Functions: Consensus Without Unity

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1. Twenty Years.

The year 1993 marked the 20th anniversary of the publication of Larry Wright's article "Functions," (1973), an article which decisively re-oriented the functions debate.

Wright's article did not answer all the questions philosophers have asked about functions, but it did answer some of them, and it showed the way forward to answering more. Much of the literature since 1973 has, in effect, engaged in the refinement of Wright's original idea. Many writers do not think of themselves as doing this; indeed, several have actively resisted this interpretation.[1] Nonetheless, since 1973 there has been a convergence towards a view of functions which has Wright's idea at its core.[2] I think of this trend as an example of real progress in philosophy.

In this paper I will sketch what I see as the view towards which the literature is converging. One feature of the theory which should reasonably be regarded as controversial is a bifurcation within it. On my view, functions as analyzed by Wright and functions as analyzed by Robert Cummins are both real, and important, and distinct. Philip Kitcher has argued recently that that the concept of *design* can unify these two conceptions of function (forthcoming). I will resist this move towards unification. Though some will find a bifurcation unattractive, unity is not always a good thing.

2. Wright's Two Advances.

Wright said:

The function of *X* is *Z* *means*

(a) *X* is there because it does *Z*,

(b) *Z* is a consequence (or result) of *X*'s being there. (1976 p.81)

The most striking thing about this formula is its simplicity. Through the 1960's philosophers became accustomed to long and intricate definitions of functions -- at least six lines long and four variables deep. And whether or not the biological phenomenon known as Cope's Rule is generally true in nature, it is admirably illustrated by most philosophical lineages: definitions of a given concept get physically bigger through time, not smaller. Yet Wright's definition of function was shorter than its predecessors. This poses a small puzzle in the history of philosophy: why, given all that had gone before, was it possible to defend a two-line theory at that point?

Earlier analyses of functions were driven in large part by general assumptions made about explanation. For writers such as Hempel, a functional explanation has to explain the presence of the functionally characterized entity, and the explanation has to conform to something like the D-N model. The D-N or deductive-nomological model of explanation, which was dominant through the 1960's, understands explanations as inferences. An explanans is a set of premises, including a law of some kind, which confers either deductive certainty (or, for I-S explanations, high probability) on the explanandum (Hempel and Oppenheim 1948). So if functional explanation was to be genuine, citing the function of the heart, for instance, had to *imply* the existence of hearts, given some other premises about the containing system. But though the heart's function is pumping blood, and people need blood to be circulated, it is not possible to infer the existence of hearts from their blood-pumping ability, and no reasonable amount of fine-tuning the envisaged argument will make this so.

Wright dismisses this conception of explanation, both in general and as applied to functions (this is most clear in his 1976 discussion). Although it is not presented in this way, Wright's conception of explanation is related to ideas developed in detail by writers such as Wesley Salmon (1984) and Bas van Fraassen (1980). An explanation cites factors which rule out or make less probable certain alternative events to the explanandum. Which ones are to be ruled out depends on the context in which the explanation is offered. It is not necessary to rule out *all* alternatives. Wright's analysis of functions is made against a background of a liberal conception of explanation. Once we have this conception of explanation, it is clear that sometimes we can explain the presence or persistence of entities by citing certain of their effects or dispositions. Whenever this is possible, these effects are those entities' functions. So one of the advances in Wright's analysis is an instance or an application of general progress made around that time with respect to explanation.

The other step forward in Wright's analysis does not have to do with philosophical currents outside the functions industry. Wright's analysis is driven in large part by constant attention to what he calls the "function/accident distinction." When attending to this distinction, one insists that there is a definite difference between a function and a fortuitous benefit. Something can have beneficial effects, or make a useful contribution to a containing system, but these are not functions unless the thing in question is there *because* of these effects. Otherwise they are accidental, fortuitous benefits. This is the manoever in Wright which disposes of the whole range of analyses of functions based upon contributions to goals. A mere contribution to a goal is not a function unless it is not fortuitous, unless this contribution explains why the thing is there. But this requirement of explanatory salience is apparently now bearing the whole weight of the concept of function, and goals drop out of the picture. In some respects Wright here does for philosophy what G.C. Williams did for biologists in his *Adaptation and Natural Selection* (1966): motivate vigilant attention to the difference between fortuitous benefit and genuine adaptation.

3. A Consensus View.

The simplicity of Wright's analysis was also intended to reflect the ease of application characteristic of the concept of function. But subsequent discussion has indicated that the schema he proposed was left too simple, or made excessive demands on contextual factors. I will run through a sequence of objections and modifications, which are designed specifically to improve the theory's analysis of functional discourse in biology.

3.1: Lineages.

Here is a counterexample modified from some used by Boorse (1976). Consider a small rock holding up a larger rock in a fast-moving stream. If the small rock did not support the larger rock, it would be washed away. Holding up the big rock is the thing the small rock does, that explains why it is there. So on Wright's original analysis this is the function of the small rock.

In Ruth Millikan's analysis of biological functions this type of problem is immediately avoided, by restricting the entities to which functions are ascribed to those which exist within lineages defined by relations of reproduction or replication (1984). Very roughly, the function of something is whatever past tokens of that reproductively-defined type did that explains the existence of present tokens. In Millikan's account it is also explictly required that the explanation make reference to a selection process.

These modifications deal effectively with many otherwise troubling counterexamples, such as Boorse's. However, it is important to note a less attractive consequence of explicitly building them into the analysis. Once we make an appeal to lineages defined by reproduction we begin to lose the *generality* of Wright's view. For example, if we do not build these restrictions into the analysis, then the concept of function used in Dretske's recent work on meaning and explanation (1988) can be understood as Wright's concept in a different setting. Dretske says an inner state C can have the function to indicate an external condition F if C has been recruited as cause of some motion M because it indicates F. This is easily understood as an instance of Wright's basic formula: the thing C does that explains why it is where it is, why it has been recruited, is indicating F. C *qua cause of M* is there because it indicates F. So indicating F is C's function (see also Godfrey-Smith 1992). Thus Dretske's work, too, can be seen as part of the unacknowledged consensus.

However, we can only understand Dretske's concept as an instance of Wright's general view if we do not build into the analysis an explicit appeal to reproduction or replication. Dretske is most interested in "recruitment" of inner indicators that results from individual learning, and in these cases it is very hard to see C as a member of a reproductively defined lineage whose earlier members indicated F and were recruited for this reason. Dretske's view fits the basic Wright formula, not the Millikan-style modifications. This is not to say that the modified concept of function has no applications in philosophy of mind -- these applications are much of Millikan's motivation for developing the concept. But the use of biological or "teleonomic" concepts of function in philosophy of mind is made more complex when Wright's formula is augmented in this way. On the other hand, if we do not build in these additional requirements we have a harder time with counterexamples such as Boorse's. In this discussion I will assume that an explicit appeal to selection processes and reproductively defined lineages is appropriate.

3.2: Past and Present.

Another set of problems derive from the facts of biological usage. In biological discussion it is common to make an explicit distinction between "evolutionary" and "functional" explanations for a trait. Tinbergen is often cited for this, especially by behavioral biologists.[3] But on Wright's analysis of functions this distinction should not exist. Horan (1989) appeals to this fact about biological usage to motivate a selection-based account of functions which is forward-looking rather than backward-looking. The best forward-looking theory I know is the propensity theory of Bigelow and Pargetter (1987): functions do not derive from a past history of selection, but from present propensities to succeed under selection.

My view is that looking forward is a mistake; it is better to look backward in a slightly different way. Functions can be seen as effects of a trait which have led to its maintenance during *recent* episodes of natural selection. The distinction between "functional" and "evolutionary" explanations can be cast as a distinction between the explanation for the original establishment of the trait, and the explanation, which may be different, for its recent maintenance (Godfrey-Smith forthcoming). Thus we can make sense of biological usage while retaining the idea that in giving a function we are, ipso facto, giving an explanation for why the functionally characterized thing exists now.

3.3: Cummins Functions.

Once a modified version of Wright's theory is in place, the explanatory role of many function statements in fields like behavioral ecology is clear. But there remain entire realms of functional discourse, in fields such as biochemistry, developmental biology and much of the neurosciences, which are hard to fit into this mold, as functional claims in these fields often appear to make no reference to evolution or selection. These are areas in which the attractive account of functions has always been that of Robert Cummins (1975). On Cummins analysis, functions are not effects which explain why something is there, but effects which contribute to the explanation of more complex capacities and dispositions of a containing system.

Although it is not always appreciated, the distinction between function and *mal*function can be made within Cummins' framework, as well as within Wright's. If a token of a component of a system is not able to do whatever it is that other tokens do, that plays a distinguished role in the explanation of the capacities of the broader system, then that token component is malfunctional. The concept of malfunction is context-dependent on Cummins' view, just as the concept of function in general is.

My view of this issue derives from Millikan (1989b). We should accept both senses of function, and keep them strictly distinct. All attempts to make one concept of function work equally for behavioral ecology and physiology are misguided. On this view, "Wright functions" and "Cummins functions" are both effects which are distinguished by their explanatory importance. The difference is in the type of explanation. So if it is claimed, for instance, that the function of the myelin sheaths round some brain cells is to make possible the efficient conduction of signals over long distances, it may not be obvious which explanatory project is involved. This may be intended as an explanation of why the myelin is there, or it could be part of an explanation of how the brain manages to perform certain complex tasks. Sometimes the same assignment of functions will be made from both perspectives, but this does not mean the questions are the same.

I conjecture that it has often been the suspicion that there must be underlying unity between function ascriptions in diverse fields, that has led to people holding back from accepting that Wright found the key to understanding the most philosophically troublesome concept of function. I realize that many people will find a fused or unified concept of function more attractive; they will prefer an account on which it is at least clearer why diverse biological discourses use the same word, "function." I will spend the rest of this paper criticizing this longing for unity.

4. False Unity.

A view of functions which has many ideas in common with the view I am defending, but which holds out for more unity, is defended skillfully by Philip Kitcher in "Function and Design" (forthcoming).

On Kitcher's view, different modes of functional characterization are unified by the concept of "design," where human intention and natural selection are equally sources of design. Kitcher claims that all biological attributions of function take place in a context characterized by design. But design can be relevant to attributions of function in more and less direct ways. One way is the way analyzed by Wright: we can explain the presence of some component of a system in terms of what it does, in terms of a selective history. This is a "direct" case.

There are also explanations which appeal to design more indirectly. We can consider an organic system which is, overall, the product of design, and then examine how its workings relate to "demands" made by the environment. If some part of a system is a "response to an identifiable selection pressure" (p.16ms), then it has a function whether we believe that component is itself the product of selection, or not. The origins of the component, the reason why that particular part is there, do not enter into it.

The explanatory project in which such functions are used is similar to that of Cummins. The aim is to understand how the component plays a role in the system's dealing with its environment. So these functions are a subset of Cummins functions as originally understood -- in many ways a core subset. According to Kitcher, whenever Cummins-style functional analysis is really done, there is a "souce of design in the background" (p.18ms). In a science such as physiology, "Selection furnishes a context in which the overall design is considered, and, within that context, the physiologist tries to understand how the system works" (p.24ms).

I agree that many aspects of biological usage in areas at some remove from evolution are accurately described by this analysis. This seems to me to be about as good as a unified theory of functional discourse in biology can be. But I do not think it is right. Let us focus more closely on cases where design plays an "indirect" role, in particular on the crucial cases where a part of a system makes a contribution to the systems' dealing with its environment without being itself the product of selection.

There are, roughly speaking, two sources for traits of organisms which fall into this second category. The sources are *chance* and *constraint*. Kitcher's two explicit examples of traits which are part of a response to an environment's demands, but which do not have a Wright-style selective history, both involve chance. In one example, similar to examples discussed in debates over Wright's analysis, a screw falls into a machine and by chance makes an essential connection between two parts. The designer of the machine did not realize this connection was necessary, so without the luckily falling screw, the machine would not work. Kitcher says the screw has the function of making that connection.

Kitcher also discusses a biological case, in which a moth has a wing pattern that provides some camoflage from predatory birds, but which is inferior to other patterns. We know the superior patterns are genetically possible alternatives because they are seen in low frequency in some areas. But, we discover, the superior rival patterns have never taken over the population because of a range of unlucky breaks. The better mutants have tended to arise in areas where predation is especially heavy, and so on.[4] Kitcher says that even when we find out that the camoflage pattern does not have a pure Wright-style history in this way, it is still natural to say that the pattern has the function of camoflaging the moths from predatory birds. This is still a contribution the pattern makes to the organism's response to environmental demands.[5]

Now let us look at how Kitcher's proposal handles a case from another family of unselected organic properties, properties due to constraint.

In Richard Levins's classic (1968) discussion of evolution in changing environments he claims that the following pattern is common in invertebrates: high temperatures speed up development (as long as the temperatures are not so high they simply break the system) and the final result is a smaller adult body size. This seems to be a physiologically inevitable consequence of at least many invertebrate metabolic systems. This fact has some interesting consequences. Consider the situation of some different types of fruit fly. Suppose first, as is reasonable, that the adaptive significance of size in fruit flies has much to do with avoiding desiccation, the loss of moisture. When it is dry, you need to be somewhat bigger than normal to avoid drying out. Then it is possible for the basic facts about temperature and metabolism to either work for the fly or against it, depending on the structure of the environment.

First, suppose that the hot areas in the fly's habitat also tend to be the humid ones, and the cool ones are the dry ones. Then physiological inevitability works in the fly's favor. Whichever way the metabolism is fine-tuned, it will always be the case that when it is dry, the fly will wind up larger than it will when it is humid, and this is just what it needs. The fly gets a certain kind of developmental plasticity for free; there is a pre-established harmony between its metabolic properties and the environment.

On the other hand, if the hot areas are also the dry ones, and the cool areas are humid, then the basic facts of metabolism work *against* the fly. When it is hot and dry, and the fly needs to be larger, it will wind up small. Levins discusses two actual species of fly, which exemplify these alternatives. Flies from a Middle East population enjoy the pre-established harmony, as there humidity is correlated with heat. Around Puerto Rico though, the dry areas are the warmer areas, and the flies must deal with a natural antipathy between the facts of development and environment.[6]

For now let us focus on the situation of the lucky flies, the flies whose metabolism makes them big when it is good to be big and smaller otherwise. On Kitcher's view of functions, as far as I can see, the physiological facts about enzymes and reaction rates that bring about this relationship have the *function* to adjust the flies' size to spatial variation in their environment. These physiological properties are properties of a system which is the product of "design," and these properties are part of the way the fly deals with variable aspects of the environment. There is an identifiable environmental demand here, a selection pressure which cannot be evaded or sidestepped without large changes to the fly's basic architecture. The fly's biochemical properties provide a "response" to this pressure, in that they are properties that produce phenotypic plasticity in the flies which enables them to deal with this environmental demand. However, these metabolic properties are entirely inevitable, given the general structure of the fly's physiology. They are the product of architectural constraint, and the fact that they work for the fly's benefit is simply a stroke of luck. Elsewhere they make the fly's life even harder.

So, I claim, a theory of biological functions which has anything to do with concepts of "design", a theory which is not explicitly as liberal as Cummins', should not recognize a case such as this as functional. The basic biochemical properties which cause the flies to change adult size with temperature do not have the function of altering the fly's size to deal with the problem of moisture loss. I am not saying this simply because these biochemical properties of the fly are not *always* useful. That is the case with many truly functional properties. I claim this is not a functional property because it is physiologically inevitable; it is the product of constraint.

It might be objected that so far I have just emitted some Wright-style intuitions, the intuition that an effect is not a function if it does not explain why the thing is there. We brace for what Bigelow and Pargetter called "the dull thud of conflicting intuitions" (1987). So I will try to justify these claims with some more theoretical considerations.

On the view I am presenting, the functions literature is heading towards a view in which the analysis of functional discourse is bifurcated, and Wright-functions and Cummins-functions are both recognized. The recognition of this disunity is itself progressive. The concept of function was bequeathed to post-Darwinian science, from an earlier conceptual scheme. The original concept of function probably did have a close connection to the concept of design, and was (for all I know) a fairly unified concept.

But the categories we recognize now should be determined, of course, by our own world view. The analyses of Wright and Cummins locate functional attribution within two distinct explanatory modes which are legitimate parts of our contemporary world view. Natural and artificial selection exist, and the attributes of various things can be explained in terms of selective histories. Complex, organized systems also exist, and have global capacities which may be explained in terms of the capacities of component parts. These are two legitimate explanatory modes within the sciences. Crucially for us, these are two *different* explanatory modes within science. There is not some single explanatory project, distinct from others, which encompasses these two modes. They are two different kinds of understanding we can have of a system. This is why I view Kitcher's proposal as offering a false unity, a unity which should be resisted in the interests of maintaining an accurate understanding of different explanatory strategies in the sciences.

I would like to approach this point from several different directions. Kitcher claims that every time Cummins-style functional characterization is (seriously) done, there is "a source of design in the background" (p.18). My point is that even if this is true, this should not be respected by a philosophical analysis of functions. It should not be respected because there is nothing scientifically special about contributions to capacities, *qua* contributions to capacities, in systems which are the product of design -- as opposed to contributions to capacities in systems which are not the product of design. This is not to say that there are not *some* differences between capacities of components of systems that are the product of design, and capacities of components of systems that are not. Components of systems which are the product of designs are often themselves the products of design -- products of selection, at least. That is to say, the components of these systems often have *Wright*-functions; they are there because of the effects and capacities they have. But this is an additional fact, over and above the mere fact that the component is within a system which is the product of selection. Part of the point of Wright's analysis is to stress the fact that there is a real difference between being a part of a certain kind of system and making a useful contribution to its working, on the one hand, and being in that system *because* of this useful contribution, on the other.

To put the point yet another way: Kitcher discusses the example of a contribution made by a chance arrangement of rocks to the structure of a river delta downstream. He says that on Cummins' original analysis these rocks can have the function of widening the delta, given the right specification of the system and so on. Kitcher says this is an inappropriate consequence for Cummins' view to have, and this problem is solved by restricting Cummins-style functional analysis to systems which are the products of design. My point is that even if this is intuitive, and even if it re-unifies the concept of function, it should be resisted by the philosopher of science. A contribution to a system has the same real status, *qua* contribution made to a system, whether the system is a river and its surrounds or the intricacies of human vision. The difference between the two systems is that the components of the visual system have Wright functions as well.

Let us also return briefly to Levins' lucky flies. The facts of biochemistry have a Cummins function in these flies. They make a contribution to the capacities and dispositions of the fly when confronted with a variable environment. However, they have this Cummins function when the flies are in an environment where the biochemical facts work for them and also where the biochemical facts work against them. Whether the fly is lucky or unlucky makes no difference; the biochemistry has effects on the system either way. On Kitcher's view the only case in which these effects are functions is the case in which the effects are beneficial, and help the organism meet the "demands" of the environment. The problem here is not that this marks a distinction without a difference -- in one case the biochemical facts are good and in the other they are bad; that is a real difference. The point is that attention to this difference, in this context, distorts our understanding of these systems. Kitcher's view assimilates the properties of the biochemistry of the lucky flies to those properties of the fly which have genuine Wright functions. But the lucky flies exhibit *bogus* design in this case; theirs is in no real sense a "response" to the environment. Thus the important distinction between selected effects and fortuitous benefits is blurred.

Once Cummins functions are recognized and understood within familiar cases, which concern systems which are complex and highly adapted, such as the nervous system, a question arises concerning the links between these cases and more peripheral ones. Fairly peripheral cases include some seen in community ecology, where the function of a predator may be to regulate the numbers of some other species. Here we have already left the domain in which systems' components have Wright functions as well, on standard conceptions of evolution. Then there are extremely peripheral cases, such as the rock and the river delta. My proposal, which I think is in line with Cummins' original attitude (1975 p.764), is that once Cummins functions have been recognized and the explanatory mode which utilises them has been understood, they should be allowed to roam freely, even into the farthest periphery.

Kitcher discusses a case where Cummins functions can be attributed, and which is not peripheral or 'stretched' by Cummins' own criteria, but in which some may want to resist functional attribution of any kind. This is the case of the contribution made by some particular mutant DNA sequence in the development of a tumor. Because the DNA sequence goes wrong in some particular way, the cancer as a whole has certain properties. It is not, Kitcher says, the function of these aspects of the mutation to produce certain characteristics in the cancer. On the view I have presented, we have to say that this is a case where components of the system have both Wright functions and Cummins functions, and some of the Cummins functions -- those determined by our explantory interest in the cancer -- are opposed to the Wright functions. The Wright functions of this stretch of DNA have to do (we suppose) with regulating cell division in a particular way, which keeps the number of cells of this type at a certain level. When the mutation produces a tumor, and this tumor becomes the subject of a certain sort of investigation, the Cummins function of this bit of DNA, relative to that investigation, is a Wright *mal*function. On Kitcher's view the only functions here are those stemming from the design properties of the system. In no sense are the causally salient effects of the cancer-causing mutation regarded as functions, even if they are part of a complex system which we want to understand. I recognize the intuitive appeal in Kitcher's view here, and this must be weighed against the arguments I have presented for the disunified view. The most important of these arguments, again, concern the need to recognise the real difference between the two modes of scientific understanding in which Wright functions and Cummins functions play a role.

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

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Notes

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[1] Bigelow and Pargetter 1987 and Millikan 1989a are examples.

[2] Works contributing to the consensus which are not discussed elsewhere in this paper include Neander 1991, Brandon 1990, Mitchell 1989, Sober 1984, and Griffiths forthcoming.

[3] Tinbergen (1963) acknowledges Julian Huxley. Mayr 1961 is another early source.

[4] See Brandon 1990 for detailed discussion of cases where environmental diversity contributes to the outcome of selection in this type of way. Some versions of this situation are cases of Simpson's paradox (Cartwright 1979).

[5] On my version of Wright's view, and probably on Wright's, this pattern does have a Wright function in any case, as long as some significant (contextually determined) range of

alternatives *were* beaten out via selection. This is a consequence of the general liberalism about explanation which goes with Wright's view. This move does not trivialize the theory; there has to actually be a range of alternatives beaten out, and whether a given range is a "significant" range is determined by the general standards applicable for causal explanation.

[6] The differences between flies in these two situations show up in interesting ways when flies from warm and cool areas are raised at a single temperature in the laboratory (Levins 1968 pp.65-69).