Function, Fitness and Disposition[1]

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ABSTRACT: In this paper I discuss recent debates concerning etiological theories of functions. I defend an etiological theory against two criticisms, namely the ability to account for malfunction, and the problem of structural "doubles". I then consider the arguments provided by Bigelow and Pargetter (1987) for a more "forward looking" account of functions as propensities or dispositions. I argue that their approach fails to address the explanatory problematic for which etiological theories were developed.

KEY WORDS: Function, fitness, dispositions, explanation.

There has been a recent resurgence of interest in functional explanation, both in the details of the analysis of functions and in the appropriate strategy for carrying out such an analysis (Millikan 1984, 1989; Bigelow and Pargetter 1987; Mitchell 1989, 1993; Horan 1989; Neander 1991; Godfrey-Smith, 1994). Some, including myself, have defended and developed an etiological theory of proper functions whose origin derives, in part, from the work of Larry Wright (1973, 1976). On this theory, a function is a consequence of some component of a system (the pumping of blood consequence of the human heart, the inclusive fitness maximizing effect of the avunculate in societies with low paternity certainty, the keeping open of the door consequence of the brick on the floor). The consequence to be a function, must have played an essential role in the causal history issuing in the presence of that very component. While agreeing on the basic character of functions, the defenders of etiological theories nevertheless disagree with respect to the nature of their enterprise, that is, whether it is an act of conceptual analysis or one of theoretical definition. Clearly the type of argument available to claim success for the etiological theory, and its right to prevail over other contenders in the field depends on what such accounts aim to do. Prior to entering some new battlegrounds in the war between function theories, I will first address this meta-analytic question. I will argue that current disputes may be better understood, if not dissolved, by understanding the explanatory enterprises in which appeal is made to functional ascriptions. In this pursuit I will consider two recent criticisms of the etiological theory, and the relevance of an alternative, dispositional theory of functions.[2]

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I. SETTING THE STAGE

Functional explanations are endemic to biology and the social sciences and are clearly intended to do explanatory work. However, the teleological character of functions, namely the fact that a function is identified as a consequence of the very item whose presence it is supposed to explain, has been taken as *prima facie* grounds for denying explanatory status. This methodological criticism was taken as evidence of the gulf between the natural sciences and the social sciences.

Hempel (1959) attempted to restore the unity of sciences under the methodological rubric of the covering-law model of explanation. On his account, which had become the "received view", a functional explanation of why a given trait or practice is present in a system consists in its derivation from a set of premises which include a statement of the function of the trait, general laws and initial conditions. However, given the absence of suitable biological, psychological and anthropological laws in the explanans to rule out functional equivalents for the explanandum event, functions logically fail to be explanatory and are allocated a merely heuristic role in scientific discourse (see also Horan 1989, pp. 137–138).

Hempel's negative solution to the philosophical puzzle engendered a new methodological problem. Biologists and anthropologists appeal to functions in ways that assume more than mere heuristic content. They dispute what is the correct function to ascribe to an organ, behavior or social practice. Empirical evidence is brought to bear in identifying functions. Hempel's solution rules that such scientific disputes are misdirected, scientific language and practice must be reformed in light of philosophy. However, some have responded to the mismatch by taking up the challenge to revise the philosophical analysis of functions in order to account for the explanatory character presupposed in scientific practice.[3]

But what type of enterprise is this? What do we want from a theory of explanatory functions, and hence what constitutes evidence that we have achieved our aims? Millikan (1989) defends her etiological theory of proper functions as providing a theoretical definition and rejects Wright's similar account on the grounds that he engages in conceptual analysis. Neander (1991) in response to Millikan's argument, distinguishes weak and strong interpretations of conceptual analysis. In the weak version, the conceptual analysis of the term "function" would identify implicit or explicit criteria of application for its utterance in the appropriate linguistic community. A strong interpretation adds to this the requirement that the analysis provide an account of the meaning of the term, and that it does so by outlining necessary and sufficient conditions for its use. These additional requirements are what Millikan rejects and motivate her promoting a theoretical definition of "function" and at the same time eschewing conceptual analysis. A theoretical definition describes the underlying phenomena that explain the linguistic category.

Neander is correct to defend the weak version of conceptual analysis here, and to suggest that it be used in conjunction with a theoretical definition. However I believe there is a stronger reason for adopting a combined approach than Neander's appeal to the "related aspects of language" which each approach illuminates. That is, "function" does not pick out a material substance like the natural kind terms "gold" or "water" which serve as exemplars for comparing conceptual analyses with theoretical descriptions for Millikan and Neander. It is more like "cause" or "reason" because it is both abstract and picks out an explanatory structure.[4]

Understanding an intended explanatory content of the use of "function" in current science will indeed employ tools of weak conceptual analysis (since it is the scientists' term and not a stipulation outside of science that is at issue), and at the same time provide a theoretical definition which grounds its "correct" use in contemporary science. Rather than ask what scientists mean by "function" or what objects the term denotes in the world, I suggest we explore instead the explanatory import of function ascriptions. That is, we should ask WHAT are they intended to explain and HOW they explain it. This approach will clarify both the content of the etiological theory and its differences with the dispositional alternative offered by Bigelow and Pargetter.

The etiological approach maintains that to explain why something occurs is to describe the causal history which led to the event - i.e. to give its etiology. The plausibility of any posited function derives from the theories in our repertoire. But acceptance of a given ascription is subject to the same evaluation as any other scientific claim.[5] While all theories of scientific explanation might well agree that causes are explanatory, functions are not causal in the usual sense, they are teleological. There have been many accounts of how intentional, goal-directed behavior is appropriately teleological. (Cummins 1975; Nagel 1977; Wimsatt 1972; Wright 1976.) The idea or the representation of the goal of an action in the mind of the actor operates as the proximate cause for that same action. My concerns, however, are with the explanation of systems to which the assignment of goals or intentions may not be appropriate. Can the presence and persistence of properties of biological populations and human societies be explained by appeal to natural and cultural functions? Larry Wright and Ruth Millikan have offered such accounts.

Wright (1976) proposes the following schema:

The function of X is Z if:

(i) Z is a consequence (result) of X's being there

(ii) X is there because it does (result in) Z.

Satisfying clause (ii) of the formula, that X is there because it does Z, depends on the appropriate causal relationship obtaining between X and Z. A rough version of Millikan's (1989, p. 288) theory is:

> A has F as its proper function if one of two conditions hold:
>
> (i) A originated as a "reproduction" 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 (causally, historically because) of this performance or
>
> (ii) A has F as a derived proper function, i.e. A originated as the product of some proper device that had performance of F as a proper function and that production of A is the normal means by which F is achieved.

Neither Wright nor Millikan make explicit the specific characteristics of the historical, causal "because" which carries the explanatory burden in their accounts. Clearly not all causal sequences render a consequence a function. To do so two conditions are required: that the organ or behavior has been selected over alternatives on the basis of its consequence, and that it is produced or reproduced as a direct result of that selection process (Mitchell 1989, 1993).[6]

II. NATURAL FUNCTIONS AND SOCIAL FUNCTIONS

Given this characterization of the two components of an etiological theory, it is clear how the etiological theory applies to natural items when their presence is the result of evolution by natural selection. The presence of traits which are adaptations can be explained by appeal to their past consequences on reproductive success. For example, in primates, males are often larger than females. What function does larger size serve for the males? Two answers were proposed. Darwin (1871) said that larger size in males was a result of sexual selection in that larger size resulted in greater success in male/male competition for mates. Selander (1972) suggested that the dimorphism allowed males and females to exploit different food resources. Clutton-Brock and Harvey (1977) tested the two hypotheses with comparative data. If the function of larger size for males was food acquisition, then one would expect to find this dimorphism in monogamous species of primates where males and females feed together. Here the dimorphism would be due to the consequence of larger size in competition with females for food. If the function was instead, enhancing success in male/male competition, one would expect to find dimorphism in polygamous species. In these situations there is strong competition between males for access to mates where the "winner" acquires a harem of females and the "loser" none. Clutton-Brock and Harvey's study gave evidence supporting Darwin's hypothesis for primates – dimorphism appeared in polygamous species. What is evident from this discussion is that the biological function of a trait is taken to be a real, discoverable property of natural organisms. Determining the correct function requires empirical evidence about conditions of selection and hence the consequences which the trait had in the past.

For an etiological functional relationship to occur, a selection background like natural selection must operate. The selected consequence of an item must furthermore be causally responsible for its replication and, hence, its current presence. Evidence only of a selection background or selection *of* an item will be insufficient to justify ascription of a function. The feature explained, like larger size in male primates, must have been selected *for* the functional consequence, say success in male/male competition, and, secondly, it must have been produced or reproduced as a direct result of that selection process, in this case by genetic transmission.

Function is also used to explain the presence of social practices. This can be understood by an etiological theory if a selection process is operating which takes the alleged functional consequence as causally relevant in the maintenance or transmission of cultural items. Functionalism in anthropology and sociology has been often associated with the program to explain human behavior and social institutions in terms of group level functions. That is, specific beliefs, behaviors, institutions exist because they allow the healthy functioning of the social group. Rituals maintain social cohesion, warfare regulates population size, marriage rules maintain cross-cultural social ties. Social practice *X* is there because it does *Y*, i.e. contributes to health or welfare of the social group., A classic contribution to ecological anthropology has been the research by Rappaport and Vayda on the Maring warfare cycle. (Rappaport 1968, Vayda 1974) The Marings of Papua New Guinea ritually plant yams and hold pig feasts in a cycle of war and truce-making with neighboring populations. It has been claimed that the function of this complex set of behaviors is to distribute protein (to allies during the pig feasts) and regulate population size (victors confiscate land of the defeated) within the context of the existing modes of production and environmental constraints.

A common criticism of functional explanation in cultural anthropology, the "fallacy of functionalism", is taking the fact that a consequence is beneficial as sufficient to accounting for why the practice exists. The etiological theory of function makes sense of such criticisms. Without evidence of selection and transmission mechanisms operating on that consequence, ascription of etiological function is groundless.

There have been a variety of responses to the recognition of this fallacy. Some, including Vayda, have taken seriously the required causal mechanisms for etiological functions and have re-directed their research toward gathering such evidence.

> Contrary to the assumptions made by me in the 1970's and by other anthropologists more recently, the fact that victorious warriors sometimes take enemy land and benefit from doing so has, by itself, no necessary explanatory import. To think that it does is a fallacy, not because it involves putting consequences forth as causes, but rather because it involves putting them forth without due concern for mechanisms, as if the mere fact of their being beneficial automatically conferred causal efficacy upon them. The mechanisms to which I am referring are ... intentional action, reinforcement, and natural selection, ontologically grounded in the actions, properties, and experiences of individual human beings. (Vayda, 1989 p. 163)

Just acknowledging the need for selection and replication mechanisms is not in itself sufficient. Vayda cautions against a "ceremonial" invocation of mechanism without any investigation into its operation. Additional evidence is required for identifying the actual selection and replication processes from the set of those that could logically do the job. Vayda now claims that conscious recognition of consequences and intentional action is responsible for the Maring ritual warfare cycle. To support the claim that redistribution of land is indeed the function of at least particular war cycles in the 1950's, Vayda has documented the fact that there was land shortage at the time, and that individuals consciously acted on the basis of this fact.

Individual intentional action is not the only selection and transmission mechanism available to support cultural function Soltis, Boyd and Richerson (1991) raise the question of whether functional accounts of cultural behavior can be justified by means of cultural group selection. They develop a model which suggests that under certain circumstances, cultural variatino can be maintained, and "cultural group selection could cause human societies to exhibit at least some group functional traits", (1991, p. 4). Group dissolution (extinction) and fissioning combined with cultural transmission are thus mechanisms which might explain the presence of practices like the warfare rituals in Papua New Guinea.[7] Groups which have a given practice and which are successful at survival or replication because of it, may pass this practice on through their lineage and to their offshoots by cultural transmission. Boyd, Richerson and Soltis present evidence from New Guinea that support the realism of the assumptions of their model, though it is not sufficient for justifying the claim that group selection actually operated. Just as in the case of natural function, an etiological explanation for cultural function requires detailed, context specific evidence of the actual causal history. Not every beneficial consequence will be explanatory, and not every explanatory consequence will invoke one particular mechanism, be it intentional choice, natural selection or cultural group selection. What the etiological theory accounts for is the structure of cultural functional explanation, whichever selection and replication mechanisms are in evidence.

To summarize, what is crucial to explaining the presence of a trait by its function on the etiological view, is that a specified type of causal background is available and that the functional consequence played the appropriate role in the causal history leading to the trait or social practice. Some practices of biologists and social scientists – theoretical development, empirical investigation and dispute resolution – make sense only if an etiological theory of function is presumed.

III. MALFUNCTION AND THE PROBLEM OF "DOUBLES"

Two intuition-driven counter-examples have been raised against the etiological approach. The first, the problem of malfunctioning items, like congenitally diseased hearts, has been heralded, curiously, as both a problem for an etiological account by Prior (1985) and an advantage of an etiological account by Millikan (1989). The problem that Prior identifies rests in how stringent one interprets Wright's condition (i) that *Z* is a consequence (result) of *X*'s being there. If one insists that each individual *X* must result in *Z* if *Z* is a function of *X* then malfunctioning hearts do not really have the function to pump blood since not only do they fail to, but they cannot succeed under any circumstances. (This distinguishes them from things such as safety devices which may not in any given time result in *Z* but would if the proper circumstances obtained. The airbag in a car may never get used, but its function is nevertheless to protect the driver from the impact of the steering wheel should a collision occur.)

If one adopts an interpretation of (i) to allow *Z* to be a typical consequence or result of the presence of things of type *X*, then the problem with malfunction dissolves. If things identified as *X*'s typically result in *Z* and condition (ii) is met, then an individual *X* which fails to result in *Z* (the typical consequence responsible for things of types *X* being present) is malfunctional. Millikan defines biological categories in terms of functions – things are grouped together in virtue of their causal history. An individual member of a category, of course, may be defective and hence unable to produce the consequences which define the category, but that does not diminish their membership. A heart that fails to pump blood, is still a heart.

> It is not then the actual constitution, powers, or dispositions of a thing that make it a member of a certain biological category. My claim will be that it is the "proper function" of a thing that puts it in a biological category, and this has to do not with its powers but with its history. (Millikan 1984, p. 17).

The objection that malfunction cannot be identified in an etiological theory is thus met by distinguishing types from tokens and by adopting a non-essentialist interpretation of types. Especially for biology, where traits and groups are both variable and evolving, there will be no set of necessary and sufficient properties which must be expressed by every item classified as a heart, or a Homo Sapiens. What allows the identification of a token as being of a specific type is historical continuity and what is the "normal" or "proper" function of that type is understood in terms of potential consequences whose expression are environmentally conditional.[8] Thus malfunction can be ascribed to items which meet the historical identity conditions for type-classification but fail to have "normal" expression in a given environment.

The second counter-example is the problem of "doubles". Bigelow and Pargetter suggest the following thought experiment to illustrate.[9] "Consider the possible world identical to this one in all matters of laws and particular matters of fact, except that it came into existence by chance (or without cause) five minutes ago." (1987, p. 188). The etiological account would find none of the items we identify as functions in our world to be functions in the parallel world. Clearly in the parallel world, by design of the example, there is no causal history in which to embed them. The problem of "doubles" offers two objects with identical current properties but with different histories. Bigelow and Pargetter claim our intuitions dictate that if the items are identical in structure and in all future consequences and one has function *Z*, the other must have the same function.

Millikan, in response, objected that they mistake a "mark" of a function (the structure and its current consequences) for the function itself (the causal historical significance of the structure/consequence pair).[10] However, Millikan's argument is not very satisfying. She admits to being brazen in just stating that the cases of doubles are like cases of gold and fool's gold, or H_2O and XYZ - cases which look alike to us, but are simply mistaken identities. This rather begs the questions, since for those who take the double case as a persuasive and adequate account of function should construe doubles as functionally identical. For Bigelow and Pargetter it is Millikan who has made the mistake by seeing doubles as functionally different. However, intuitions about fictional worlds, on either side, seem poor grounds for determining allegiance to one or another theory of functional explanation. If intuitions are to play a role, I believe they should be intuitions about our world.

At least for some evolutionary biologists, to ascribe a function to a trait does seem to require a causal history of adaptation. "Evolutionary adaptation is a special and onerous concept that should not be used unnecessarily, and no effect should be called a function unless it is clearly produced by design and not by chance". (Williams 1966, p. vii). This perspective if applied to Bigelow and Pargetter's double world would see traits but no functions – since chance produced those traits.

What intuitions are evoked by considering a "natural double"? In the case of mimicry members of two distinct species display very similar structures or traits, in fact indistinguishable to the appropriate parties, similar current consequences of these traits, and yet the populations have experienced different causal histories. Do we say that the traits of models and mimics – the natural doubles – have the same function?

Take the case of the Monarch butterfly (*Danaus plexippus*). These butterflies, found in North America, are brightly colored, orange and white or black and white, and are abundant. An early puzzle for Darwinism was explaining such conspicuous coloration. After all, one could see that camouflage coloration would be adaptive by helping an organism avoid predators. But conspicuous coloration presents a detectable visual signal to a predator. It turns out that having easily identifiable markings may have evolved for the function of signalling unpalatability. The Monarch caterpillars feed on milkweeds and thereby absorb and store cardenolides, a distasteful substance which makes them inedible both as caterpillars and as butterflies. If predators learn that Monarchs are unpalatable, then they will not eat them. Experiments in the laboratory and in the wild indicate that it is indeed the case that birds have aversive reactions to Monarchs after having tasted one, and they decrease the frequency of biting until completely stopping, and predation rates are thereby substantially reduced.[11] The body of experiments has been taken as evidence that "the function of conspicuous color patterns is aposematic (i.e. warning of unpalatability)."[12] Now consider the Viceroy butterfly (*Nymphalidae, Limenitus archippus*) which is the "natural double" to the Monarch in color pattern, even though it belongs not only to another species, but to a different family. The Viceroy has a diet unlike the Monarch's, and is, indeed, a desirable food source for birds. Nevertheless, the conspicuous coloration in the Viceroy also allows it to avoid predation, by being a mimic of the unpalatable Monarch. In cases of Batesian mimicry in butterflies the operator (predator) cannot visually distinguish individuals from the two unrelated prey species. One – the model – is both conspicuously colored and unpalatable. The other – the mimic – is similar colored, but palatable. The evolution of Batesian mimicry requires that the model organism must be more abundant than the mimic (or the learning process would work against the association of coloration with unpalatability). Studies of models and mimics have shown that mimetic resemblance tracks changes in morphology of the model, disappears in the absence of a model, and is controlled by a complex of genes.[13]

I have presented what I take to be a case of "natural doubles". Two types of organism, the Monarch butterfly and the Viceroy butterfly, are structurally similar or indistinguishable. Parallel to Bigelow and Pargetter's "double world", the morphological structures have the same future consequences, i.e. avoiding predation, but have had different evolutionary histories. Do we want to say that the conspicuous coloration of the Monarch and Viceroy have the same function? No. Mimics and models are not the same. As Wickler (1968, p. 108) says: "In general, we use the term mimicry only when the mimetic characters have been evolved for a specific mimetic function". The function of conspicuous coloration in the Monarch is to warn the predator of its unpalatability. The function of the Viceroy coloration is to mimic the model and deceive the predator into presuming it is unpalatable and thereby avoid predation. The same structure has two functions, one is to warn and the other is to deceive.

Thus, if two butterflies flit into our view and we see virtually identical structures and virtually identical current consequences on predation avoidance, Bigelow and Pargetter might have us conclude that the features of the model and the mimic have the same function. But surely that is a mistake. By knowing the causal history of conspicuous coloration, the models and mimics can be distinguished, and the functions of the traits understood. Perhaps a more compelling argument could be made if two identical structures had more divergent evolutionary histories, i.e. one the product of direct selection of the trait for the resulting consequence, and the other being a case of genetic "hitch-hiking" such that it was not selected directly for any feature save its location on a chromosome. While this is perfectly plausible on current biological theory, I know of no actual case that displays sufficient similarity to be a "natural double". Nevertheless, with the mimicry example I hope to have shown that knowledge of identical current structures is not sufficient to compel assent to identical function. History does matter to biological function, and it is that fact that the etiological account acknowledges.

Bigelow and Pargetter grant that an etiological theory of function renders function explanatory, but at the cost of looking "backward" to causal history. The doubles thought experiment was intended to suggest the need for a theory of function which is not just explanatory, but also "forward-looking". To satisfy this intuition they propose a dispositional theory of function, based on an analogical argument.

IV. BIOLOGICAL FITNESS ANALOGY

Bigelow and Pargetter propose to characterize function not in terms of causal history but rather in terms of future effect, that is, as a disposition. They draw an analogy between biological fitness and biological function and argue that just as "fitness" lacked explanatory status when it was identified with actual reproductive success and that status was restored by construing fitness as a disposition or propensity to have a certain degree of reproductive success – so too with "function". Explanation fails, according to them, when either is characterized solely in terms of actual consequences and is restored when characterized as a disposition to have certain consequences.

There is some initial plausibility for pursuing this line of reasoning. Given that a dispositional interpretation of fitness did solve an explanatory failure problem, it might well apply to other scientific concepts. However, as I have argued (Mitchell 1993) neither the negative nor the positive analogy of function with fitness can be sustained in ways that justify replacing an etiological theory of functions with a dispositional one. In fact, although the dispositional analysis offered by Bigelow and Pargetter may correctly describe the nature of a functional trait, it does not address the problematic of the etiological view, namely explaining why the functional trait with that nature is present. In the remainder of the section I will briefly outline how the strategy of dispositional analysis was successful in the case of biological fitness and show that the correlative move to a dispositional theory of biological function is beside the point.

An often cited criticism of evolutionary theory is that its central principle – the principle of natural selection – is tautological. Since this principle was taken to be empirical and not definitional, so much the worse for the scientific status of evolutionary theory. The principle of natural selection may be represented by the Spencerian motto of "the survival of the fittest". Or, to put it in a more acceptable form: "Those organisms which are more fit will reproduce more successfully" (you can substitute genes, groups, species, etc. for organisms). If, however, fitness is defined as actual reproductive success, an interpretation found in population genetics, then the principle of natural selection becomes the tautology: "Those organisms which have greater actual reproductive success will reproduce more successfully." Dispositional theories of fitness argue that actual reproductive success is a mistaken interpretation (Brandon 1978; Mills and Beatty 1979; Brandon and Beatty 1984). Identical twins in the same environment are equally fit to that environment. Nevertheless, by chance, one may perish before it reproduces while the other does not. Thus while the actual reproductive success of the twins differs, their fitness does not. If fitness is construed instead as a propensity to have a certain reproductive success, then the alleged tautology is transformed into an empirical explanatory principle. The twins have identical dispositions, but only one of them actualizes or manifests its potential.

How does the appeal to disposition work in an explanation, and what is thereby explained? Dispositional properties are commonly characterized as subjunctive conditionals describing the resultant behavior (the manifestation of the disposition) when certain antecedent conditions are realized. Furthermore, dispositional properties are explanatory, an implicit assumption of Bigelow and

Pargetter's argument, when associated with a causal basis.[14] The basis may be a physical structure that in conjunction with the antecedent circumstances, *ceteris paribus*, will cause the manifestation described by the subjunctive conditional. For example, fragility is a real dispositional property characterized by the conditional "If this object should be dropped, *ceteris paribus*, then it will break". The manifestation (breaking) is caused by one of a number of molecular structures (M, N, ...) given the object is dropped from a sufficient height onto a hard surface, etc. The dispositional property and the particular molecular structure that forms its basis in a specific object are distinct properties. It may well be the case that in different objects, different molecular structures will all be such that the object will break when dropped – hence fragility is multiply realised and supervenes on its bases. Which set of molecular structures are co-extensive with fragility is an empirical matter. That fragile objects break when dropped is a matter of definition.

When we ask, why did this egg break, we might explain it by its disposition, i.e. because it is fragile. Or we might explain it by its causal basis, because it has molecular structure M. The first answer abstracts away from the particular material of the causal basis and picks out a class of causally efficacious properties. Hence, fragility can explain why, say, chicken eggs break when dropped but marble eggs do not. The second explanation appeals to the basis, thus giving the specific cause of the manifestation in this instance.[15]

Biological fitness as a dispositional property or propensity is characterized by the subjunctive: "if the organism (or organism type) should be in environment E, then it will leave N offspring". Fitness is probabilistic in that it's causal bases determine probability distributions of a range of offspring outcomes in contrast to an all-or-nothing disposition like fragility. Thus a specific causal basis – like camouflage coloration in peppered moths or large male size in primates – given a set of environmental circumstances, confers probabilities onto the individual (or type) to display specific levels of reproductive success. Fitness thus may be expressed as the disposition to have an expected number of offspring.[16]

As we have seen, the disposition "fragility" explains why a certain object breaks. Either abstractly or by appeal to a causal basis in an object, a disposition explains the occurrence of its manifestation under the appropriate circumstances. Similarly with fitness. Differential fitness between organisms or organism types explains why one individual or type of individual reproduces more successfully, that is, manifests a specific reproductive success. Why did primate A produce more offspring than primate B? Answer, because primate A was more fit than primate B. The abstract, general disposition appeals to overall fitness specified only as a probabilistic disposition or propensity and is explanatory in the same way a general law is explanatory.[17] It explains the similarity with respect to reproductive success of two *prima facie* dissimilar phenomena. Darwin's original insight was that the features of Galapagos finches and the English pigeons were the results of the same causal process. Selection, whether by nature or by pigeon fanciers, "preserves" any advantageous character through time via differential reproduction of its bearers, given the heritability of the character.

Fitness, the disposition to leave N offspring, describes a property which is often important to evolutionary change. Of course, like any other disposition, there may be reasons why it is not expressed (and hence could not effect evolutionary change). The unfortunate twin who, by chance, failed to reproduce at all, failed to manifest the dispositional fitness endowed by its properties.

At a more concrete level, fitness or the disposition to leave N offspring identifies the causal basis, say, large size, and thus explains in a given circumstance why some individuals or types are more reproductively successful than others. Large size in conjunction with triggering conditions of male-male competition causes the manifestation of a reproductive success of N. In either case the dispositional property – fitness N – explains actual reproductive success. By construing fitness as a disposition, we see what fitness is – both at the concrete level by attaching it to a specific causal basis in a specific population and at the abstract level by seeing how whatever the particulars of the causal basis – be it large size or small number of eggs in the nest – having such a disposition can have consequences in the processes of natural selection and evolution.[18]

Thus the fitness of an organism, being a disposition or propensity to leave an expected number of offspring, can explain why the organism realizes a certain reproductive success, just as the disposition of fragility can explain why an object breaks. But does biological function operate like biological fitness in explanations?

V. FUNCTIONS AS DISPOSITIONS

The classic puzzle regarding functions is how the consequence of a trait could explain why the trait is, in fact, present. As we have seen the etiological account identifies function with that consequence in the past which was responsible for the current presence of the item in question. Both selection *for* the functional consequence over alternatives and replication *of* the structure with that consequence as a direct result of selection, are required to warrant etiological function ascription.

Bigelow and Pargetter objected to the causal history strategy for being too "backward looking". "Fitness is forward-looking. Functions should be forward-looking in the same way and, hence, are explanatory in the same way ... something has a function just when it confers a survival-enhancing propensity on a creature that possesses it" (Bigelow and Pargetter 1987, pp. 191–2). But what do these sorts of properties – dispositional functions – allow us to explain? Bigelow and Pargetter say (1987, p. 193):

> ...functions will be explanatory of survival, just as dispositions are explanatory of their manifestations; for they will explain survival by positing the existence of a character or structure in virtue of which the creature has a propensity to survive".

Applied to the case of sexual dimorphism discussed above, we get the following claims. Let us say that the function of large body size in male primates in a specified environment is to contribute to success in male/male competition for mates. What does that mean? On the etiological view, it is clear that identifying the function explains why the males are larger by appealing to the causal history of natural selection and evolution which took the differential effect on male/male competition as a cause for differential female acquisition and hence differential reproductive success. Given the heritability of the trait, this selection process would propagate larger body size over smaller size through a series of generations. On the dispositional view, identifying the function as contribution to success in conflict is to say that this is a survival-enhancing propensity (although the conflicts usually don't have such direct effects on physical survival of the winners and losers, but rather on their acquisition to mates in this case) which accrues to the trait in question. Hence when the right conditions obtain (two males meet in competition), the function will be manifested in actual domination and female acquisition and thereby enhance the survival of the larger male. What function explains on the dispositional view is why a large male primate reproduces more successfully than a small one. What function explains on the etiological view is why male primates are large.

Another way to put this distinction is as follows: the dispositional account tells us how having a trait with a function (a disposition to have a certain consequence) contributes to the survival and reproductive success of individuals with that trait, i.e. how they fare in the struggle for existence that constitutes natural selection. In contrast, the etiological view tells us how having a trait with a function (a consequence which has played a certain role in its causal history) contributes to the presence of the trait, i.e. how the trait has evolved by means of natural selection.

This is not to say that Bigelow and Pargetter's dispositional analysis renders function non-explanatory. Rather, scientific explanations are developed to do different things, are directed at different targets and give explanations at different levels of abstraction or concreteness. As Cartwright (1986, p. 203) puts it: "Explanations give answers not only to *why* questions, but also to *what* questions. They say *of* something, what it really is." If one asks what is this property of conspicuous coloration? One can answer by outlining the role it might play in contributing to survival. On the dispositional theory, a function identifies the nature of such a trait – its function in the current system. However, it does not account for the explanatory use of function to answer the original question of why the trait is there.[19]

Thus, the dispositional account does not offer a competing analysis to the etiological theory of functional explanation. Function is used in different scientific projects, there is not a single, univocal explanatory task for which such language is employed in scientific practice. The two theories discussed each focuses on a different use.[20] One might argue about which project is most significant, or most common in biological discourse. However, I believe such arguments are fruitless. Rather, the philosophical task is to recognize the plurality of explanatory projects, to clarify their relationships, and to explicate their structures. By attempting to do so in this paper, I hope to have shown that the etiological theory of function accounts for the explanatory structures implicit in at least some scientific practices, and that a dispositional theory does not offer a competing alternative account.

NOTES

[1] An early version of this paper was presented at the International Society for History, Philosophy and Social Studies of Biology, in London, Ontario, July 1989. See also Mitchell (1993) for related arguments against Bigelow and Pargetter (1987). I wish to thank the anonymous reviewer for detailed, helpful comments.

[2] Bigelow and Pargetter (1987).

[3] See Bigelow and Pargetter (1987) for an account of the range of such views in the current market.

[4] Thus it is a "methodological" concept, as Nordmann suggests, which needs to be "worked through again and again" (1990, p. 380)

[5] Or, if you prefer, which function ascription is accepted by the scientific community will depend on the constitutive standards for evidence and confirmation in that community.

[6] Wright suggests this: "functional explanation depends essentially on a selection background". (Wright 1976, p. 101)

[7] See also Boyd and Richerson (1991).

[8] See Hull (1987), Millikan (1989), p. 300.

[9] See also similar cases given by Prior (1985).

[10] Similar objections to adaptation claims are made by Gould who criticizes biologists who mistake current consequences for traits as sufficient evidence for the ascription of adaptive function. For discussion of this argument see Mitchell (1992), Gould (1987a, 1987b) and Alcock (1987).

[11] J. Brower (1958), L. Brower (1988) for experimental results.

[12] Guilford (1988), p. 9. Guilford however argues that this is insufficient evidence for the conclusion, and suggests various evolutionary explanations for the association between conspicuous coloration and unpalatability.

[13] Wickler (1968).

[14] Armstrong (1969), Prior (1985), Prior, Pargetter and Jackson (1982) defend the necessity of a causal basis, but seem to allow for the disposition, i.e. conditional behavior, to supervene over a variety of different causal bases. Sober, on the other hand, defends the reality of dispositions as similarly independent of the behavioral subjunctive conditional description, but requires something stronger, namely that "A scientifically respectable dispositional property must be a univocal characteristic that underlies all the instances in which the subjective conditional displays itself." (Sober 1984b, p. 47).

[15] General issues concerning the explanatory status of dispositions arise in the particular case of biological fitness. Namely, here has been discussion about how "fitness" is to be understood dispositionally, namely whether (i) dispositional language itself is non explanatory and hence that fitness be taken as an undefined primitive (Rosenberg 1982, 1985), (ii) "fitness" as a dispositional term merely marks a temporary stage in scientific development, and should discarded since biology has made sufficient advances to fill out the place-holder for which it was used (Waters, 1986), or (iii) the dispositional character of "fitness" is still required to be open-ended (Resnik 1988 and Nordmann 1990). In the argument that follows, I defend an explanatory role for the abstract place-holder and thus in this respect share the views of Resnik and Nordmann.

[16] This is the view presented by Mills and Beatty (1979), 270–275.

[17] See Cartwright (1986) for similar argument regarding teleological explanations in general, and Kitcher 1981 on the unifying role of schematic explanations.

[18] Ayala (1970) had something like this in mind when he spoke of two levels of teleology in organisms, the proximate end of features of organisms, as well as the ultimate goal of contribution to increase in reproductive success.

[19] For an argument that the two types of function are not always compatible, see Mitchell (1993).

[20] See Wouters (forthcoming) for other types of explanatory projects associated with function.

REFERENCES

Alcock, J.: 1987, "Ardent Adaptationist". *Natural History* **96**, 4.
Armstrong, D.: 1969, "Dispositions are Causes", *Analysis*, **30**, 23–26.

Ayala, F.: 1970, "Teleological Explanations in Evolutionary Biology", *Philosophy of Science*, **37**, 1–15.

Bigelow, J. and Pargetter, R.: 1987, "Functions", *Journal of Philosophy*, **LXXXIV, 4**, 181–196.

Boyd, R. and Richerson, P. J.: 1991, "Culture and Co-operation", in R.A. Hinde and J. Groebel (eds,); *Cooperation and Prosocial Behavior*, Cambridge University Press, pp. 27–48.

Brandon, R.: 1978, "Adaptation and Evolutionary Theory", *Studies in the History and Philosophy of Science* **IX**, 3: 181–206.

Brandon, R. and Beatt, J.: 1984, "Discussion: The Propensity Interpretation of 'Fitness' – No Interpretation is No substitute", *Philosophy of Science* **51**, 342–347.

Brower, J. 1960 "Experimental Studies of Mimicry", *American Naturalist* 44, 271–83.

Brower, L. 1988 "Avian Predation on the Monarch Butterfly and its Implications for Mimicry Theory", in L. Brower (ed.), *Mimicry and the Evolutionary Process*, University of Chicago Press, Chicago, pp. 4–6.

Cartwright, N.: 1986, "Two Kinds of Teleological Explanation", in A. Donagan, A.N. Perovich, Jr. and M. V. Wedin (eds.), *Human nature and Natural Knowledge*, D. Reidel, Dordrecht, pp. 201–210.

Clutton-Brock, T. .H. and Harvey, P. H.: 1977, "Primate Ecology and Social Organization". *Journal of Zoology London* **183**, 1–39.

Clutton-Brock, T. H., Harvey, P. H. and Rudder, B.: 1977, "Sexual Dimorphism, Socioeconomic Sex Ratio and Body Weight in Primates", *Nature* **269**, 797–800.

Cummins, R.: 1975, "Functional Analysis", *Journal of Philosophy* **72**, 741–765.

Darwin, C.: 1871, *The Descent of Man and Selection in Relation to Sex*, Princeton University Press, Princeton (1981).

Godfrey-Smith, P: 1994, "A Modern History Theory of Function", *Nous* XXVIII:3, 344–62.

Gould, S. J.: 1987a, "Freudian Slip", *Natural History* **92**, 14–21.

Gould, S. J.: 1987b, "Stephen Jay Gould Replies", *Natural History* **96**, 4–6.

Guildford, T.: 1988, "The Evolution of Conspicuous Coloration", in L. Brower (ed.), pp. 7–21.

Hempel, C. G.: 1959, "The Logic of Functional Analysis", in L. Gross (ed.), *Symposium on Sociological Theory*, Harper and Row, New York. Reprinted in C. G. Hempel, 1965, *Aspects of Scientific Explanation*, The Free Press, New York, pp. 297–330.

Horan, B. L.: 1989, "Functional Explanations in Sociobiobiology", *Biology and Philosophy* **4**, 131–158.

Hull, D.: 1987, "On Human Nature", *PSA 1986, Volume 2, Philosophy of Science Association*, 3–13.

Kitcher, P.: 1981, "Explanatory Unification", *Philosophy of Science*, **48**, 507–531.

Millikan, R. G.: 1984, *Language, Thought, and Other Biological Categories*, The MIT Press, Cambridge.

Millikan, R. G.: 1989, "In Defense of Proper Functions", *Philosophy of Science* **56**, 288–302.

Mills, S. and Beatty J.: 1979, "The Propensity Interpretation of Fitness", *Philosophy of Science* **46**, 263–286.

Mitchell, S. D.: 1987, *"Why Functions (in Evolutionary Biology and Cultural Anthropology)"*, unpublished Ph. D. thesis, University of Pittsburgh, microfilm no. 8808338, U. M. I., Ann Arbor, Michigan.

Mitchell, S. D.: 1989, "The Causal Background for Functional Explanations", *International Studies in the Philosophy of Science* **3**, 213–230.

Mitchell, S. D.: 1992, "On Pluralism and Competition in Evolutionary Explanations", *American Zoologist* **32**.

Mitchell, S. D.: 1993, "Comments and Criticism: Dispositions or Etiologies? A Comment on Bigelow and Pargetter" *The Journal of Philosophy* **XC**, 249–259.

Nagel, E.: 1977, "Teleology Revisited", *Journal of Philosophy* **74**, 261–301.

Neander, K.: 1991, "Functions as Selected Effects: The Conceptual Analyst's Defense", *Philosophy of Science*, **58**, 168–184.

Nordmann, A.: 1990, "Persistent Propensities: Portrait of a Familiar Controversy", *Biology and Philosophy* **5**, 379–399.

Prior, E.: 1985, *Dispositions*, Humanities Press, Atlantic Highlands, N.J.

Prior, E., Pargetter, R. and Jackson, F.: 1982, "Three Theses about Dispositions", *American Philosophical Quarterly* **19**, 252.

Rappaport, R.: 1968, *Pigs for the Ancestors*, Yale University Press.

Resnik, D.: 1988, "Survival of the Fittest: Law of Evolution or Law of Probability?", *Biology and Philosophy* **3**, 349–362.

Rosenberg, A.: 1982, "Discussion: On the Propensity Interpretation of Fitness", *Philosophy of Science* **49**, 268–273.

Rosenberg, A.: 1985, *The Structure of Biological Science*, Cambridge University Press, Cambridge.

Selander, R. K.: (1972), "Sexual Selection and Dimorphism in Birds", in B. G. Campbell (ed.), *Sexual Selection and the Descent of Man 1871–1971*, Aldine, Chicago, pp. 180–230.

Sober, E.: 1984a, *The Nature of Selection*. The MIT press, Cambridge.

Sober, E.: 1984b, "Force and Disposition in Evolutionary Theory", in C. Hookway (ed.), *Minds, Machines and Evolution*, Cambridge University Press, Cambridge, pp. 43–61.

Soltis, J., Boyd, R. and Richerson, P. J.: 1991, "Can Group-Functional Behaviors Evolve by Cultural Group Selection? An Empirical Test", *Preprint Series of the Research Group on Biological Foundations of Human Culture* (1991/92), at the Center for Interdisciplinary Research, University of Bielefeld, Germany.

Thornhill, R.: 1979, "Review of *Insect Behavior*, by R. W. Matthews and J. R. Matthews" *Quarterly Review of Biology* **54**, 365–6.

Vayda, A.: 1974, "Warfare in Ecological Perspective", *Annual Review of Ecology and Systematics* **5**, 183–93.

Vayda, A.: 1989, "Explaining Why Marings Fought", *Journal of Anthropological Research*, **45**, No. 2, 159–77.

Waters, K.: 1986, "Natural Selection without Survival of the Fittest", *Biology and Philosophy*, **1**, 207–225.

Wickler, W.: 1968 *Mimicry in Plants and Animals*. McGraw Hill.

Williams, G. C.: 1966, *Adaptation and Natural Selection*, Princeton University Press, Princeton.

Wimsatt, W. C.: 1972, "Teleology and the Logical Structure of Function Statements", *Studies in History and Philosophy of Science* **3**, 1–80.

Wouters, A (forthcoming) "Viability Explanation", *Biology and Philosophy*.

Wright, L.: 1973, "Functions", *Philosophical Review* **LXXXII, 2**, 139–168.

Wright, L.: 1976, *Teleological Explanations*, University of California Press, Los Angeles.