2008).

For the moment being, let us stress another conclusion of this paper, regarding the legitimacy and domain of application of etiological approaches. We argued against Cummins' attempt to eliminate etiological accounts as a whole, showing that they do have a legitimate domain of application, although much more restricted than originally intended by their advocates: they do not explain the origins of biological items, and can only explain their spread when we are dealing with items instantiating novel functions. Organizational accounts, in turn, have a much broader domain of application, but this can also put them at risk, if we do not face in an adequate manner the problem of their liberality. We can do so if we stick to the pragmatic grounds to select a systemic capacity to be explained and the testable and fallible nature of the decomposition/localization models used to ascribe function to an item. Moreover, Collier's organizational approach has the resources to successfully avoid this problem.

It is also interesting to consider how the concept of adaptation fits into these two accounts of function. An important feature of the etiological account, which still needs to be properly worked out in the organizational approaches, is that it provides a sound distinction between function and accident, connected with the distinction between adaptations, which are traits preserved by natural selection as a consequence of the advantageous performance of a given function, and exaptations, coopted during evolution due to their fortuitous effects, which happen to be advantageous in environments that appear after their spread in a population. But, then, there is the problem that the spread of adaptive traits in a population is not due to function itself, except in rare cases, but to differential efficacy in performing the same function. Those rare cases in which function explains the spread of adaptive traits are rather important in evolution, since they involve the evolution of functional novelties, but they cannot be generalized. And it is in these cases that the etiological approach fits nicely together with an account of adaptations. Therefore, there is importantly more to be said about the connection between adaptation and function.

The organizational approach has something to say here, as a sort of internalist counterpart to the externalist explanation provided by selection leading to the spread of an adaptive trait. Since it contributes to a system's self-preservation or viability, functionality is likely to be selected, but its existence cannot be explained only in terms of the relationship between living systems and environmental conditions. After all, changes of functionality always involve, as Collier, 2004a, stresses, changes in the internal organization of a living system. We cannot understand functionality in biological entities by just focusing on their relationships with the external environment, since there are always organizational requirements that affect how functional traits appear and are modified in living systems.

This allows us to highlight an important limitation of the etiological approach to function: under this account, it is sufficient to show that a trait was selected to ascribe a function to it, but the recognition that changes in functionality necessarily results from changes in the organization of the living system indicates that we need to consider much more to grasp the explanatory role of functional ascriptions. Natural selection acts upon relative differences in autonomy or in entities/processes that contribute to autonomy. Function cannot explain the origins of traits, since it is not selection that makes selected traits functional. Traits are functional because they contribute to autonomy, and they are more likely to be selected for (all other things being equal) if (in combination) they are more functional than other (combinations of) traits. Since it is relative difference in functionality and autonomy that is often selected for, in many cases the spread of traits cannot be explained, also, by appealing to function itself. To explain biological functionality, we cannot limit ourselves to a history of selection. If we limit ourselves to selection, we will turn organisms into black boxes, not recognizing the required changes in organization for any change in functionality, and, thus, losing from sight that we also need an account of relative functionality in terms of autonomy to build an adequate treatment of evolutionary causes (Collier, 2000).

Some of the changes in functionality make a system's part perform an entirely new role, as in the case of the evolution of wings in insects from the branchial branch of the ancestral arthropod biramous limb (Nunes-Neto and El-Hani, 2009). In these cases, function plays an explanatory role in accounting for the spread of the biological item, as elucidated by the etiological approach to function. Most of the changes, however, just make a component part perform the same function it performed before, but sometimes in a more efficient manner.[11] In these cases, natural selection may favor the new organization of the system in which this part plays that function more efficiently. The change is to be called an adaptation if its spread is a product of natural selection, but since in this case it is not function per se that explains the spread, the etiological perspective cannot account for the explanatory role of function. The organizational approach suffices here, ascribing function to the component of the system that globally performed the capacity at stake in a more efficient way, clearly distinguishing the target of functional ascription from the target of functional explanation.

Finally, we should briefly consider another taxonomy of approaches to function. Wouters, 2005, discriminates more than two approaches. According to him (p. 126), functional talk in biology is appropriate in four different contexts: (i) to talk about parts and activities of organized systems ("functions are roles in an organization"); (ii) to talk about systems that have goals or purposes ("functions are means to ends"); (iii) to talk about systems that result from a process of reasoned design ("functions are the reasons why something is there"); (iv) to talk about parts of systems that have a selection history ("functions are the reasons why something is there"). The categories pointed out by Wouters can, in our opinion, be grouped into only two, if we notice that the first two categories can be gathered together in the organizational approach to function, and the third and fourth categories in the etiological approach, although the third one can be regarded as an inadequate view of the evolution of traits, which can be criticized based on how it appeals to the concept of design (see note 2) and its reliance on function to explain the origins and spread of traits.

In another passage, Wouters (*ibid*., p. 125) claims that

> Roughly speaking, one may distinguish five main approaches to an analysis of function: the systemic approach [...], which defines function as a contribution to a capacity of a complex system [...]; the goal contribution approach [...], which defines function as a contribution to the achievement and/or maintenance of a goal state [....]; the life chances approach [...], which sees functions as effects that

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[11] We are focusing on just one part for the sake of the argument, not intending to advocate that evolution can be explained by atomistically decomposing a living system into entirely separate parts. Structural and functional correlations abound, and natural selection has to deal most of the time with correlated suites of traits, some advantageous, some disadvantageous. It is the net cost/benefit relationship that counts, as translated into contributions to fitness.

> enhance the life chances of their bearers [...]; the etiological (or historical) approach [...], which sees functions as past effects that explain the current presence of the function bearer [...]; and the non-historical selection approach [...], which sees functions as effects for which the function bearer is selected (in certain circumstances)...

We think that the first two approaches can be grouped into the organizational perspective. The second approach qualifies itself as organizational since we interpret the organizational perspectives analyzed here (Cummins' and Collier's) as teleological: the achievement or maintenance of some systemic capacity is a goal of the system as a whole, and this process is dependent on the realization of some capacities of the system's parts. The third, fourth, and fifth approaches can be grouped into the etiological perspectives. The third and fourth approaches are straightforwardly etiological, and the fifth one can also be regarded as such, as it becomes clear when we examine the example given by Wouters (*ibid*., p. 125) to elucidate it: "Finally, according to the non-historical selection approach propelling the blood is a function of the heart (of the organisms of a certain population) because propelling blood is why hearts are maintained in that population." That a function not only originates, but also maintains a trait in a population, is the central claim of etiological theories. Therefore, we think there is a good reason to avoid the proliferation of theories on function proposed by Wouters, sticking to a distinction between just two perspectives, the etiological and the organizational, as we propose in this paper.

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