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ONTOLOGY AND TELEOFUNCTIONS: A DEFENSE AND REVISION OF THE SYSTEMATIC ACCOUNT OF TELEOLOGICAL EXPLANATION

ABSTRACT. I defend and revise the systematic account of normative functions (teleofunctions), as recently developed by Gerhard Schlosser and by W. D. Christensen and M. H. Bickhard. This account proposes that teleofunctions are had by structures that play certain kinds of roles in complex systems. This theory is an alternative to the historical etiological account of teleofunctions, developed by Ruth Millikan and others. The historical etiological account is susceptible to a general ontological problem that has been under-appreciated, and that offers important reasons to adopt the systematic account. However, the systematic account must be revised to allow for two distinct kinds of teleofunctions in order to avoid another ontological problem.

1.

The vibrant debate that has grown around the issue of biological purpose and other kinds of normative functions (hereafter *teleofunctions*) has largely focussed upon the question of whether a description of teleofunctions can be developed which allows us to make use of them in theories like biology or psychology, but which does not refer to scientifically illegitimate entities like final causes or irreducible purposes. There has been great progress in understanding and answering this question, especially in the development of the historical, etiological account of teleofunctions. This account, as developed most notably by Ruth Millikan, is a substantial theoretical achievement both in describing how there might be a scientific theory of teleofunctions, and also in applying this account of teleofunctions to solve problems in the philosophy of language and the philosophy of mind. However, the historical etiological account has also been found vulnerable to a number of important criticisms. One of the most important, but least appreciated or developed, of these criticisms is the recognition of what I will call the *ontological problem:* granted that an account of teleofunctions does not ostensibly make

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reference to scientifically illegitimate kinds, is the property of having a purpose, as described by the theory, a kind that a naturalist would want to admit? We might refer only to scientifically legitimate kinds in our theory, but do so in a way that admits entities that we would prefer to avoid and that we can do without. I shall argue that the historical etiological account does just this. These specific ontological difficulties for the historical etiological account are best addressed by the systematic account of teleofunctions, recent versions of which have been developed by Schlosser (1998) and by Christensen and Bickhard (2002). However, this theory is itself vulnerable to an ontological challenge which has not previously been described. This challenge in turn reveals that for many applications of a theory of teleofunctions, our best explanation will make use of two different accounts of systematic teleofunctions.

My task in this paper is therefore constructive: I defend the systematic account of teleofunctions on grounds of a hitherto unappreciated benefit, and develop a revision of this theory that clarifies and strengthens this benefit. I proceed as follows: In Section II, I review the historical etiological account of teleofunctions, and the problems that it faces, with special attention given to the ontological problem. In Section III, I review the systematic account, and show how it escapes the challenges to the historical account, but as presently best formulated it also glosses over an important ontological difficulty. In Sections IV and V, I argue that the solution to this ontological difficulty is to adopt two different accounts of systematic teleofunctions. In conclusion, I grant some remaining challenges to the systematic account.

2.

The challenge to accounting for teleofunctions is to explain how some structure can have a purpose. We say, for example, that the purpose of a human heart is to pump blood. It is not sufficient to note that any particular heart pumps blood, nor even that most hearts pump blood, since the notion of purpose is a normative one: it tells us what a thing *should* be doing, or what it is (in some sense) *meant* to do, and not necessarily what it is or was or will be doing. This is clear when we recognize that a heart can be *dysfunctional:* it can fail to serve its purpose. Also, many kinds of structures are such that many instances do not serve their function, so that we cannot even refer to their

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typical behavior. To use an example of Ruth Millikan's: we say that sperm have the purpose of inseminating an egg, and yet the overwhelming majority of sperm in any particular mammal will not accomplish this purpose.

A clarification is required at this point. It is not uncommon in the now substantial literature on teleofunctions to talk of teleofunctions being had by structures (e.g., hearts), events (e.g., a heart beat), and even by seemingly abstract entities (e.g., Millikan allows individual words). In what follows, I will be concerned with teleofunctions as properties of structures of organisms. I will focus on structures for at least three reasons. First, although it is quite clear how an event may be said to have a teleofunction (the beating of a heart has a clear purpose), there are substantial differences between events and structures having teleofunctions. An event which has a teleofunction may in part constitute that teleofunction; thus, the beating of a heart may reasonably be called part of pumping blood. Instead, the heart itself can only be said to have (that is, to be necessary and in some restricted sense sufficient for) a capability for an activity (which may indeed be beating) which is necessary for and may (in part) constitute the purposeful behavior. Thus, the situations can be quite different, and we should be careful not to confuse them. Second, in what follows I will argue that we need to refer to an organism or other system in order to identify teleofunctions. It is easier to properly identify and place a structure in a system than it is to place an event in one; an organism, in fact, is a collection of functional structures (organs). I grant that an organism's life is a collection of events, but I see no way to specify this collection except by referring to the actual structures that enable those events. Third, I believe the ontology of structures is more conservative, and more manageable, than an ontology of events. In fact, without being shown some alternative method, one should expect that an ontology of events for a teleofunctional theory will ultimately make reference to structures, or at least organisms, to properly distinguish and categorize the relevant events (see Section V). None of these points is meant to be a refutation of the idea that events can have teleofunctions; rather, they are reasons to believe that focussing on structures is a more conservative position. A focus on structures is sufficient to make the arguments that I want to make in this paper. In the case where an events-based teleofunctional theory is relevant to my argument, I will discuss this alternative.

Because they are normative, teleofunctions appear at first consideration to be scientifically illegitimate. Superficially, it would seem

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that science can tell us about causes, and about history, but not about what should be the case. This leaves us with a serious problem: biology, psychology, and related disciplines require talk about teleofunctions, but it would seem that teleofunctions have no place in a naturalistic theory. The principal challenge is therefore to explain how a science or a naturalistic world view can countenance purpose, or to explain how biology and other sciences are going to get along without teleofunctions. Valuable work has been done taking the former approach, developing accounts of teleofunctions that do not take purpose as a primitive but rather analyze it using naturalist terms.

An early, and powerful, general formulation of teleofunctions comes from Wright (1976). Wright worked to capture the general features of teleofunctions, and developed a schema to describe, and potentially to reduce, a teleofunctional claim. I paraphrase his schema (1976, 81) here:

> *S* has teleofunction *F* iff:
> (1) *S* does (has as a consequence) *F*;
> (2) *S* exists because it does *F*.

Wright's schema has proved very robust, and all of the contemporary theories of teleofunctions can be understood and paraphrased as some version of it. As formulated, this schema leaves unclear what the proper role of particulars and kinds are, but *S* is clearly a kind; clause (1) is straight-forward in that we surely want it to be the case that instances of the structure do the thing it has as its teleofunction, but we must caveat this by noting that only some instances of the structure may perform this teleofunction (again, the case of sperm is an illustrative example); and clause (2) can refer to either some or all instances of *S*, depending upon one's explanation of how this clause will be satisfied.

It is clause (2) that requires the most explanation, for in what sense can such a structure be said to exist *because* it serves this function? This is where all of the heavy lifting has been done in developing theories of teleofunctions. The most successful such theories have been etiological accounts, which cash out clause (2) by holding that in natural organisms the structure is there after having survived natural selection because it does *F*.

Millikan has developed (1984, 1993), and most fruitfully applied, a historical etiological account. An independent but similar development of a historical etiological account is in Neander (1991a, b). Millikan's formulation is distinct from Wright's, but can be

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reformulated into it. To paraphrase and simplify: Millikan argues that a kind of structure has a teleofunction if some instances sometimes perform the relevant action and also all present instances (that have the teleofunction) belong to a *reproductively established family* in part because some instances of this family in the past sometimes performed that action. In nature, organisms and their organs can be such reproductively established families. Instances of these relevant structure types can be reproduced in part because former instances of the type gave some selection advantage to the organisms they compose, and this selection advantage came because these structures or events undertake the particular actions that are their teleofunction. Other kinds of teleofunctions can arise as a product of these relevant structures but not be directly selected for, resulting in what Millikan calls *derived* functions. Thus (Millikan 1993, 13–14):

> To put things very roughly, for an item *A* to have a function *F* as a "proper function", it is necessary (and close to sufficient) that one of these two conditions should hold. (1) *A* originated as a "reproduction" (to give one example, as a copy, or a copy of a copy) of some prior item or items that, *due* in part to possession of the properties reproduced, have actually performed *F* in the past, and *A* exists because (causally historically because) of this or these performances. (2) *A* originated as the product of some prior device that, givens it circumstances, had performance of *F* as a proper function and that, under those circumstances, normally causes *F* to be performed by *means* of producing an item like *A*.

Millikan has used this account to develop original insights in the philosophy of language. However, this historical etiological account faces some serious challenges. Most widely discussed have been the fact that structures may change their function after having been selected for, or otherwise perform a new teleofunction that has been only of utility in immediate history. In response to this concern, a number of different revisions to the historical etiological account have been offered. Godfrey-Smith (1994) argues for a modern history theory, in which only the most recent selective advantage conferred by some structure should count as determining its teleofunction. Griffiths (1993) develops a similar proposal. These accounts attempt to retain the historical spirit of the historical etiological account. However, as Schlosser has observed (1998, 304), by limiting the period of time in which a functional role can be determined we can only reduce the number of changes in a teleofunction that fail to satisfy the etiological account; we do not alter the fundamental problem that it is what a structure is actually doing, and not what it did, that we refer to when we describe its present activity in teleofunctional terms. Bigelow and Pargetter (1987) propose a

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propensity view, in which a structure has a teleofunction because of its propensity to improve fitness. Thus, a structure may have a teleofunction that was not yet selected for, but which tends to give the kind of advantage that typically will (eventually) be selected for. This drops the historical requirement, but retains the externalism of Millikan's account. However, the propensity account suffers from the fact that it is difficult to make clear what a propensity to improve survival is. A principal advantage to the etiological account was that it replaces such talk with concrete historical events.

The difficulty with the historical etiological account can be seen in a powerful common sense criticism: it fails to explain how actual field biology works. An ethologist, confronted with a new organism, is not stumped to explain what it is up to in its everyday behavior. Rather, she observes its activities, and ascribes to them purposes based on her understanding of organisms more generally. In her account of teleofunctions, Neander explicitly argues that a historical etiological account is an analysis of the current concept of biological function used by biologists. She addresses this objection about biological practice by arguing that to be acting consistently with the historical etiological account, scientists need only to be identifying as the teleofunction of some structure the very activity that it evolved to do (1991a, 176). In contrast, Millikan claims to be describing what teleofunctions are; she is not performing conceptual analysis of what "purpose" means, nor trying to describe the practice of biologists. Thus, Millikan could claim that the scientists who ascribe biological functions without reference to history are not identifying proper functions. Or, more likely, she could say that they are doing something like drawing analogies: they see activities and assign to them teleofunctions as reasonable hypotheses, arising perhaps from understanding of genuine historical etiological teleofunctions, and all such hypotheses must ultimately answer to a historical etiological account. However, both responses do not address the real concern. Biologists and the biological sciences do not posit teleofunctions as historical, external entities, but rather as activities, generally of internal structures, that have current utility to the organism. If we can link a structure with its teleofunction without reference to (distant) history, and if our determining criteria are (or can be) sufficient to explain the existence of the teleofunction, then the history is not essential to the property of being a teleofunction.

This issue of how the field biologist must actually be working is really just an illustration of a broader issue, less discussed in the

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literature, concerning a general ontological problem which arises for the radical historicism and externalism of the etiological account. In sum, the problem is that a teleofunction, according to the historical etiological account, is a very odd kind of property. The identity conditions of the property are in part external to, and in some important senses quite independent of, the relevant structure. Of course, there are many relational properties that we make use of, and they often have external determinants, but two features make the historical etiological account particularly problematic. First, the external features which determine the property can have their effect even if there is no relevant causal action on the structures in question or their operation. Second, the external features which determine the property can be wholly beyond the reach of measure or any other kind of verification. As a consequence of these features, a range of specific problems for teleofunctional explanation arise.

The first difficulty, concerning the lack of the relevant kind of causal action, arises because according to the etiological account, instances of structure kind *S* have as a teleofunction *F* only if external historical conditions are such that instances of *S* were selected for because they did *F*. Thus, whether *S* has a teleofunction is ultimately determined by factors external to instances of *S* and to the organism. This general concern has been raised to Millikan's account in a misleadingly exotic thought experiment of a pair of identical organisms. We are to imagine an organism that evolved, and also a perfect copy of the organism that appears *ex nihilo*. According to the etiological account, the evolved organism is composed of structures that have teleofunctions, where the exact duplicate has none:

> Take any object, then, that has a proper function or functions, a purpose or purposes, and consider a double of it, molecule for molecule exactly the same. Now suppose that this double has just come into being through a cosmic accident resulting in the sudden spontaneous convergence of molecules which, until a moment ago, had been scattered about in random motion. Such a double has no proper functions because its history is not right. It is not a reproduction of anything, nor has it been produced by anything having proper function. Suppose, for example, that this double is your double. Suddenly it is sitting right there beside you. The thing that appears to be its heart does not, in fact, have circulating blood as a proper function, and when it scratches where it itches, the scratching has no proper function. (Millikan 1993, 17–18.)

Millikan bites this bullet, and argues that no such double ever has or could exist, and so the issue is moot. This response needs no debate since it turns on a misleading feature of this thought experiment:

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nothing so exotic as instantaneous *ex nihilo* organisms is required. The *ex nihilo* organism is extremely unlikely to arise (so much so we are safe calling it impossible), but this does not change the fact that any structural change that has utility and which we might later call a teleofunction of that structure must arise *before* it can be selected for. The difference between what actually happens in evolution, and occurrence of an *ex nihilo* organism, is a matter of degree, not of kind. In evolution, any incremental change in a structure must appear in a population and provide some benefits for some time before the change is selected. Thus, many situations like the following must have occurred: a population of some kind of organism is split into two, perhaps by some kind of geographical dividing of its habitat. In one group, instances of structure type *S* are doing *F*, and *S* spreads through the population just because of chance alone (alternative reasons include that *F* is connected somehow to another teleofunction which is selected for – perhaps another teleofunction of *S* – but *F* is never the reason that *S* is selected for itself). In the other group, *S* is causing the same kind of things to happen in these organisms (it is doing *F*), but environmental conditions are slightly different, and *S* is selected for because of this activity *F*. The relevant features of *S* have spread through both populations, and are doing the same things. Thus, in these two populations, there is nothing in instances of *S* that is relevantly different from the perspective of its causal role in the organism, or even the immediate present environment. However, the historical etiological account has it that in the latter population, *S* has as a purpose *F*, and in the former it does not. Being a teleofunction is, on such a view, determined by external criteria that may make no difference of any kind to the internal workings of the organism. Note also that it could be that in the former population *S* does *F* and *F* is even beneficial, but suppose that the environmental conditions are so propitious that the benefit never makes a significant difference in reproductive success. Again, doing *F*, and *F* having the same benefits, are insufficient for *F* to be a purpose in this population. Wholly external changes alone make the determination.

The second problem arises because on Millikan's account we need not even be able to verify in any way what the historical conditions are. That is, suppose that just before (within one generation of the time) we make our observations, the environments of the two relevant populations of organisms described above are destroyed by rampant clearcut logging. Many of their predators and their food sources are driven extinct by this action, but the organisms manage

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to survive. No historical or fossil records exist of the prior environment. We then have it that organisms from the former population have structures of type *S* doing *F*, but *F* is not a purpose of structures of type *S*; in the latter group, the organisms have structures of type *S* doing *F*, and *F* is a purpose of structures of type *S*. But we can never confirm this, and no measurement of any kind can determine between these cases. We can of course be strong realists about teleofunctions, and accept that some things are forever beyond our knowing. But if we had an explanation of teleofunctions that was verifiable, it should count as a superior explanation to any naturalist. That is, suppose we have two ontologies O_1 and O_2, that differ only in the positing of some property P_1 versus P_2, and these ontologies have equivalent explanatory power, but in O_1 we cannot determine in any arbitrary case whether P_1 is present, whereas in O_2 we could in principle always determine for any arbitrary case whether P_2 was present. In such cases, for the naturalist at least, O_2 is always preferable to O_1. I shall argue in the next section that we can replace the ontology of the historical etiological account with a verifiable systematic account.

To observe that the externalism of the historical etiological account makes teleofunctions peculiar entities is a highly general criticism. Even the now looming questions are abstract: do we want to have a kind of property in our naturalist ontology that can be present or absent without having any current causal effects, can potentially appear in structures where the determining factor (the right kind of history) can have no causal effect on the structure in question, and in any case may depend on historical events which cannot be confirmed? However, this historical externalism has more concrete and problematic consequences. Such consequences can be illustrated when we apply the theory to more specific issues. Three such cases include the specification of content, the nature of phenomenal states, and the moral import of purposes.

(1) *Content*. This ontological problem of radical externalism for the historical etiological account has consequences for any theory that tries to make use of teleofunctions to account for representational content. A theory of teleofunctions has powerful generality to help explain representation, in part because the most difficult aspect of representations to explain is their normative dimension (a representation can be incorrect), and this can be explained with teleofunctions. However, if we use an etiological theory of teleofunctions, the content of representations becomes something determined not by

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the internal states of the organism that has the representations, but rather it is a function of the historical condition of the structures that enable them. Thus, in two organisms, say frogs, that are in all relevant ways similar, a particular state in one can mean ‘fly’, and the identical state in the other could mean ‘beetle’, because their historical conditions were different. Millikan’s program to explain representation and language embraces this surprising consequence. But one reason to reject such radical externalism is ontological: from the perspective of the goal to understand the mind, we may want to reject any approach which makes much of what we aim to explain external and potentially wholly inaccessible (as noted above, we many never be able to determine what some internal state of an organism means if the environmental conditions are somehow lost to us).

There is a parallel here with a more general criticism made by Fodor of externalist theories of content. Fodor has argued that, if we accept externalism about content, we could only really know the content of some mental state after our sciences of the external world were complete: ‘the naturalistic psychologists [as Fodor calls those committed to an externalism about content] will inherit the Earth, but only after everybody else is finished with it’ (1980, 248). Even those of us with sympathies for a causal-historical theory of reference (Kripke 1972), and for Putnam’s arguments for a linguistic division of labor (Putnam 1975), should recognize that Fodor’s criticism is powerful.[1] But the case is worse for the historical-etiological account of teleofunctions, since the kind of scientific natural kinds that motivate the externalist account of content that Fodor is criticizing are in principle confirmable (we derived tests to show water is H_2O, and may decide as a society to require that some of our linguistic division of labor be based in part on something like verifiability – in fact, arguably we have done this when we defer to scientists). The historical externalist account of teleofunctions must allow for meanings which can never be confirmed.

(2) *Consciousness*. Arguably the most promising program to explain phenomenal experience has been the representational theory of consciousness, in which phenomenal states are understood to be representational states (Tye 1996; Lycan 1996; in a distinct sense, Churchland 1989). However, if this view is coupled with an account of representations based on an etiological account of teleofunctions, then phenomenal experience becomes ontologically bizarre (see DeLancey 2002, 169ff). Consider the organisms with a divided population described above, and suppose that S is a structure that allows

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the organisms to perceive a shade of deep blue that those lacking the structure cannot perceive. In the population where this was selected for, there is a teleofunction of seeing deeper blue, and if we suppose that phenomenal experience supervenes on teleofunctions, there is an experience of seeing deeper blue. But the internal states of individuals in the other population are identical internally in the relevant respects, and yet there would be no such experience. This follows directly from the claims that all phenomenal experiences are representations, and that all representations are teleofunctional states. This is a radical consequence: two organisms could have relevantly identical internal states, perform all the same relevant behaviors, and yet one is feeling pain or smelling strawberries or seeing blue, and the other is not.

(3) *Moral value*. One of the leading views in environmental ethics is biocentric individualism, the view that all individual organisms deserve some moral respect because they have some of their own purposes (Taylor 1986; Varner 1998). On this view, it is purposes which are deserving of moral respect, which in turn means that we have an important interest in developing a better understanding of purposes. This can be seen as a problem in understanding teleofunctions, and Varner (1998) has the most carefully developed account of this kind: to understand what kinds of welfare interests an organism has, we identify the teleofunctions that it has. These then *prima facie* are activities which deserve some, albeit often relatively little, respect. However, when the teleofunction theory in use is historical etiological (as it is in Varner 1998), this account allows for many obvious dysfunctions to have moral value. Certain features of an organism which have been explicitly selected for, but which obviously are harmful to the organism, must count as teleofunctions and therefore as welfare interests of the organism (see DeLancey 2004). For example, oncomice are transgenic organisms manufactured with a gene that causes them to develop rampant cancerous tumors. This gene has been explicitly selected for by researchers. And yet, although it is common sense that such a gene is dysfunctional, according to the historical etiological account it has a teleofunction, and according to the moral view that adopts this as an account of value, tumor development is a welfare consideration of these mice and deserves some moral respect.[2] This example is rather specific, but the problem here will generalize to many other kinds of cases where we grant that an organism's flourishing (including human flourishing) is in part constituted by achieving one's purposes, including biological pur-

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poses. Note that the unverifiable nature of the historical etiological account means also that we can never be sure that an apparent defect is really a defect.

These three examples illustrate concrete cases where, as an explanation, the historical etiological account of teleofunctions yields difficulties that arise from the peculiar ontological status it gives teleofunctions. Many different attempts to apply the theory will result in unrealistic and even absurd consequences.

3.

Both the recent-history (or changing teleofunctions) objection, and the ontological objection, reveal that what is needed is an account of teleofunctions that recognizes them as internal features of current utility to the organism. A number of accounts of teleofunctions based on the idea of an organism as a *system* have been developed to meet this end. Christensen and Bickhard contend that

> Normative functional organization itself is analyzed in terms of the interdependencies of the processes that constitute the system. Each of the processes that form part of the system requires outputs from other processes in the system to function, and in turn contributes to the requirements of other processes. These process interdependencies constitute norms on adaptive functioning, since if the requirements of a particular system process aren't met it will cease to produce the outputs required by other processes, potentially resulting in propagating dysfunction that may reduce or destroy the viability of the system. (2002, 4)

In other words, teleofunctions are had by structures that perform certain kinds of sustaining activities in kinds of systems. Their normative nature comes from the fact that (portions of) the systems that they constitute cannot exist or continue without those activities. Systematic teleofunctions have a kind of normative force then akin to a Kantian hypothetical imperative. The structure in question *should* do such and such activities *in order to* maintain (some portion of) the complex system to which it belongs. This same insight is developed by Schlosser (1998). Schlosser retains the Wright schema, and uses it to specify when a structure has a teleofunction for a system. His formulation, somewhat simplified, is

F is a teleofunction of structure or event, $S(t_1)$ iff:
for a certain period of time, T, where $t_1 < t_2 < t_3$, and T
ranges from before, t_1 to after t_3,

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(1) $S(t_1)$ is directly causally necessary to establish $F(t_2)$ under circumstances c_1.
(2) $F(t_2)$ is indirectly causally necessary to establish $S(t_3)$ under circumstances c_2.
(3) The causal relations between $S(t_1)$, $F(t_2)$, and $S(t_3)$ are complex.

The basic insight is simple but powerful. What makes a structure or event *S* have a teleofunction *F* is that it plays a role in a complex system, such as an organism, that in turn allows and supports *S*, including *S* doing *F*. An organism, for example, is a bootstrapping collection of teleofunctions, and this is what makes it both an organism and what makes some activities into teleofunctions. Schlosser puts this, 'the *functionality* of a state can be defined as the *conditional necessity for complex self-re-production'* (315). The observation that the causal relations must be complex has its clearest case when the relations are participating in systems that are complex (e.g., see Schlosser 1998, 305). A system is complex when there are many different kinds of teleofunctions, such as F, connected through these chains of dependency, and when the chains of dependency tend to vary. About this Christensen and Bickhard are more explicit, since their account is essentially linked to their explanation of what it is to be an autonomous system: 'The central plank of our account is a theory of autonomous (or "self-governed") systems that are composed of webs of interdependent processes whose collective activity is self-generating' (2002, 3).

The systematic account makes clear what a system is, but it does not make it as clear what a *complex* system is. One might especially be concerned that there is no clear line between complex systems and trivial ones (see Nagel 1977, 273ff). This concern has been addressed in part by Schlosser (1998, 329ff), but two observations should be stressed here. First, complex systems are phenomena. We can point at them in the world – for example, all organisms are complex systems, and humans are very complex systems. For this reason, we should not expect a complete and final definition or characterization of complex systems. Rather, we need only to be able to identify them, and have some hypotheses about what makes them complex systems, and then we can investigate them empirically. We may revise our theories as we learn more. Second, since there are natural phenomena which are complex systems, and since the relevant kind of complexity is a property that comes in fine degrees, we should expect that there is no non-arbitrary line to be drawn between complex systems and other, simpler systems – just as, for example, there is no non-arbitrary line between a planet and an asteroid, or between a hurricane and a big tropical storm. There are systems that satisfy all the other criteria above except the stricture that they be complex. This should be unsurprising: a naturalist account often results in there being no clear division between certain kinds. It follows that there is no non-arbitrary line to be drawn between complex systems with structures that have teleofunctions, and simple systems which we may want to deny teleofunctions.

This does mean that we should revise Schlosser's schema, for it refers only to complex relationships and not complex systems. There are at least two reasons to believe this is too weak if it is to mean something other than the relationships of a complex system. First, I argued above that there were phenomena (organisms) that are complex systems, and therefore the requirement in the systematic account that the relationships be complex need not be fully defined but can rather refer to actual cases. However, if we drop the criterion that these relations constitute an organism, we give up the ability to refer to organisms as uncontroversial instances of complex systems, and must give now an independent account of complex relations. Second, reference to complex relations, and not complex systems, will also require some account of how we are to pick out the relevant types that play a role in (some applications of) the systematic theory. It is quite clear what it means to say that a particular wolf's heart is of a kind that are wolf's hearts – the complex system (of the kind *Canis lupus*) picks out for us the relevant system, and this in turn specifies the kinds that constitute the elements which we may see being reproduced or sustained (e.g., hearts of *C. lupus*). But if we drop reference to complex systems, we must have an explanation of what the relevant kinds are that is independent of reference to organisms or other complex systems that are granted as phenomena. For these reasons, I will require, in what follows, that systematic teleofunctions be identified in relation to complex systems, and not just complex causal relations. (I return to this issue in Section V below.)

These observations are relevant to some alternatives that have been offered to the systematic account of teleofunctions. Carolyn Price has recently proposed a revised historical etiological account that requires of any teleofunction that early instances of its kind supported another structure which performs some activity that in turn supports the first kind. We can understand this as a simple, two step complexity requirement.

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> Where *d* is an item and *F* is an activity, *d* has the function to do *F* if and only if there is some family of items *G*; there is some family of items *D*, to which *d* belongs, and which consists of items produced by a member of *G* in some manner *M*; there is some item g which belongs to *G* and which produced *d* by *M*; and there is some activity *E*, such that:
>
> CD.1 Members of *D* have done *F* in the recent past.
>
> CD.2 The fact that *g* produced *d* by *M* is explained partly by the fact that the doing of *F* by members of *D* in the recent past assisted *g*, or other members of *G* that are ancestors of *g*, to do some further thing *E*. (Price 2001, 36)

Alternatively, Price allows that *g* does not produce but rather maintains *d*. Price holds that this account is superior to a systematic account because, she claims, it can rule out a kind of problematic case that the systematic account cannot. The kind of case is one introduced by Bedau (1991), in which crystals in a stream bed are slowly forming. If these form into 'chunky' shapes, they can even slow the water flow around them, and increase thereby the rate of their formation. Price argues with Bedau that crystal formation should not be seen as a case of a teleofunction. As stated in a preliminary way above, her theory cannot rule it out, since first her theory allows that *G* and *D* can be the same, so that one layer of crystals could count as an instance of *D* for the next, which would count as an instance of *G*, and so on; and second, if the water is slowed then this may count as the second (and a different) activity that is both caused by and supports crystal formation. Price's responses are to pursue each of these problems in turn.

First, Price requires that 'affecting the producer g should not include bringing it into being' (39). Presumably in at least some cases 'affecting the producer' or 'assisting the producer' is a necessary condition for *g* to exist, since Price does not rule out that things of kind *D* may be structures with a teleofunction which supports directly or indirectly the existence of things of kind *G*, including assisting in their production. This is evident in Price's own examples of a thing of kind *G* (which she calls a 'governor'): 'The governors of the heart will include those organs that, like the liver, benefit directly from the operation of the circulatory system' (2001, 38). The liver is of course supported by the circulation done by the heart, so that without the heart the liver both cannot continue its teleofunctions and it cannot be reproduced. The heart thus contributes to sustaining a liver and to bringing other livers into being, since its teleofunction is necessary for all reproductive activity. In such cases, this rule that the governor cannot be 'brought into being' by the structure it governs is nothing

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more than the requirement that things of kind *D* cannot be alone sufficient for the development of things of type *G*. But that is just to require that there are other features (other things than structures of type *D*) of the system contributing to the bringing into being of things of type *G*. For the many cases where *d* will be a structure that contributes to the survival and reproduction of the organism, this restriction against 'bringing into being' is nothing more than a requirement that the relationship(s) that sustain and reproduce things of type *G* be complex.

Second, Price claims she needs to also account for the two step case in Bedau, where (following the schema above) the crystal is to act as a *d* and some feature of the stream itself is to act as *g*. Price argues for what she calls 'the immediacy condition': 'where a device *d* is present because earlier devices of the same type did *F*, *d* will have the function to do *F* only if there is nothing else that those earlier devices did that provides a more immediate explanation for the presence of *d*' (58). What is an 'immediate explanation'? Price gives a clarifying example: 'Compare: I frightened the burglar by turning on the light, by flicking the switch, by moving my finger; but I did not frighten the burglar by entering the room' (59). The example suggests that we should understand by 'immediate explanation' the last step in the causal chain that is necessary for the event in question. This seems a potentially important insight, but it also does not seem to apply to the test case that Price has picked out. Price argues that this solves the Bedau case because 'there is something else that the crystals do that provides a more immediate explanation for their survival – namely to replicate' (60). This seems to trade upon an ambiguity between the growth of crystals and the survival of some layer of crystal – that is, layer 1 of crystal may immediately explain the formation of layer 2 of crystal, but this does not explain the survival of layer 1. Furthermore, it appears we can rephrase Bedau's example very slightly to retain the same problem. If we are to admit some features of the stream as a structure, then if one lets the arrangement into the crystal lattice of local molecules of the appropriate type be the activity *F*, and the moving of the molecules of the appropriate type near the existing lattice be activity *M*, and the slowing of the water flow be activity *E*, then we appear to have satisfied the schema as given above and still retained the plausible immediacy condition. But such concerns are secondary to the real issue. We need not be fixated on the details of Bedau's case. We need only one two-step case to demonstrate that Price's single-governor condition is not yielding a satisfactory

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account of teleofunctions. There very likely are instances of two or a very few individual structures alone each supporting the other, such that with just two or a very few kinds we have the satisfaction of the teleofunctional schema that Price provides. An example might be two chemicals in a solution each of which in a different way is sometimes necessary, perhaps as a catalyst, for the production of the other. Price has not shown that her account rules out any such case where there are just two or three or a very few interdependent processes, but of course any such very simple case is going to be as counter-intuitive as an example of purposively behavior as is Bedau's case.

What is needed to overcome Bedau's example and other kinds of simple cases is the recognition that there is no difference in kind, but rather in quantity, between such cases and the teleofunctions of an organism. Crystal formation and the slowing of the water should not count as teleofunctions (or should count as trivial or degenerate teleofunctions – this is a matter of semantics now, not of substance) because the relationship is a simple one, of just two interlocked activities. The systematic account will require that the relationships be complex; this will include that the various interdependent teleofunctions will be of distinct kinds (replacing Price's requirement that the relationship not be one of 'bringing into being'). This directly confutes Price's claim that the problem with a systematic account is that it cannot handle the Bedau case because 'there will be no guarantee that the part of the system that is responsible for replicating the device is not identical with the device itself' (41). If it is identical, then the system is not complex. Thus, the systematic account alone offers a clear explanation of how to interpret these trivial or degenerate cases in such a way that they are not successful examples of a *reductio ad absurdum*.

The systematic account of teleofunctions admirably avoids both the recent-history objection and the ontological objections. Since the teleofunction of some structure at some time is its present and ongoing performance as part of the relevant kind of system, no problem arises for any change in the teleofunction of a structure from any function it may have been selected for; it is the current role, and not the selected one, that matters. And, since the purpose of the relevant structure is internal to the relevant system(s), there is no ontological problem regarding their status – the conditions of being a teleofunction are real states of the system or systems involved, and the relevant causal activity must be internal to this system or the immediate environment and can be confirmed as such. Thus, the

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systematic account can handle the three different examples given as specific consequences of the ontological problem. Any ascription of content based on a systematic teleofunction cannot refer to items external to the system unless they are immediate and measurable environmental conditions (as opposed to, say, distant historical conditions) to determine the actual activity of the teleofunction itself. We may decide to adopt some externalist criteria in our semantics, but any such commitment cannot be part of what it means to be a teleofunction for the relevant structures involved. Similarly, since we've abandoned historical external determination of the teleofunctions, any feature of consciousness arising from or otherwise determined by teleofunctions will be determined by aspects that must be internal to the relevant system, since only these aspects constitute the teleofunction. Finally, there is no possibility of defective structures that were selected counting as having teleofunctions and therefore, on some accounts, as having value deserving some respect. To play a teleofunctional role such structures must support the system itself, rather than, as in the counter example discussed, serve ends or otherwise satisfy criteria of purposefulness that are wholly external to the relevant system.[3]

4.

I have argued that the historical etiological account proposes properties (radically external and unverifiable teleofunctions) that have ontological features a naturalist should shy away from; and that for this reason the systematic account, which lacks these problematic features, is a better explanation. But although superior to the historical etiological account, the systematic account of teleofunctions hides an important ontological distinction, and one which could give rise to its own difficulties. Schlosser recognizes that the structure or event *S* referred to in the schema must be a *type*. It is not hard to see why this is so. No individual sperm acts in such a way that it causes its own self-reproduction. Rather, for some species, their sperm is necessary for the production of other organisms, some of which in turn can create structures of that type (other sperm), or which can have offspring which can do so. But now we can see that there is a different ontological issue buried here, for there is a profound difference between at least two kinds of teleofunctions that are recognized in Christensen and Bickhard's account, or that can satisfy

Schlosser's schema. First, there are what Schlosser calls 'intra-generational functions.' These are individual structures which are in turn actually sustained, albeit indirectly, by their own activity. For example, a particular wolf's heart pumps blood. This in turn allows this wolf's lungs to aerate that blood, which in turn supports the functioning of the brain, the stomach, and so on, all of which in turn support this particular heart by supplying it with oxygen, nutrients, the appropriate neural inputs, and so on. Second, there are what Schlosser calls 'cross-generational functions'. One example is the already-mentioned sperm. Our particular wolf, assuming it is a male, has sperm that in no way can sustain themselves. Rather, a very tiny number of them (on average, a few more than two out of many millions) can create offspring which either will create sperm or may create offspring that will. In this case, what is reproduced are more instances of the kind.

Stated more fully, Schlosser's schema should read:

F is a teleofunction of structure or event type S at t_1 iff:
for a certain period of time T, and where $t_1 < t_2 < t_3$, and T ranges from before t_1 to after t_3,

(1) An instance of S, $s_1(t_1)$, is directly causally necessary to establish $F(t_2)$ under circumstances c_1,
(2) $F(t_2)$ is indirectly causally necessary to establish an instance of S, $s_2(t_3)$ under circumstances c_2,
(3) The causal relations between $s_1(t_1)$, $F(t_2)$, and $s_2(t_3)$ are complex.

This makes more clear that there can be two very different kinds of systematic teleofunctions. First, there can be cases where $s_1 = s_2$; these are the clearest cases of intra-generational instances. Second, there can be cases where s_1 and s_2 are different, and share only that they are of the same kind S; these are the clearest cases of cross-generational instances.

Thus, the systematic approach currently does not make an explicit and significant distinction between these two kinds of cases. Also, when the case of cross-generational instances do arise, the systematic account (restricted to the teleofunctions of structures) allows that there is a system or set of complex relationships that stretches across individual complex systems. Both of these moves are mistakes: the systematic account must make explicit distinctions between these kinds, and it should do so without referring to new (and, I will argue, problematic) kinds.

First, it is a mistake to obscure, by way of using a single general account, the difference between cross-generational and intra-genera-

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tional teleofunctions. The two situations are significantly different. A particular wolf's heart is an actual object, which bears through time a series of actual causal loops that constitute a self-sustaining relationship to itself. There is an enduring systematic relationship between actual individual structures, and what gets sustained is one of these structures. Here the term *systematic* teleofunctions has its clearest application, since these elements continue to be parts of one particular system. On the other hand, when a particular wolf's sperm makes possible another wolf which can produce sperm, the relationship is very different indeed. No particular sperm is sustained by the elements of the system(s) which that sperm sustains. Thus, not individual objects, but rather a series of instances of a type are produced. One might say that it is a kind or type, as opposed to an individual, which is 'sustained'. But a kind reproducing or sustaining itself is at most a metaphor for a particular reproducing another particular of its own kind. That is, we do not want to say that a kind is sustaining itself as a kind – there are no causal loops from a kind back to that kind, since there can be no causal connections to kinds, only to particulars.

If we return to some of the examples considered in Section II to illustrate concrete applications, then one can also make explicit the two distinct kinds of situations involved here. The examples of explaining content, and any value that may attach or depend on teleofunctions, are sufficient.

(1′) *Content*. In terms of representational content, a guiding concern in any explanatory theory is what kind of context is required to identify that content. We may have a rhesus monkey that is trained to perform some visual identification task, choosing different kinds of shapes over others to get a reward. It is a reasonable hypothesis that we should be able to explain the relevant representational content in terms internal to the monkey. Given the relevant and stable stimuli, we would refer to things like the patterns of activation in the visual cortex of the individual monkey in question. The relevant activity may then be wholly self-sustaining, or what Schlosser calls 'intragenerational'. However, it is easy to find examples which demand a wider context. A rhesus monkey raised in isolation can transmit by its emotional facial expressions significant information to other monkeys, and though these monkeys raised in isolation fail to recognize the meaning of the facial cues of other monkeys as well as do the monkeys raised in a social setting, these isolated monkeys still showed facial affective cues that other monkeys can recognize and properly

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understand (Miller, et al. 1971; see also Chevalier-Skolnikoff 1973). Presumably then the function of facial expressions in these organisms must be in part explained by reference to their social role, and full competence in using them must require some socialization, while it is also evident that some of the relevant structures are inherited. This means that a full explanation will need to refer to how some structures can have an inherited teleofunction which may not directly sustain the individual or the relevant individual structures with that capability, but also it will need to refer to structures that can be understood fully in internal terms. These are substantially different projects. In fact, they would be farmed out to different kinds of scientific disciplines, with different methods and subject matters. Given that the phenomena are at least in part dictating the differences in methods employed, should not the systematic teleofunction account respect these differences by clarifying what they are?

(3′) *Moral Value*. Similarly, if we ascribe like some biocentric individualists to the claim that the purposes of an organism deserve *prima facie* respect, we would still typically want to distinguish between purposes which served the organism in an immediate way, perhaps even being necessary to its own survival or at least to its flourishing, and purposes which were had by some population, perhaps even the whole species of the organism. Another way to make this point is to recognize that one of the most fundamental (and contentious!) issues in ethics regards our duties to future generations. Many ethicists believe that our duties to existing individuals outweigh our duties to future ones. Similarly, many ethicists would argue that there is substantial difference between, on the one hand, harm to me that reduces my lifespan, and, on the other hand, 'harm' to my offspring done by reducing my chances for reproduction. There is little consensus in ethics, of course, and so for any position on these issues there is disagreement. But, regardless of whether we adopt one or another position in this debate, *if the debate is coherent,* then *prima facie* we must grant some importance to the distinction between the sustaining, and the reproduction, of some systems or purposes.

Second, if we restrict the explanation of systematic teleofunctions to complex systems, as I have recommended, then the current systematic account must treat both cross-generational and intra-generational instances as each being parts of different kinds of systems. At first consideration, it may seem that this is beneficial for its perspicuity in specifying the teleofunctions in one general format. However, it would be more perspicuous, and less problematic, to instead pro-

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pose one kind of system with two accounts. As the account stands now, for an intra-generational teleofunction specific to one organism, such as the heart of a wolf, the system is the intuitively clear wolf itself. However, for something like the wolf's sperm, if our account is to refer to one system, then that system must be something like the species of the wolf or some other population of wolves. But although we understand that a wolf is a complex system, the sense in which the species or some population is a complex system of the appropriate kind is much more difficult to discern. These systems are very distinct. The individual wolf is a relatively stable system sustained over time. It has a number of clear bounds, such as its skin. Also, it can reproduce other particular organisms which are like it in important ways. These are all features which are examples of an autonomous system. The species or some other population, instead, does not reproduce, but changes over time. It has no clear boundaries. Its form is very indistinct, and is likely determined not (as is an individual organism) by a stable internal form but rather by the similarity of its members. These differences are problematic for the normative aspect of systematic teleofunctions, which arises because the teleofunctions are required to sustain the relevant system. This is quite clear in the case of an individual organism, where some structure performs some teleofunction that contributes to the continuing existence of the complex system that it in part composes. But this not clear for a system that stretches across some population. The population is loosely defined, with no clear boundaries, and can be divided, or can be melded with other populations. What are the dependencies which create (hypothetical) norms, and what is the thing that is being sustained? There is an analog with evolutionary theory here: there may be biological functions which serve a population, even a species, but they are difficult to discern and remain controversial. The biological functions devoted to producing similar offspring, however, are relatively uncontroversial.

These differences are not sufficiently explored in the systematic account. If we change the schema to refer to a single complex system, then we are forced to confront this new ontological challenge since we must either posit several systems or one system that stretches across organisms. Without controversy, we can say that an organism is a complex system of the relevant kind, what Christensen and Bickhard call an autonomous system; but can we say the same of a species? Christensen and Bickhard are explicitly committed to this: 'living organisms in general are autonomous systems, as are repro-

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ductive lineages, species, and some kinds of biological communities' (2002, 3). Christensen and Bickhard see self-sustenance and self-reproduction as just two ways for processes to perpetuate themselves.[4]

We need to recognize explicitly at least two different kinds of cases of systematic teleofunctions; but, if we are to treat those teleofunctions that stretch across organisms as belonging to one system, a new ontological challenge arises. We need, at the very least, an account of how these very different kinds of cross-organism systems are to be specified, what makes them a system, how they are similar to organisms themselves, in what sense they can sustain themselves in any other way than producing more individuals, and so on. However, although a species or a population may be in some senses a complex system that shares some interesting features with individual organisms, it is not necessary to refer to such a mysterious system to develop a more satisfactory systematic account of teleofunctions. We can make explicit the difference between these two kinds of teleofunctions while adopting the more perspicuous approach of positing only one kind of complex system (in the biological cases, single individual organisms).

What needs to be identified, therefore, are two different kinds of systematic teleofunctions, without reference to new kinds of complex systems constituted by populations of organisms. To draw attention to, and to clarify, this difference, I suggest an alternative terminology: self-sustaining, and self-reproducing, teleofunctions. Revising Schlosser's schema, self-sustaining teleofunctions would then be defined as:

> F is a *self-sustaining teleofunction* of structure type S[5] iff:
> for a certain period of time T, and where $t_1 < t_2 < t_3$, and T ranges from before t_1 to after t_3,
> (1) There is an instance of S, $s(t_1)$, directly causally necessary to establish $F(t_2)$ under circumstances c_1.
> (2) $F(t_2)$ is indirectly causally necessary to sustain $s(t_3)$ under circumstances c_2.
> (3) The causal relations between $s(t_1)$, $F(t_2)$, and $s(t_3)$ are part of a single complex system.

And self-reproducing teleofunctions would be defined as

> F is a *self-reproducing teleofunction* of structure type S iff:
> for a certain period of time T, and where $t_1 < t_2 < t_3$; and where T ranges from before t_1 to after t_3,
> (1) There is an instance of S, $s_1(t_1)$, that is directly causally necessary to establish $F(t_2)$ under circumstances c_1.

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(2) $F(t_2)$ is indirectly causally necessary to establish some instance of S, $s_2(t_3)$ under circumstances c_2.
(3) The causal relations between $s_1(t_1)$, $F(t_2)$, and $s_2(t_3)$ are part of at least one complex system; if they are part of more than one complex system, then these are of the same type of complex system.
(4) It is not the case that $s_1{=}s_2$.

Here "complex system" in the case of biological organisms will refer to individual organisms; there is no reference to the species or any other population as a complex system. As already noted, this is not to deny that a population may be a kind of complex system, but rather to recognize that it is not necessary to include such a system in order to identify the kinds of self-reproducing teleofunctions under consideration here. Structure types are identified by reference to the type of complex system involved (e.g., heart of *C. lupus*). The note in clause (3) that there may be just one complex system involved recognizes that there could be self-reproducing teleofunctions within one system. Clause (4) is here to make explicit that the relevant relations are not self-sustaining – no one structure is sustaining itself in this description.

5.

I conclude that adopting these two accounts, and therefore positing two kinds, of systematic teleofunctions avoids the issues raised for explaining the cross-generational kinds of teleofunctions. Call such a systematic account a *splitting account,* in contrast to the kind of *unified account* in Christensen and Bickhard or Schlosser; my contention is that a splitting account is a better explanation than is a unified account. However, it is important to note that this is not an argument that the unified accounts are demonstrably false. Furthermore, my account limits teleofunctions to structures, and it requires that the complex relationships in question are specified in reference to a complex system, the best examples of which are organisms. Easing either of these limitations suggests alternative ways that a unified systematic account may be developed. It is necessary then to elaborate my arguments against using in the account either events or complex relationships (that may not be complex systems). This also provides an opportunity to rephrase the argument for the splitting account in the form of a challenge to unified accounts.

First, if we allow that events may have teleofunctions, we might cover all or almost all cases of teleofunctions with an account akin to

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the cross-generational account as presented here[6] (one can read Christensen and Bickhard in this way). An event instance of the wolf's heart beat has the teleofunction of pumping blood, and this would be because that beat, through a complex chain of events, ends up being necessary for some later heart beat. This way of describing the situation satisfies Schlosser's schema by way of granting to the heart beats of an individual organism the property of being cross-generational teleofunctions. The story is about types all the way through, and there is no obvious difference between the details of this case and a cross-generational one where we are concerned with several organisms (as in the case of sperm). Thus, we appear to avoid the problematic nature of referring to species (or other kinds of complex systems) as complex systems by focussing instead on events alone.

Second, if like Schlosser one admits not systems but just any complex web of self-reproduction as defining the teleofunction, there may be no substantial difference in kind between such webs within and across organisms. Thus, we seem to avoid the problematic issues by not admitting species or other kinds-as-systems into the account. There is just one relevant kind, stretching perhaps across individual organisms.

It is my contention that in either of these cases, the unified account depends implicitly on the structures of individual organisms. In the first approach, we can specify the relevant event kinds in either the cross-generation or intra-generation cases only with reference to particular structures, which in turn we specify only in reference to individual complex systems (organisms). I start by having identified the organism(s) in question as the phenomenon under observation; then I identify its organs, such as a heart; I observe the heart is pumping blood; I trace out some of the relationships of this structure to others in the organism, including especially the relationships that depend upon it pumping blood; and thereby identify causal loops within the organism. Only then can I identify the role of an individual heart beat. The individual heart beat event is necessarily individuated in terms of a structure (this heart), which is in turn individuated in terms of the complex system it in part constitutes (this particular organism, of kind *C. lupus*). Once I have done all this, I have enough information to identify chains of events and to begin to develop the cross-generation event account.

In the second approach, we can specify complex relationships only by picking out events or structures which are related in certain ways.

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But first picking out these structures or events requires us then to pick out individual complex systems or kinds of complex systems (organisms or organism kinds). If I want to say that the teleofunction of this particular sperm is to inseminate an egg, and we can see this because it in part results in the reproduction of other sperm of the same kind (in a male heir), I need to identify the sperm in question, the other structures with which it interacts (e.g., an egg), and the later instances of the same kind that it may produce. I will do all of these things by identifying structures (including sperm and eggs), and these in turn are identified in reference to the kind of organism. The type of organism also helps me to identify the relevant cases of later instances of a same kind (two sperm are of the same kind if they are sperm of the same kind of wolf). The complex relationships that might constitute a teleofunction cannot be specified without reference to structures which have the relevant capabilities, and these in turn are specified in relation to the structures they in part compose, ultimately the complex systems themselves.

In both cases, I begin by identifying complex systems (organisms), then identify their constituent structures (organs) and their activities, and how these activities form networks of dependencies between them and throughout the organism. If I aim to use an event ontology, this allows me to add to my account the events that these structures enable or cause, and then I lay down an event ontology based upon these distinctions. If I aim to identify complex relations which are simpler than or not bounded to any one complex system, this allows me to add to my account a specification of some network of activities and dependencies which may be other than the organism. But in either approach, *I have done all the work required to satisfy the splitting account before I can complete a unified account*. That is, these unified account alternatives depends upon, *and build on top of*, all the distinctions that are sufficient to make a structure-based splitting account. If we have identified complex systems, identified the structures in them and their activities, and mapped out interdependencies between structures in terms of those activities and under certain identified conditions, I have all the elements required to satisfy either one or the other definition of teleofunctions as given in the splitting account. To add to it a description of a series of events, or a description of a complex set of relationships, is unnecessary.

Since the splitting account I describe is sufficient, and is also satisfied by the implicit work that goes into preparing the way for either of the unproblematic forms of the unified account, Ockham's razor

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cuts in favor of the splitting account alone. This then poses a challenge to the unified account: a viable unified account must explain how we are to identify events or complex relations without reference to individual organisms and their structures; or, it must explain what benefits the account has which make it preferable to the simpler splitting account. If the unified account cannot do either of these, then the splitting account is preferable both because it is simpler and because it is already implicitly satisfied in the preliminary work required to develop a unified account.

6.

I have described and solved two kinds of ontological problems for explaining teleofunctions. I have argued that each of these problems can arise when the goal to provide an acceptable description of teleofunctions (where no term refers to a kind a naturalist would refuse) may incline us to overlook other naturalist concerns (such as, is the property described of a kind that a naturalist should admit into her ontology?). In the case of the historical etiological account, this led to a theory which allows properties which may lack the appropriate causal relations, and which may be determined by completely unverifiable properties. In the case of the unified systematic account, the problem was that the unity of a single account required the introduction of a controversial kind of system, species or populations; or it already presupposed everything needed for a splitting account before it could be constructed. The systematic account is a better explanation for avoiding both of the problems of the historical etiological account, and when revised into two explicit forms based on a single kind of complex system (individual organisms), the account provides an explanation of teleofunctions of great explanatory power which avoids the pitfalls of the alternative systematic accounts or is simpler than them.

Many kinds of explanations will require reference to both kinds of systematic teleofunctions. As noted in the case of the monkey affective facial expressions, where an inherited signaling systems and also some important learning component are apparently involved, one can predict that there are interacting combinations of self-sustaining and self-reproducing teleofunctions. A complete scientific account may need to refer to both kinds in explaining this one group of phenomena. By recognizing at least two kinds of systematic teleofunc-

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tions, we gain in realism and power of explanation what we relinquish in apparent unity.

As an explanation of the kinds of things that teleofunctions are, the systematic account has been shown to be a better explanation than the historical etiological account. However, it is important to note in closing that significant challenges remain for the systematic approach. The historical etiological account has been used to develop compelling accounts of a range of semantic properties, including meaning and truth (Millikan 1984, 1993; Price 2001). In these accounts, the externalism of the historical etiological account can be made into a virtue, by potentially providing concise (even if unverifiable) determination of the role of some structure. Ultimately, if we are to replace the historical etiological account with a systematic account (we might, of course, chose to keep both, but for different purposes), a full defense of the claim that the systematic account is preferably will require a demonstration that it is equally powerful where required. Semantics is the most likely and important candidate for such a requirement, since representations and other semantic properties are normative and thus may be explained by a teleofunctional explanation. This will mean that defenders of a systematic account must either show that semantics does not need much of the work that the historical etiological account can do for it, or show that the systematic account can do as much. Either project is a substantial task for future research.[7]

NOTES

[1] Granting that Fodor identifies a problem with the kind of unreserved externalism that is seen in Millikan does not then mean that one must endorse methodological solipsism. It is plausible that the systematic account of teleofunctions developed below could allow that some structure may have causal-historical reference as a teleofunction; but the referent or refential content in such a case could not explain the actual activity of the teleofunctional structure.

[2] An obvious response is to say that teleofunctions must arise naturally, but the division between artificial as opposed to natural teleofunctions in these cases is untenable. For example, humans sometimes choose their mate on the basis in part of inherited features (e.g., an appearance judged to be healthy), but I take it that we would not then deny their offspring some teleofunctions because the relevant structural features were chosen. For a consideration of this and other objections see (DeLancey 2004).

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[3] This is not to deny that a system may have teleofunctions which can sometimes conflict, and thus under some conditions a structure that is otherwise beneficial could be dysfunctional.

[4] Conflating or ignoring the difference between these kinds of systems would also foster a pending problem for any future attempts to explain the nature of life and autonomy, since these systems are different but have important interrelationships. For example, following von Neumann's dream to develop a mathematical theory of life, Gregory Chaitin has used his developments in complexity theory to suggest that organisms are systems that have more compact descriptions as a whole than do the sum of their parts (1970, 1979). Suppose this suggestive hypothesis turns out to be right; can the same be said of some population or the species? This open question should remind us that in terms of *prima facie* observation – basic phenomenology, one might say – we have reason to suspect that the two kinds of things are very distinct.

[5] My focus has been on individual structures. We could specify these schema as identifying the teleofunction F of individual structure s_1, and recognize that we say structure type S has the teleofunction F as a generalization over some instances. However, here Schlosser's restriction to conditions c_1 and c_2 does double duty, and is sufficient to allow reference to the purpose of a type of structure, since presumably once we specify the same conditions, the same kind of structure is going to perform the same kinds of actions.

[6] I am indebted to an anonymous reviewer for this observation.

[7] I am indebted to two anonymous reviewers for *Synthese* for insightful comments.

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