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UNIVERSITY OF OKLAHOMA

GRADUATE COLLEGE

**SOCIAL EXPLANATION AND SOCIAL FUNCTION:
AN INVESTIGATION OF THE NATURE OF FUNCTIONAL EXPLANATION
FOR THE SOCIAL SCIENCES.**

A Dissertation

SUBMITTED TO THE GRADUATE FACULTY

in partial fulfillment of the requirements for the

degree of

Doctor of Philosophy

By

**GARY RICK CHEW
Norman, Oklahoma
1999**

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**SOCIAL EXPLANATION AND SOCIAL FUNCTION:
AN INVESTIGATION OF THE NATURE OF FUNCTIONAL EXPLANATION
FOR THE SOCIAL SCIENCES**

**A Dissertation APPROVED FOR THE
DEPARTMENT OF PHILOSOPHY**

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INTRODUCTION

One of the primary tasks of a science is to explain empirical phenomena. In large measure the essential feature of an empirical scientific discipline, what makes it scientific, is its ability to offer scientific explanations. Philosophers of science have studied and explicated a very wide range of explanatory schemata in an attempt to flesh out the explanatory structure of the sciences. Some of these schemata are more suited to the physical sciences, while others seem more appropriate to the biological and the social sciences. Indeed, many have recognized a strong kinship between explanations in the biological and the social sciences. But there also has been much disagreement as to the precise nature of this kinship. Both sciences draw heavily on an appeal to the function of a trait or attribute in many of their deepest explanations. But what is a function, and how does reference to functions explain? Much ink has been spilled on these issues -- not only by philosophers, but by social scientists as well. A primary goal of this dissertation is to clarify, and perhaps resolve some of these questions.

The vexing thing about functional explanations is that they appear to be teleological. They look as though they appeal to intentions, to goal directed behavior. Intuitively, an artifact or trait has a function only relative to a design intended to accomplish a goal. Thus, it seems to function in the system in order to help the system accomplish the goal for which it was designed. It seems right to explain the existence of the heart in animals functionally by saying that the heart functions as a pump, and that animals need such a pump to circulate the blood. But upon deeper analysis we run into the following problem. The explanation appears to be this: the animal needs its blood to circulate in order to survive, and the heart will accomplish this, so the animal has (been

furnished with) a heart. Now, we can better explicate our problem. Our intuitive response has led us to look forward to the purpose to be served by the function of the trait in order to explain its existence. But this suggests that the heart was provided in the past in order that the animal could circulate its blood in the future. Such an explanation implicitly points to some type of designing intelligence that has predetermined what purpose the trait should play within the system. While this does not present a problem in the functional explanation of the existence of a bi-metallic strip in a thermostat, it does present a problem in the functional explanation of the existence of a heart in an animal. Thus, functional explanations in the biological and social sciences will (often) need cashing out in terms of natural, non-teleological mechanisms -- in terms of "design without a designer." In biology, natural selection is supposed to perform this work. But, as the philosophical literature attests, getting the explanatory scheme spelled out precisely, so that the required functional explanations work just right, is no easy matter.

Such issues are doubly complicated in the social sciences. For, here there is intentional design (by human agents); but there are also "unintended" social functions. The intended function of the Hopi rain dance, for instance, is, presumably, to cause rain. But empirical evidence suggests that it does not accomplish this goal very well. What then is the function of the rain dance in Hopi culture? Some anthropologists have suggested that it has a "latent" social function -- that it is part of a ritual structure that promotes social cohesion, and thus helps the culture to survive. And, presumably, since no single agent designed the rain dance to accomplish this function, this example illustrates the need within the social sciences for a naturalistic account of functional

explanation — an account of "design without a designer" — a need that seems as great as that within biology.

The main point of this dissertation is to explore the nature of functional explanation and to see how, as an explanatory scheme, it may be adapted to naturalistic explanation in the social sciences. The most penetrating work on functional explanation has been pursued with biology in mind. I will, therefore, spend a good deal of effort investigating and analyzing this work on biological explanation. However, my ultimate goal here is to find suitable functional explanatory schemes that elucidate the nature of functional explanation for the social sciences, particularly cultural anthropology.

In Chapter One I explicate the main philosophical themes in the study of explanation: the Deductive Nomological Model, the Inductive-Statistical Model, modern accounts in the D-N tradition, Paul Humphreys' causal account, functional explanation, and, heuristic explanation. In subsequent chapters I endorse a view of explanation that attempts to be consistent with some of the theoretical insights discussed in this chapter, particularly those of Wesley Salmon, Peter Railton, Paul Humphreys, Larry Wright and Harold Kincaid.

In Chapter Two I discuss the social doctrine of functionalism, the mainstay of explanation within the social sciences during much of the last century. I begin with some comments about the nature of social theories. I follow that with a brief review of the history of the social doctrine of functionalism and then move to a more substantial discussion of some of the classic criticisms directed against it. I close this chapter with a review of the lessons taught by these criticisms and the explanatory problems that require resolution in order to have successful functional explanation within the social sciences.

In Chapter Three I explore in some depth the issues that arise in the debate regarding functional explanation in biology. I outline the explanatory goals of the two accounts of biological functions, evaluate the strengths and weaknesses of the kind of work each account performs in different explanatory areas, and thereafter, address the question of whether or not the explanations produced by these accounts appear to be complementary parts of a larger explanatory text. I supplement some of my arguments in this area with a collection of analytical exercises. I argue that the exchanges with biology provide a splendid example for the social sciences to consider as they come to terms with their own applications of functional explanations within their own domain.

In Chapter Four I argue that functional explanation can succeed in the social sciences by employing a concept of social proper functions. As in biology, this requires the ability to make the connection between the consequence of the trait (contribution-to-survival) and the current presence of the trait (persistence-over-time). This leads to a discussion of selection mechanisms and the explanatory work required of them within the social sciences. I discuss relevant philosophic criticisms related to the presence or absence of social selection mechanisms and then discuss in some detail one of the selection mechanisms that I argue can make the critical contribution-to-survival/persistence-over-time connection. I demonstrate this claim with the use of one of the analytical tools employed in Chapter Three.

Having completed my attempts to cash out functional explanations in causal terms, I suggest that such explanations in the social sciences may be understood as ultimately non-teleological and causal, as they are in biology. Thus, when social mechanisms are used to explicate the contribution-to-survival/persistence-over-time

connection between the consequence of the trait and the current presence of the trait, the social sciences can create a viable concept of functional explanation — one that can successfully resolve the classic criticisms of the social doctrine of functionalism.

Chapter One

SCIENTIFIC EXPLANATION

This chapter reviews several accounts of the notion of *explanation* appropriate to the sciences. Given the multiplicity of accounts that have been investigated in the philosophical literature, no attempt can be made here to cover them all. But the most widely discussed accounts of explanation are included.¹ Since the Deductive Nomological (D-N) Model of Explanation is the point of departure for many of these accounts, I discuss this model in Section One. Thereafter, I discuss: the Inductive-Statistical Model in Section Two; Modern Accounts in the D-N tradition in Section Three; Paul Humphreys' causal account in Section Four; Functional explanations in Section Five; and, Heuristic explanations in Section Six.

In each section I attempt to define, illustrate and discuss one account of explanation, and also address some of the problems raised by that particular approach. In so doing, I discuss the views of Carl Hempel, Ernest Nagel, Wesley Salmon, Peter Railton, James Fetzer, Paul Humphreys, Larry Wright and Harold Kincaid. Lastly, in Section VII I provide some commentary on the relative merits and appropriate roles for the different accounts of explanation in the sciences.

Section One

The Deductive-Nomological Model of Explanation

The centerpiece of a D-N explanation is a valid deductive argument, an argument in which the premises deductively entail the conclusion. The conclusion of the argument, called the explanandum, describes the event or state of affairs to be explained. The premises must

contain at least one statement that is a general law, and typically contains one or more statements describing "antecedent conditions" which are particular events or states of affairs which, through the law, deductively "produce" the event to be explained.

The premises of a D-N explanation are called the *explanans*. A proper *explanans* must:

1. contain a statement of at least one general law that is essential to the validity of the argument (if deleted the argument would no longer be valid),
2. have statements (including the law) with empirical content, and
3. be true.

If the premises are not in fact true, the "explanation" cannot really explain. Similarly, if the law-like statement is not really a law of nature (in the actual world), then although the argument has the trappings of an explanation, it does not in fact explain the explanandum.

The conclusion of a D-N, the explanation must be a description of the empirical phenomenon to be explained. The argument formed from the explanans as premises and the explanandum as conclusion must be a valid deductive argument.

One of the classic examples of a D-N explanation (which was used by Hempel (1966:50)) is Perier's finding in the Puy-de-Dome experiment that the length of the mercury column in a Toricelli barometer decreased with increasing altitude:

- (a) At any location, the pressure that the mercury column in the closed branch of the Toricelli apparatus exerts upon the mercury below equals the pressure exerted on the surface of the mercury in the open vessel by the column of air above it.
 - (b) The pressures exerted by the columns of mercury and the air are proportional to their weights; and the shorter the columns, the smaller their weights.
 - (c) As Perier carried the apparatus to the top of the mountain, the column of air above the open vessel became steadily shorter.
-
- (d) (Therefore,) the mercury column in the closed vessel grew steadily shorter during the ascent.

Here both (a) and (b) are the laws in the explanation. Actually, (a) is not quite general enough to be a law, but could be cashed out as a law about equalization of pressure applied to a particular apparatus.

1. **The concept of "laws."** In a D-N explanation, we are already aware that the event described in the explanandum has occurred. We only try to "explain" what we already know to be the case. We seek to explain this event by finding the relevant laws of nature and by reconstructing the initial conditions from which the event can be derived. When this is successful, the general law is said to "cover" or "subsume" the specific event. This is so because the explanandum is logically entailed by the explanans.

The sentences in the explanans which state purported laws of nature are called law-like sentences. Under Hempel's view of D-N explanations law-like sentences have four properties:

1. they have universal form, i.e., their main logical "connective" is a universal quantifier,
2. their scope is unlimited, i.e., their domain of discourse is *everything*,
3. they do not contain designations of particular places or things or objects,
4. they contain only purely qualitative predicates.

When a law-like sentence is in fact true, it is a *law*. Thus, whenever a person calls a law-like sentence a *law*, he is asserting his belief that it is true.

Consider the sentence "All Anasazi pottery was made by women." While the sentence seems universal (1 above), it fails to be a law-like sentence because of its explicit reference to a particular group of people (3 above), the Anasazi. Property 4 above is designed to exclude implicit reference to one or more particulars such as occurs with the use of words like "precolumbian," "lunar," or "arctic."

Laws must be distinguished from *accidental generalizations*. The later, even if they are true, do not have *explanatory power*. "All the apples in my refrigerator are yellow" does not, intuitively, explain anything about why a particular apple in my refrigerator is yellow. This intuition derives from the fact that a red apple would not necessarily turn yellow *if* placed there. Law-like sentences should support *counterfactuals*. They should tell us what would happen *if* - e.g., if this table salt were placed in water, it would dissolve. This ability to support counterfactuals is connected with a more general feature. Law-like sentences should have (physical) *modal import*. They should tell us what is physically necessary, possible or impossible. E.g., according to relativity theory it is physically impossible to send a signal faster than light *in vacuo*.

2. Counterexamples to the D-N model. A number of stock counterexamples have arisen out of philosophical discussions about Hempel's D-N model. Almost all of these demonstrate instances of how the D-N model may fail to get at causal mechanisms.² That is, one driving intuition about explanations is that they describe the causes of the event to be explained – and in that way tell us why the event happened. It is should not be surprising that D-N explanations fail to mention causes. Hempel developed the D-N model of explanation in a radical empiricist climate that rejected the notion of causation as metaphysically suspect and empirically meaningless. So, in many cases where it may seem natural to require that laws be causal – i.e., that they at least indicate the causal processes involved in bringing about a phenomenon – Hempel tried to give describe a notion of *law* and give an account of *explanation* that completely avoided the notion of causation.

(CE-1) The flagpole example. A flagpole casts a shadow of length l . From the angle of the elevation of the sun (which is brightly shining), the length of the shadow and the law of

rectilinear propagation of light, we can deduce the height h of the flagpole. Does this explain the height of the flagpole in terms of the length of its shadow? Despite being a perfectly good D-N explanation — it satisfies all of the formal requirements — intuitively *l* does not explain h .

What went wrong? Given the angle of elevation of the sun, the height of the flagpole on a sunny day explains the length of the shadow because its interaction with the sun's rays *caused* the shadow, while the same cannot be said about the relationship of the shadow to the flagpole. CE-1 shows a failure of the D-N model to capture the *asymmetry* of the cause and effect relationship.

(CE-2) The barometer example. Falling barometer readings are (almost) always followed by storms. This observation of regularity, when combined with a statement that the barometer has just dropped, is a good predictor of a storm. However, it is no explanation of why a storm occurred.

What went wrong? The occurrence of the storm and the dropping of the barometer reading are both effects of a common cause, a drop in the atmospheric pressure. The problem is the failure of even a universal regularity to distinguish between cases when one event causes another and cases in which two events are the joint effects of a common cause.

(CE-3) The eclipse example. A D-N explanation of a total lunar eclipse could be satisfactorily created by deducing the eclipse from the relative positions of the earth, sun, and moon at some prior time in conjunction with the laws of celestial mechanics that govern their motions. However, a similar D-N explanation could be created by deducing (i.e. retrodicting) the eclipse from the positions of these bodies at some subsequent time. Most people would deny the latter as a proper explanation. It certainly has no predictive force.

What went wrong? D-N explanations do not impose any type of temporal restrictions such as requiring the explanatory facts within the explanans to pre-date the event described in the explanandum. Since temporal order follows causal order, a truly causal explanation will not have this defect.

(CE-4) The hexed salt example. I wave my hands over a quantity of table salt and "hex" it. The salt then placed in warm water, and dissolves. An explanation is offered as to why the salt dissolved. All hexed salt dissolves in warm water (note: this sentence is true and law-like); this particular salt was hexed. While this seems to be legitimate explanation under the D-N account it is clearly a flawed explanation.

What went wrong? The correct cause has been cited – all salt dissolves in warm water – but it has been combined with an irrelevant non-causal factor (hexing).

(CE-5) The moon and the tides example. Prior to Newton, mariners recorded the correlation between the phase of the moon and the rising and falling of the tides. They lacked causal understanding, and Wesley Salmon suggests that if they attempted any explanation it may well have been to the effect that, "God in his goodness put the moon in the sky as a sign for the benefit of mariners." (Kitcher & Salmon 1989:47) However, they could make accurate predictions and, arguably, had within their power the ability to create D-N type explanations concerning the behavior of the tides based upon the regularities present in their observations. Still, it was not until Newton that actual explanations of the operation of the tides were available.

What went wrong? Here regularity provided sufficient data for prediction but not for causal understanding. As in the barometer example, regularity cannot distinguish between a causal relationship and the relationship between joint effects of some other common cause.

Does the day cause the night? Night has regularly followed day since the creation of our solar system, forming an impressive record of constant conjunction. These are important counterexamples to Hume's constant conjunction thesis – i.e. that causation is nothing but constant conjunction of similar antecedent events followed by like consequent events. And they show the flaw in Hempel's symmetry thesis, that, "every (nonstatistical) scientific prediction is a D-N explanation." (Kitcher & Salmon 1989:49)

It is perhaps worth re-emphasizing that many of these counterexamples may be fixed by appealing to the notion of causation. Thus, in a less strictly empiricist climate, much of Hempel's D-N account of explanation may be recovered. It will not be recovered as a complete account of all types of explanation, but it can remain as one important kind of scientific explanation, provided that the law-like sentences involved are taken to state the purported cause of the explanandum.

Section Two

The Inductive-Statistical Model of Explanation

As originally conceived by Hempel, an Inductive-Statistical explanation is an inductive argument that provides a high probability that the event-to-be-explained, the *explanandum*, would occur given the truth of certain explanatory facts, the *explanans*. These explanatory facts must contain at least one general statistical law, thus making an I-S explanation a "covering law explanation" in the same sense that D-N explanations are.

Similar to D-N explanations, the premises of an I-S explanations, the *explanans*, must:

1. contain a statement of at least one general statistical law that is essential to the argument,
2. have statements (including the law) with empirical content, and
3. be true.

The conclusion of an I-S explanation, the *explanandum*, must be a description of the empirical phenomenon to be explained. The argument formed from the explanans as premises and the explanandum as conclusion must have a correct inductive form, as follows:

$$\frac{\begin{array}{c} P[G / F \ \& \ H] = r \\ Fb \ \& \ Hb \end{array}}{\text{Gb}} \quad [r]$$

for r a number close to 1. For example, 'Fx' may stand for 'x has a strep infection (during a specific period of time)', 'Hx' for 'x receives penicillin (during the period)', 'Gx' for 'x quickly recovers from strep (during the period)', 'b' may name John Jones.

The first premise of this argument is a statistical law that asserts that the relative frequency of G s among F s that are H s is r , where r is a number fairly close to 1. The double line indicates that the argument is inductive, as opposed to deductive. The '[r]' next to the double line represents the degree of inductive probability conferred on the conclusion by the premises. In the example above, the second premise is "covered" by the general statistical law, which is the first premise. It therefore produces a conclusion which may be expected by the degree of inductive probability indicated by r . Hempel supposes for the argument to be *explanatory*, r must be fairly close to 1. Explanations are supposed to make the explanans likely.

1. I-S explanations and the concept of "laws." For I-S explanations, Hempel adds the condition that correct explanations must employ statistical laws in the explanans that are *maximally specific*. Formally, the *requirement of maximal specificity* (RMS) means this:

Let S be the conjunction of all of the premises of an I-S explanation, and let K be the body of knowledge (of the community in which the explanation is

offered) at the time in question. Then the proposed explanation (of the form just given) must meet the following condition:

If $(S \ \& \ K)$ logically implies that b belongs to a subclass $(F \ \& \ H_1)$ of $(F \ \& \ H)$ (i.e. if $(Fb \ \& \ H_1b)$ and $\forall x((Fx \ \& \ H_1x) \supset (Fx \ \& \ Hx))$), and if $(S \ \& \ K)$ also logically implies that the statistical probability of G in $(F \ \& \ H_1)$ is some number r_1 (i.e. $P[G / F \ \& \ H_1] = r_1$), then $(S \ \& \ K)$ must imply that $r_1 = r$. (see 1965:400).

The point of *RMS* is that we may have a number of statistical laws that apply to the

explanans – e.g. $P[\text{recovery} / \text{has strep \ \& \ takes bed rest}] = .75$, $P[\text{recovery} / \text{has strep \ \& \ receives penicillin}] = .93$, $P[\text{recovery} / \text{has strep \ \& \ receives penicillin \ \& \ takes bed rest}] = .97$. If we know only that the subject, b , takes bed rest, then the first statistical statement is the relevant one. Similarly, if we don't know whether b takes to his bed, but we do know he takes penicillin, then the second statistical statement is the relevant one. And if we know that b both takes penicillin and bed rest, then the narrower reference class given by the third statistical statement becomes the relevant law, and the two statistical statements employing wider reference classes are superceded by the more specific, narrower reference class. Thus, a statistical explanation should employ the statistical statement with the narrowest reference class to which the subject of the explanation, b , is known to belong.

For Hempel, then, all bona fide I-S explanations must satisfy *RMS*, therefore they are all relative to a given knowledge body of knowledge, represented by a class K of accepted statements (1965:402). The *epistemic relativity of statistical explanations* does not exist in D-N explanations since the requirement of maximal specificity is automatically fulfilled in the case of a proper D-N explanation. This is so because in a D-N explanation the conclusion deductively follows from the premises and any more specific information, if added to the premises, cannot undermine the deductive entailment of the conclusion. However, the degree of support r of the conclusion of an I-S argument can be undermined by adding additional information to the premises (i.e., I-S arguments are nonmonotonic). So the most relevant

statistical argument for an I-S explanation must be related to premises that account for all relevant information that *the person offering the explanation* knows.

2. Problems with I-S explanations. The following are typical problems that often arise with I-S explanations:

a. The relevance problem: Counter-examples to the I-S model of explanation.

One problem for I-S explanations is their inability to always block out irrelevant information from becoming part of the explanation.

(CE-6) The vitamin C example. John Jones recovers from a cold within a week. An explanation is offered that he was almost certain to recover from his cold within a week because he took vitamin C, and that almost all colds clear up within a week when vitamin C is taken. However, it is a known fact that almost all colds clear up within a week whether or not vitamin C is taken.

What went wrong? The administration of vitamin C has not been shown to be relevant to John Jones' recovery from a cold. Thus, CE-6 fails to be a bona fide explanation.

(CE-7) The oral contraceptive example. John Jones offers the 'explanation' that his failure to become pregnant is a result of his consumption of oral contraceptives.

What went wrong? Again, the problem is one of relevance, in this case the relevance of males taking oral contraceptives.

As the above counter-examples demonstrate, RMS fails to block irrelevant information from becoming part of an I-S explanation. However, the relevance problem impacts more than just I-S explanations. The same type of relevance issue can be applied to D-N model explanations, which have no RMS requirement, as is the case with the prior example of the 'hexed salt' (CE-4).

This problem of relevance is, however, easily overcome. One need only require that the explanation employ the most general law-like statement (either "deductive" or statistical) that will supply the argument needed for the explanation. For I-S explanations, this means that the statistical law in the premises should employ the broadest relevant reference class.

b. The problem of the ambiguity of I-S explanation. The requirement of maximal specificity was designed to solve the problem Hempel called *the ambiguity of I-S explanation*. This problem can be illustrated by the use of our prior example of an I-S explanation concerning John Jones quick recovery from a streptococcus infection. In that example, F stands for having a strep infection, H for administration of penicillin, G for quick recovery, b is John Jones, and r is a number close to 1.

$$\frac{P(G/F.H) = r}{Fb.Hb} \\ \hline \hline Gb \quad [r]$$

(C-E 8) The penicillin-resistant strain of streptococcus infection. We can see the problem of the ambiguity of I-S explanation if we modify our example. We can do this by partitioning the class of people who have strep infection and have received an injection of penicillin into a set of subclasses, i.e., those that have a penicillin-resistant strain of streptococcus bacilli, and those that do not have such a penicillin-resistant strain. We then include the knowledge that if a person is infected with a penicillin-resistant strain, then the probability of his quick recovery after treatment of penicillin is low (or at least not very high); John Jones is infected with a penicillin-resistant strain; J stands for the penicillin-resistant

character of the strep infection; and, $r1$ is a number *close to zero* (or at least not close to 1).

Our argument now looks like this:

$$\frac{\begin{array}{c} P(G/G.H.J) = r1 \\ Fb.Hb.Jb \end{array}}{Gb} [r]$$

What went wrong? The ambiguity of I-S explanations is present in this counter-example when compared to the original example of strep infection because we now have two inductive arguments with similar premises and both of those argument's premises could be true. However, the conclusion of these arguments is not only different, the conclusion of the second argument strongly undermines the conclusion of the first argument. Indeed, the second argument cannot explain Gb on Hempel's view, since $r1$ must have a large value to contribute to an explanation. This issue is discussed hereafter as the problem of high probability.

To better understand why Hempel created the requirement of maximal specificity with which to address the problem of the ambiguity of I-S explanations, it is helpful to look at the problem which underlies the ambiguity problem.

c. The reference class problem. Hempel designed RMS in an attempt to solve the ambiguity problem of I-S explanations. At its base, the ambiguity problem is a reference class problem. In a probability expression of the form " $P(Y/X)$ " X is referred to as the *reference class* and Y as the *attribute class*. In our prior strep example, F (having a strep infection) is the reference class and G (quick recovery) is the attribute class. The problem concerns how to choose a statistical law with a suitable (*maximally specific*) reference class for an I-S explanation of any particular fact.

A reference class is maximally specific with respect to any attribute class if our available knowledge does not allow us to make a *relevant partition* of the reference class. A *partition* is a division of the reference class, or a cell thereof, into a set of subclasses that are mutually exclusive and exhaustive within F. Such a partition is *relevant* if it picks out a subset of F in which the probability of G is different from what it is in F. In the strep example, the initial relevant partition of F was into two subclasses: one consisting of those treated with penicillin, H, and the other consisting of those not so treated. If F.H was maximally specific at this point, e.g., we couldn't make a further relevant partition, then the explanation for why John Jones recovered from strep was that he was a member of a class of those who were infected and were treated with penicillin. However, we made a further relevant partition in CE-8. We partitioned the subclass of those treated with penicillin into two further subclasses: those whose infection was of the resistant to penicillin strain of strep, and those whose infection was not of the resistant to penicillin strain. Now, if F.H and F.HJ are maximally specific at this point, then the explanation for why John Jones recovered from strep is that he was a member of a class of those who were infected and were treated with penicillin, and he was a member of a subclass of that class whose infection was of the strain not resistant to penicillin, J.

In the above example, a requirement of relevance has been added to the reference class partition in relation to the attribute class in order to attempt to block partitions based on irrelevant information from becoming part of an I-S explanation. Wesley Salmon also suggests that another restriction in RMS is needed. Salmon argues that we should insist that the antecedent conditions in an I-S explanation must be temporally antecedent to the explanandum ("b's membership in F1 cannot be known before its membership in G has been ascertained" p. 79).

(CE-9) Strep recovery from news report. John Jones, of our previous strep examples, is a well-known person whose recovery is reported on the news. It turns out that people that have the non-penicillin resistant strain of strep, who take penicillin and who have their recovery reported on the news have a higher recovery than those who in the same circumstances do not have their cases reported on the news. An explanation is offered for John Jones' quick recovery that includes the fact that it was reported on the news.

What went wrong? RMS failed to block this subsequent reference class partition into subclasses of those whose quick recovery was reported on the news and those whose quick recovery was not reported on the news. Additionally, the news report partition was relevant since the probability of some attribute G in each partition cell is different than the other partition cell. Thus, even the addition of a *relevant* partition requirement did not beef-up RMS sufficiently to block this proffered explanation.

However, Salmon's addition to RMS, that the antecedent conditions in an I-S explanation be temporally antecedent to its explanandum, does block the use of the news report as part of the explanation since the news report is not temporally antecedent to John Jones' quick recovery from strep. Despite this success, Salmon's requirement seems artificial. The reason the news report should not be a premise in the explanation of Jones' quick recovery is because it played no *causal* part in that recovery. Salmon's unasserted but central claim for the success for the relevance blockage performed by this additional requirement appears to be the claim that causation must be temporally antecedent to its effect. Consider the following:

(CE-10) Strep recovery from *prior* news report. *City Times* is a newspaper that has followed the career of John Jones. Recently, *their rivals, State Post, have scooped City Times on several major stories.* Desperate to outdo the *Post*, the editor for the *Times* decides to

gamble that Jones will have a quick recovery from strep, since most people do, and reports Jones' quick recovery before it actually happens. It turns out that by chance the editor is quite good at such guesses and people who take penicillin and who have their recovery reported on the news before they are recovered have a higher recovery than those who in the circumstances do not have their cases prematurely reported on the news. An explanation is offered for John Jones' quick recovery that includes the fact that it was prematurely reported on the news.

What went wrong? No version of RMS blocks the use of good guessing, and premature reporting on the news as a premise in an explanation of Jones' quick recovery, including Salmon's latest requirement that the report be temporally antecedent to the quick recovery. Again, the real issue is whether the report is causally related to the recovery, not whether or not it was antecedent to it.

d. The problem of high probability versus statistical relevance. As noted earlier, for Hempel, in an I-S explanation there must be a *high probability* that the explanandum was to be expected by virtue of the explanans. Wesley Salmon argued, contra to Hempel, that statistical relevance, not high probability, is the key desideratum in statistical explanation.

What is crucial for statistical explanation . . . is not how probable the explanans renders the explanandum, but rather, whether the facts cited in the explanans make a difference to the probability of the explanandum (Kitcher and Salmon 1989:59).

Salmon's point is easily illustrated by the following counter-example from Mendelian genetics.

(CE-11) The short pea plant with tall parents. If it is given that pea plants have dominant genes T for tallness and recessive genes t for shortness, then if numerous heterozygous tall plants (Tt) are crossed with other heterozygous tall plants (Tt), it is probable

that the average results will be 25% TT, 50% Tt and 25% tt. In other words, there is a probability of .25 that an offspring will be short, as well as a probability of .75 that an offspring will be tall. Thus, we can create a *low probability* argument in I-S form as follows:

Pea plants have dominant genes T for tallness and recessive genes t for shortness.

When heterozygous plants (Tt) are crossed, the genes for each plant split into (T)(t) and, provide the opportunity for each to join with one of the split halves of the other heterozygous pea plant, thereby producing a probability of .25 that an off-spring will have two different recessive genes (t)(t), and a probability of .75 for the other combination in which there is present at least one dominant gene (T).

a and b are tall heterozygous pea plants that were crossed and had an offspring, c.

[25%]

c is a short pea plant.

What went wrong? Notice that if c is in fact a short plant, this fact must be left unexplained on Hempel's I-S account. Only the existence of tall plants is explainable (provided .75 is considered high enough for such an explanation). But it seems more reasonable to hold that the likelihood of recessive genes combining is what is relevant to the event-to-be-explained, and supplies a completely satisfactory explanation regardless of whether that likelihood is one of *high probability*.

As Salmon documents, in 1977 Hempel retracted the high-probability requirement on I-S explanations (p. 61).

Section Three

Modern Accounts in the D-N Tradition:

Peter Railton and James Fetzer

Two recent versions of views, which at their core remain compatible with the basic concepts of Hempel's D-N and I-S accounts, are those of Peter Railton and James Fetzer. Both accounts are nomically oriented in the "covering law" tradition as well as being deductively oriented in their treatment of probabilistic explanation.

1. Railton's Nomothetic/Mechanistic Account: A deductive-nomological model of probabilistic explanation (D-N-P). Railton's account seeks to avoid three of the major problems in Hempel's I-S model, (1) epistemic relativization, (2) the requirement of maximal specificity, and (3) the requirement of high probability. In formulating this account, Railton employs the concept of an "ideal explanatory text," a notion that will be described below.

Railton uses the example of an event that has an extremely small probability of occurring, the alpha decay of a nucleus of uranium 238, to illustrate how on his account even unlikely events are explainable. He begins as follows:

- (a) All nuclei of *U-238* have probability p of emitting an alpha-particle during any interval of length δt , unless subjected to environmental radiation.
- (b) Nucleus u was a nucleus of *U-238* at time t and was subjected to no environmental radiation during the interval $[t, t + \delta t]$.

- (c) Nucleus u had a probability p of emitting an alpha-particle during the interval $[t, t + \delta t]$.

Railton construes the probabilities involved in this and other probabilistic explanations as single-case propensities, and premises such as (a) as lawful generalizations. While (c) is subsumed by (a), Railton requires more, namely, an additional premise that is itself a derivation

of (a) from the higher law behind (a). Under his view, this derivation explains the mechanisms at work which provide for the *possibility* of the occurrence of (c).

Note that the form of the argument involved in the explanation is deductive. Lastly, Railton requires the addition of the parenthetic addendum, which advises us that the event-to-be-explained in fact occurred. This is not included as a premise, since to do so would make the explanation circular.

Any explanatory account employing Railton's D-N-P (Deductive Nomological Probabilistic) model contains these three components:

1. A derivation of the theoretical law which explains the lawful generalization used as a premise in the account,
2. A D-N inference (covering law subsumption) of a chance statement about the event-to-be-explained, and
3. The parenthetic addendum, stating that the explanandum occurred.

Such an explanatory account is not an argument. It does not explain the occurrence of the event-to-be-explained, only the possibility of the event occurring. Stated otherwise, it does not provide a reason for *why* one probability was realized rather than another, since, among randomly occurring events, the reason is *chance*.

a. Avoidance of Hempel's use of epistemic relativization in I-S explanations.

Railton seeks the objectivity of D-N explanations in his approach to I-S explanations. He requires that explanations reveal the mechanisms at work in the world. In an attempt to achieve this goal, he requires not only that the lawful generalization in the explanans *subsume* the explanandum, but that an additional premise set out and *explain* the lawful generalization. In essence, the explanation of the lawful generalization is itself a D-S explanation. Thus, under Railton's view, the epistemic relativization of the explanation to the *state of knowledge of the*

explainer is avoided and in its place is substituted the more objective standard of explaining the mechanisms behind the initial law.

b. Avoidance of Hempel's requirement for maximal specificity. As was previously discussed, in a probability expression of the form " $P(Y/X)$," X is referred to as the reference class. The problem for Hempel's account was how to choose a statistical law with a suitable (*maximally specific*) reference class. Hempel's attempted resolution of this problem was to formulate RMS. Hempel did not have this problem with D-N explanations because there the conclusion deductively follows from the premises and any more specific information, if added to the premises, cannot undermine the deductive entailment of the conclusion. Railton's resolution of the RMS issue is not dissimilar. He requires the added putative law to entail the possible occurrence of the event described in the conclusion, if it does not (by omitting further relevant factors), the putative law is simply false. This approach avoids the need for reference classes. But as Salmon notes, this avoidance of reference classes is possible because Railton adopts a *single case propensity interpretation* of probability.

c. Rejection of Hempel's high probability requirement. Railton simply rejects high probability as a requirement for I-S explanation. Recall our prior example of the explanation of the short pea plant. For Railton, like the example demonstrates, the less probable results of stochastic mechanisms are understood as well as their more probable counterparts. However, Railton requires that such an explanation contain a D-N inference (a deduction) of the probability of the occurrence of the event-to-be-explained. It is important to note that Railton is not deducing the occurrence of the event-to-be-explained, only the *probability of its occurrence*. Railton thereafter adds a *parenthetic addendum*, which simply notes that the

event did in fact occur. The parenthetic addendum is *not* deduced, only the propensity of its occurrence.

d. Railton's concept of an "ideal explanatory text." Given an event that we wish to explain, an "ideal explanatory text" would completely spell out all of the causal and nomic connections that were relevant to the occurrence of the event-to-be-explained. Such an account would obviously be extremely large and complicated, an ideal that could never be completely realized. However, Railton argues that this is not important since scientists are searching for *explanatory information* that enables them to fill in parts of an ideal explanatory text. But although the ideal text is never completely filled in, it constitutes a framework that provides guidance for understanding of different aspects of the world.

Salmon argues that Railton's distinction between the ideal explanatory text and explanatory information can go a long way toward reconciling the views of the pragmatists and the realists. Salmon suggests thinking of the objectivists as focusing on the ideal explanatory text while pragmatists are focusing on explanatory information (part of the ideal explanatory text). Under this view, one comprehends the different focus of each – and the resultant tendency to talk past each other. However the concept of an ideal explanatory text creates a common foundation for their focus, albeit different aspects of the same thing, upon which a new consensus between the two might be erected.

2. **Fetzer's Account of Probabilistic Causality.** Fetzer's account of scientific explanation is based upon two major precepts: (1) single case propensity interpretation of probability (like Railton), and (2) the inadequacy of extensional logic for the explication of *law-likeness* and *causality*. As to the first precept, Fetzer requires a probalistic inference (I-S like) that the event-to-be-explained holds, i.e., that the explanandum is partially entailed by the

explanans. This results in Fetzer joining Railton in abandoning a high probability requirement for I-S explanations. While their purposes are similar, their methods are slightly different. Railton does so on the basis of a D-N inference of a chance statement about the explanandum event, whereas Fetzer employs a notion of partial entailment, resulting in an I-S like inference.

Fetzer's second precept is addressed by means of a modal intensional logic he develops for this purpose. Salmon's criticisms of this axiomatic system are extended, substantial (K & S 1989: 168 - 171), and beyond the scope of this paper.

Fetzer also addresses the reference class problem of I-S explanations by creating a requirement for *strict* maximal specificity (RSMS), i.e., "(a)n explanation of why an explanandum event . . . occurs is adequate *only if* every property described by the antecedent condition(s) is nomically relevant to the occurrence of its attribute property" (Fetzer 1981: 125-26). Stated otherwise, Fetzer sidesteps the reference class problem by imposing a strict requirement for the partial entailment of the explanandum for the explanation to be considered adequate. For Fetzer, if the law statement in the premises is universal, then the relationship between explanandum and explanans will be that of complete (deductive) entailment. However, if the law statement is statistical, then the relationship between explanandum and explanans will be that of partial entailment.

3. Summary. Several concepts unify these two accounts. Among these are their rejection of the high probability requirement of I-S explanations, their attempts to link probabilistic explanation with causal factors, and their retention of significant portions of Hempel's original models.

Lastly, this appears an appropriate place to return to the reoccurring reference to whether or not explanations are arguments. No one really holds that an explanation is an argument. Rather, nearly everyone holds that:

- (1) explanations (scientific explanations) involve some logical connection between a law-like (perhaps statistical) statement and the statement to be explained (or at least a closely related statement to the statement to be explained);
- (2) the statement to be explained must be *known to be true* (or at least is presumed true);
- (3) the law-like statement must *be true*;
- (4) the law-like statement must be essential to making the logical connection hold;
- (5) other sentences that aid the law-like statement in making the logical connection to the explanandum (or related statement) must be true; and,
- (6) some consideration of relevance such as generality is usually required also.

Accounts mainly differ on other details, e.g., high probability requirement, logical relation is deductive only (Railton) or probabilistic also (Hempel or Fetzer).

1. Collateral problems raised by modern accounts in the D-N tradition

a. Consequences of rejecting Hempel's high probability requirement for I-S explanations Both Railton and Fetzer concur with Salmon in rejecting Hempel's requirement of high probability in I-S explanations. The central concept behind that rejection is a symmetry principle, namely, if a given stochastic process gives rise to both improbable as well as highly probable outcomes, then we understand the improbable results just as well as we understand the highly probable results. This symmetry principle is illustrated by CE-11, the pea plant example. There we can understand the likelihood of recessive genes combining and producing a short plant (a .25 probability) just as well as we can understand the likelihood of at least one

of the combining genes being a dominant for tallness gene and producing a tall plant (a .75 probability).

So what is given up by this seemingly rational rejection of a high probability requirement in I-S explanations? Salmon describes the intuition that leads to the high probability requirement as follows:

. . . if a set of circumstances of type C on one occasion explains the occurrence of an event of type E, then circumstances of the same type C cannot, on another occasion, explain the nonoccurrence of an event of type E (or the occurrence of an event of a type E' that is incompatible with E) (Kitcher and Salmon 1989: 178).

Salmon notes that it has been feared that the rejection of this principle would open the floodgates to all sorts of unfounded teleological, metaphysical, and pseudo-explanations such as explaining any and every thing which occurs within the universe as owing to "God's will." However, Salmon argues that we can rule out 'explanations' of this type without the help of the high probability principle by insisting that scientific explanations invoke only the relevant laws and facts.

b. Railton's and Fetzer's adoption of the single-case propensity interpretation of the law premise. Both Railton and Fetzer employ the single-case propensity interpretation to avoid the problems inherent in the requirement of maximal specificity. Both justify their adoption of the single case propensity interpretation by requiring that the explanandum be entailed by the explanans, which gets them out of the necessity of dealing with the reference class problem. But Railton and Fetzer use the single case propensity interpretation in different ways. Whereas Railton employs a logical entailment between the law-like statement and a single case propensity statement about the explanandum event, Fetzer employs a partial

entailment between the single case propensity statement and a (non-probabilistic) description of the explanandum event.

Salmon argues that the propensity interpretation has equivalent problems of its own. He argues that the single case propensity interpretation requires the concept of a *chance set-up*, a device that produces a set of alternative outcomes, each with a determinate probability. When a fair die is rolled from a dice cup in the standard manner, there are six possible outcomes and thus, the chance set-up has a propensity of $1/6$ to produce each of the outcomes on a given roll. While a chance set-up can be repeated, we have to be specific to do so. We have to describe the relevant features and omit various irrelevant ones to determine what constitutes an additional trial on the same chance set-up, i.e., a symmetrical die is relevant while the fact of whether the die is thrown by a right or left-handed player is irrelevant. Salmon argues that this relevancy determination is not essentially different from the reference class problem.

The further question arises as to whether a "propensity interpretation of probability" does the same work as a straight forward frequency interpretation of probability. Consider the following:

(CE-12) **The corkscrew factory.** A corkscrew factory has two machines, one old and one new, each of which produces a certain amount of corkscrews each day. Some of the corkscrews made by each machine are defective. We can speak of the relative *propensities* of the machines to produce corkscrews or of their *propensities* to produce defective corkscrews. Given numerical values, we can calculate the *propensity* of the factory to produce defective corkscrews. But, an inspector picks out one defective corkscrew and wants to know the *propensity* that it was produced by the new machine. Bayes's theorem and some calculations will produce the *probability* that the defective corkscrew was produced by the new machine

but it seems less than reasonable for the inspector to request the *propensity* that the defective corkscrew was produced by the new machine.

What went wrong? *Propensities* seems to make sense as direct probabilities, but not as inverse probabilities. Salmon suggests that this is probably because in the former, we think of them as probabilistic causes, while in the later we cannot because the causal direction is wrong. This is, of course, an illustrative return to the concept of causal asymmetry. Recall CE-1, the flagpole example, that showed even Hempel's D-N model as subject to causal asymmetry counterexamples.

Note also how clearly this example makes the point that probabilities are neutral *noncausal* scorekeeping entities. We have *no* problem talking about direct or inverse probabilities in CE-12. A probabilistic disposition may measure the occurrence of certain causal factors but only because it is being used to measure causality. It could just as easily measure and assign a probability for noncausal occurrences.

c. Railton's parenthetic addendum and its relation to partial entailment. To add a parenthetic addendum to the effect that the event-to-be-explained held, is to acknowledge that the nomothetic structure of the account actually only provides an explanation of the probable mechanisms at work *if* the event-to-be-explained in fact occurred. It is an explanation of the structure of *how* such events often occur. This *how* statement includes a probability determination of how often these events occur as a result of this mechanistic structure compared to the total number of occurrences of these type of events. Note that the explanation does not claim that the event-to-be-explained actually occurred *as a result of* these nomic structural mechanisms, only that its occurrence would be consistent with the results of these

mechanisms. We are left with the strange result that this *deductive* account of how the event-to-be-explained occurred, *may* apply to the event.

Section Four

Paul Humphreys: Causal Explanation

1. Humphreys' agenda: prioritizing causal structure over linguistic form.

Humphreys suggests that many views of scientific explanation assume that there is, or can be, a "correct" form for explanation. This logical structure of explanation is taken as primary, and causal explanations have to conform to this primary form. Humphreys argues that most of the counterexamples to Hempel's models of explanation involve weaknesses of these models in addressing causal issues. He proposes that we should be concerned ". . . to see how the analytic methods of science discover the structural form of causal explanations of phenomena, and then to construct an appropriate linguistic representation which faithfully preserves that structure" (Kitcher and Salmon 1989: 284). An analogy to the thrust of Humphreys' reasoning is the old architectural maxim, "Form follows function," not vice versa.

2. The structural form of causal explanations which must be preserved by linguistic representation. Humphreys argues that in order to convey the causal origins of a phenomenon, linguistic explanations must:

- a. correctly represent the *multiplicity and separateness* of causal influences on a given phenomenon,
- b. correctly represent, where appropriate, the *diversity* of causal influences on a given phenomenon, such as whether the influences are "contributing" or "counteracting" and

c. be able to provide true yet *incomplete* representations of causal explanations since we most often are unable to list all the influences which affected a given outcome.

3. Humphreys' canonical form for causal explanations. Humphreys suggests the following linguistic mode to facilitate preserving the three features of structural form of causal explanations discussed above.

a. Request: "What is the explanation of *Y* in *S* at *t*?"

b. Appropriate explanation: "*Y* in *S* at *t* [occurred, was present] because of *phi*, despite *psi*" where '*Y*', '*S*', '*t*' are terms referring to, respectively, a property or change of property, a system, and a time; "*phi*" is a (nonempty) list of terms referring to contributing causes of *Y* and "*psi*" is a (possibly empty) list of terms referring to counteracting causes of *Y*.

Humphreys designed this linguistic form *to directly mirror the separate structure of causal influences*.

c. Three examples. Contributing cause terminology from Humphreys' canonical form for causal explanations is used in the three examples which follow in order to illustrate aspects of the structural form of causal explanations.

(1) In 1981 physicians began noticing an unusual cluster of cases of formerly rare infections, primarily in young men. They sought explanation for the individual illness in a patient, but they also sought an explanation of the multiple incidence of these symptoms. Humphreys suggests that what the investigators were searching for was not a speech act or a set of sentences but a real thing, a cause of the sickness. The explanation that was found involved a group of retroviruses that cause AIDS. Over an extended period of time, the causal story of the multiple and separate influences related to this illness was filled in so that groups

with different interests—homosexuals, intravenous drug users, public health officials, etc.—could be given portions of the explanation that dealt with their interests.

(2) The simplest explanation of the angular momentum of the earth treats the sun as fixed. This creates a roughly constant angular momentum of the earth, and its value is given by the relevant conservation law. But, even with this simple model, a number of small but important causal influences should also be considered such as the moon's orbital plane, the nonuniformity of the sun's gravitational field, and the action of thermodynamical tides, despite the slowing effects of tidal friction. As consideration of each of these items is added to our original explanation, the explanation becomes more *complete*, but it does not render the prior explanation false, only *incomplete*.

(3) Bubonic plague bacillus will produce death in humans between 50 to 90 percent of the time if left untreated. Treatment with antibiotics such as tetracycline reduce the chance of mortality to between 5 to 10 percent. Infection with the bacillus is a *contributing* cause of death and administration of tetracycline is a *counteracting* cause of death. The cited probabilities show that the contributing cause is not a sufficient cause for death and the counteracting cause does not guarantee recovery. If asked for an explanation of the death of Albert, who contracted the disease and was administered tetracycline, the traditional form of "Y because X" is inappropriate according to Humphreys. He suggests that an appropriate response would be "Albert's death occurred because of his infection with the plague bacillus, despite the administration of tetracycline to him."

4. **Humphreys' ontological definition of an event.** Humphreys is concerned with taking events as concrete, specific entities. He therefore defines an event as *the possession of, or change in, a property of a system on a given occasion*. This, Humphreys argues, enables us

to maintain a proper *separateness* of *multiple* causal influences. Additionally, Humphreys suggests that permanent structures are required for his ontology in order to meaningfully contribute to the existing debate between views which assert that singular causal sequences are instances of primary universal law, and, alternatively, views which assert that general law is nothing more than a collection of singular causal sequences.

With such a material ontological view of events, Humphreys thinks that the consequences of the regularity theorist's answer to this debate are fairly straightforward. Regularity theorists require that an event must be an instance of a universal regularity to count as causal. Given his view of the chaotic nature of the world, there are few, if any, observed universal regularities (except in the lab). If causal laws are identified with observed universal regularities, and singular event sequences are causal only if instances of those causal laws, then there will rarely be causal explanations for singular phenomena (except in the lab). This, in Humphreys' view, is a serious problem for any model of explanation that requires deductive subsumption under universal regularities.

Humphreys argues that the only way such a view may embrace causal explanations and yet retain universality is to restrict initial causal identification to permanent or semi-permanent structures observed in experiments. These are then matched with structures existing outside of experimental contexts.

Humphreys extends this argument to probabilistic causality by suggesting what to him is an "intuitive" picture, i.e., "physical probabilities are dispositional properties, alterations in the structural basis or in the conditioning variables of which result in an alteration of the associated probability distribution" (Kitcher and Salmon 1989: 290). In other words, a probabilistic contributing cause produces an increase in the value of the chance of the effect.

5. Humphreys' thesis that probabilities have no explanatory role. Humphreys notes that every other contemporary account of probabilistic or statistical explanation requires the stating of a probability value. Most often this requirement is because they are all versions of a covering law model of explanation and that all covering law models assign probability value to the explanandum, either absolutely, or conditionally, or relationally. When this is combined with the requirement that all elements of the explanans be true, it results in the requirement that the value cited by the probability law in the explanans must be true.

Humphreys rejects this conclusion for two reasons:

a. In his view this insistence on specifying probability values makes it impossible to separate true explanations from complete explanations, thereby making it rare, if ever, when we can produce such an explanation. The problem of the single case highlights Humphreys' point. Omit even the smallest probabilistic relevant factor and the probability value for that specific single case is incorrect, and so the "explanation" is incorrect too.

b. When a complete explanation is available, i.e., all causally relevant factors have been specified, then a correct reference class is automatically provided by the constituents of *phi* and *psi*. However, even this information will not always provide the probability value since there is no guarantee that all such values are theoretically computable. If probability values are epiphenomena of complete causal explanations then those values have themselves no explanatory power. All that has been omitted from a complete listing of causal factors in a single case is the value of sheer chance, and chance alone explains nothing.

Humphreys argues that the large collection of counterexamples to Hempelian models underlines the fact that real explanations must cite the *causes*, and that good explanations should avoid the conflation of relevant and irrelevant causal factors. For Humphreys, the most

direct way to avoid these problems is to select a linguistic form which directly mirrors the separate structure of contributing and counteracting causal influences.

Humphreys's account is an example of pragmatics in explanation. Under such a view, Railton's concept of an ideal explanatory text would cite relevant causal factors *and* the propensity strengths. The interaction between relevant causal factors and propensity strengths is that information about contributing and counteracting causes is only information about propensities for closely related reference classes.

Section Five

Functional Explanations

A functional explanation is a causal account of the role played by an attribute in the attribute's typical context of appearance. It provides an answer to a "Why?" question by describing the present-day role of the attribute as well as the history of the causal efficaciousness of the attribute in fulfilling that role. It is arrived at by providing:

1. an ascription of a function to an attribute, along with
2. a consequence-etiology of that attribute (Wright 1976:81).

The structure of a functional explanation must contain the two elements set out above. As to the first requirement, Larry Wright suggests that the proper schematic formulation (F) of an ascription of a function is as follows:

"The function of X is Z iff:

- (i) Z is a consequence (result) of X's being there,
- (ii) X is there because it does (results in) Z" (1976:81).

As to the second requirement, the "consequence-etiology of an attribute," this simply refers to the causal story of the capacity of the attribute for producing the desired result ascribed to it.

1. Some examples of functional explanations.

a. Why does a giraffe have a long neck? Note that the ascription of a function to an attribute provides an explanation, e.g., *the function of the long neck of a giraffe is to enable it to reach food that would otherwise be unavailable to it and to many other animals*. The attribute in this case, i.e., the long neck of the giraffe, is explained by ascribing its function, i.e., to reach food not otherwise available. The ascription of a function is obviously a necessary condition for a functional explanation, but it is not a sufficient condition since it does not provide a causal account of how this consequence came to have the capacity to fulfill this function. In this example, the consequence-etiology of a giraffe's long neck might include the attribute's initial appearance as a chance genetic mutation that provided the giraffe and its offspring a survival advantage. While we would not ascribe a function to the initial chance appearance of the giraffe's long neck, we do to its genetic continuation.

Similar examples from physiology or evolutionary biology abound. The function of the stripes on the tiger is to provide camouflage. The function of the heart is to pump blood. The function of the chlorophyll in green plants is to enable them to produce starch by photosynthesis.

b. Why does a steam engine have a governor on it? The function of the governor on a steam engine is to regulate the speed of the engine. It is worth noting that a functional explanation need not be the exclusive form of explanation available in order to be a legitimate form of explanation. As Wright notes, we could provide a completely mechanical account of how the governor works, but that fact does not deprive the governor of its function to regulate

speed. Other examples of both functional and mechanistic explanations that are legitimate different answers to the same factual inquiry most often involve the behavior of human artifacts that have been designed to perform some particular job. This is illustrated by an example such as the actions of a homing torpedo, which like the governor on the steam engine can receive both a mechanistic as well as nonmechanistic (functional) explanation.

c. **Why is a baseball field covered with a tarp when it rains?** The function of the vinyl cover is to keep the field dry. It may also result in other consequences, e.g., catching pools of water, but these are merely accidental results. We can confirm the difference between the two results (determine which is the function) by applying Wright's schematic formulation (F). The dry field is the consequence of the tarp being there and the tarp is there because it keeps the field dry. The same cannot be said about the pools of water collected by the tarp.

d. **Why does a cat "stalk" its prey?** This example begins to appear to involve the concept of intentional behavior. The function of "stalking behavior" for the cat is the ability to be able to sneak up unnoticed on its prey before it attacks them. The consequence-etiology of this stalking behavior is that this type of behavior has been causally efficacious enough in the past to have conferred an evolutionary advantage upon cats and to therefore have become typical behavior for members of their species.

e. **Why do some native tribes participate in rain dances?** With this example, we move to the area of intentional behavior (discussed later under *Problems for Functional Explanations*) and the distinction between *manifest* and *latent* functions. It is the *manifest* function of the rain dance to assist the forces of nature in the production of rain during times of drought, while many social scientists ascribe to the attribute of ritual rain dancing the *latent* function of promoting social cohesiveness in times of distress. Salmon suggests that the latent

function *explains* the continuation of a practice which is not very successful in bringing about its manifest function (Kitcher and Salmon 1989:28).

2. How functional explanations differ from D-N explanations. Functional explanations differ from Hempelian like D-N explanations in that they fail to "cover" the event-to-be-explained with a "law," as that term has been previously defined in our discussion of D-N explanations. Under this view a relevant law of nature is said to "subsume" the specific event since the event is derived from initial conditions encompassed by that law of nature. However, if there were several "equivalent" laws, each of which could "cover" or "subsume" an event-to-be-explained, D-N explanations would be unable to choose between these equivalents in any principled manner. This is the situation for functional explanations, since most functions have "functional equivalents," i.e., more than one way in which to be able to accomplish the function (discussed below in paragraph 1 of "problems").

3. Functional equivalents. Instead of developing long necks, giraffes could have developed the attribute of standing on their hind legs in order to reach food unavailable to other species. Alternatively, they could have developed the attribute of being able to climb tall trees, or they could have become smaller and developed the attribute of flying and landing in tall trees. In any of these changes to our example, the new attribute (standing, climbing or flying) can have the same function ascribed to it of reaching food unavailable to other species. Note that the new attribute is "equivalent" in its capacity to accomplish the same function. Thus, any law-like statement about long/necks providing their possessors with a survival advantage by allowing them to reach unavailable food, might be only one of many similar "equivalent" law-like statements about other ways of accomplishing the same function.

4. Other differences. Functional explanations also differ from Railton's mechanistic account of D-N explanations in that they offer a nonmechanistic causal explanation, even when a mechanistic one is available, e.g., the governor on the steam engine or the actions of the homing torpedo. Strangely, each claims to be a *causal* account but for contradictory reasons.

Note also that the explanandum of a functional explanation *is not* deductively entailed by the explanans together with the initial conditions. If we return to the example of our long necked giraffes, we can note that:

If an animal has a long neck, it can reach otherwise unavailable food,
If an animal can reach otherwise unavailable food, it has a survival advantage.
[So having a long neck provides a survival advantage]
[But this does not entail the following]
Therefore, Giraffes have long necks.

5. Problems that arise with functional explanations. The following are typical problems that can arise with functional explanations:

a. The functional equivalent/logical connection problem. Recall our previous discussions concerning the fact that nearly everyone holds that scientific explanations involve some logical connection between a law-like statement and the statement-to-be-explained. The concept of "functional equivalents" makes it hard to see how this logical connection can be made. In functional explanations the description of the function of an attribute of every organism of a certain type would constitute the law-like sentence. However, if there were several competing ways that these functions could be accomplished (different "covering laws") then this would undercut D-N type explanations since there would be no principled manner by which to choose between these equivalent law candidates.

Contra to the above view, Nagel saw functional explanations as "stand-ins" for more proper D-N explanations. Nagel's approach to this problem was to argue that if one was specific enough about one's description of the kind of organism being dealt with in the law-like statement, only one attribute would have the capacity to perform an ascribed function (Nagel 1961:403-6). The problem with Nagel's view of "dismissing" the serious problem of functional equivalents by defining it out of existence is that while there are instances where this can be done, there appear to be hundreds or thousands of examples (maybe even hundreds of thousands) where it cannot be done. Salmon notes that Nagel confines his examples to biology and avoids areas such as anthropology, sociology, and psychology, which Hempel discussed and which are areas where it is "*much* harder to argue against functional equivalents" than in biology (Kitcher and Salmon 1989: 193, fn 50).

While Salmon attributes Nagel's restricted view to his concentration on biological examples, perhaps it is more likely that it was Nagel's familiarity with the physical sciences that caused him to minimize the problem of functional equivalents. As Rosenberg points out, examples of structural diversity with equivalence of effects are rare in the physical sciences, while natural selection in the life sciences "makes equivalence cum structural diversity the rule and not the exception" (1994:33).

As Hempel perceived it, the concept of "functional equivalents" takes away the predictability of the fact-to-be-explained being *expected* by virtue of the explanatory fact. For this reason, Hempel did not see functional explanations as "admissible" explanations. Salmon argues that this is a problem for Hempel's view, not for functional explanations which he argues play a legitimate scientific role (Kitcher and Salmon 1989:31).

b. **The claim that functional explanations are not legitimate explanations because they are teleological.** Functional explanations have long been criticized as *teleological* - from the Greek "telos" meaning end. In other words, the present function is explained by a future end, e.g., the long neck of the giraffe developed so that *in the future* it could eat food not otherwise available to it. But, goes the argument, these types of explanations cannot be causal since the future event (eating from tall trees) cannot explain the past event (developing a long neck).

Since Darwin, this type of objection is dealt with by the theory of natural selection. Darwin argued that biological phenomena are the result of a large amount of blind, random inherited genetic variation. Survival among the fittest of a variety meant reproduction of that species into the future. But the initial field of candidates for survival were created by a random recombination of genes, or mutation, or some other causal process. The important point to note for this discussion is that whatever the process that created the survival candidates, it was, according to Darwin, a nonteleological process. In our example above, the long neck of the giraffe was not created *for* reaching unavailable food, but the present day giraffe may well have survived instead of other genetic giraffe variants *because* the long neck allowed it to reach food unavailable to nonsurviving candidates. Thus, when we ascribe a function to this attribute and describe its consequence-etiology, it provides a causal account.

Ascribing functions to biological phenomena seems convenient shorthand for discussing an underlying completely causal mechanism. However, it is not at all clear that the same strategy may be used to eliminate teleology in the form of conscious human intentional behavior and related functional explanations from anthropology, sociology and psychology.

c. Functional explanations and the problem of teleology in individual intentional human behavior. We often describe our behavior in a teleological manner. Why did I go to the New York Bagel shop? To get a dozen "everything" bagels. It is not my future act of procuring bagels that explains my behavior, but instead my preceding desire for the "everything" bagels along with a belief based upon prior experience that this is the only place in town that makes them. Even if the shop is out of "everything" bagels, the explanation based upon my prior desire is still accurate (but not if the explanation had been based simply upon "behavior"). Goal-directed behavior in humans is preceded by *prior* goal-directed beliefs and/or *prior* goal-directed desires.

But even if we want to call our descriptions of our behavior teleological, we cannot call them functional. In response to the question, "Why do I go to the New York Bagel shop?" it is not proper to ascribe the function of the attribute of my traveling to the New York Bagel shop in order to purchase "everything" bagels. This is because the present-day role of my travelling to the New York Bagel shop will vary from day to day based upon the then existing content of my beliefs and desires. If I want an explanation of my individual action, I need to examine those individual desires and beliefs, not ascribe a function to that individual action. Simply stated, functional explanations do not seem to accomplish any work in explaining *individual* intentional human actions. It does not appear to be a very helpful tool for explanation in psychology (for an excellent discussion of problems related to the explanation of *individual* human action, see Rosenberg 1988:22-49).

d. Functional explanations in anthropology or sociology and the related problems of holism. Functional explanations within the social sciences of anthropology and sociology are often associated with "holism" - taken from the Greek *holos* meaning "whole." It

is the concept that the object of scientific study within these disciplines is large groups of people and the institutions in which their interaction consists. Holists quite often argue for the existence of "social wholes," theoretical entities which are not reducible to mere collections of individual human agents which constitute them; the whole is made up of more than the sum of its parts. Thus, functional explanation within these disciplines is associated with the strategy of identifying and explaining features of society in terms of the purpose they serve for society as a whole, instead of by appeal to the behavior of the individuals who comprise these social wholes.

In our prior example concerning the function of the rain dancing, we discussed both *manifest* and *latent* functions. Note that these are manifest and latent functions for the *group*. But if these are functions that fulfill a role for the group, how is it that society arranges such institutions to meet its needs? Durkheim spoke of a "collective consciousness," a group mind that was responsible for such arrangements. More than a few scholars have found this response too abstract. And, even if it isn't, aren't we talking about teleological explanations again, fulfilling needs (functions) in the future as an attempted causal explanation of our actions in the past?

Section Six

Heuristic Explanations

For many, the term "heuristic explanation" is internally inconsistent. Philosophers in the hermeneutical tradition argue that human behavior must be understood as meaningful and that such understanding requires *interpretation*, not *explanation*. Within this tradition, interpretation is viewed as a process not amenable to *naturalism*, i.e., the methods developed

within the natural sciences based upon the discovery of general regularities. Under such a view, there can be no heuristic "explanation" so long as "explanation" implies a theoretical process restricted to the use of methods developed within the natural sciences.

A different argument with similar conclusion is made by philosophers in the positivistic tradition. They argue that *interpretation* does not meet the scientific standards of *explanation*. Under such a view, the problem is not with the standards regarding what counts as an "explanation," but instead with the methodology involved with *heuristic interpretation*. Thus, this tradition also argues that there can be no "heuristic explanation."

While the latter tradition argues for upholding a single scientific standard (naturalism) thereby dismissing the claims of interpretatists as non-scientific, the former argues for a pluralism of standards (naturalism for explanation, antinaturalism for interpretations). Under both views, *explanation* is viewed as strictly a naturalistic concept. Thus, for there to be an acceptable and internally consistent sense of a "heuristic explanation," it must be one consistent with our prior discussions concerning forms of explanation while availing itself of additional theoretical benefits from the use of interpretative methodology.

As the comments above suggest, philosophers within the heuristic tradition offer no proper "form" for heuristic interpretation. To the contrary, those within this tradition might consider the request for some type of objective "form" as a request for a subjective or contextual "prior understanding" or background assumption with which to begin the interpretative process.

If we maintain the requirement that "explanation" be viewed as a naturalistic concept, then any argument that included a claim of being in a proper form of a "heuristic explanation" would, by necessity, provide a naturalistic form of explanation from outside the heuristic

tradition. Additionally, it would have to further argue that heuristic methods could be employed within that form of explanation.

1. The contrast between heuristic explanation and a D-N type

explanation. For the antinaturalist philosopher in the hermeneutical tradition, *interpretation*, not explanation, is the process of understanding meaning. Interpretation is argued to be an appropriate alternative to formal D-N types of explanation because:

1. human behavior is meaningful,
2. understanding meaning relies on prior understanding,
3. meanings can be understood in multiple ways because interpreters may differ in their background assumptions,
4. interpretation requires reference to the subject's own categories, and
5. interpretation is contextual (Kincaid 1996:205).

Under this view, the prior understanding used to provide meaning to the actions of others creates what Dilthey called the hermeneutic circle which ultimately returns to its point of beginning, i.e., the prior understanding of the interpreter. Thus, it is argued that there is no way to break out of the circle and "cover" the behavior and its "meaning" with non-subjective or non-contextual laws. It is argued under this view that this is so because either the requirement for including the subject's perspective makes the interpretation subjective or the law-like premises that would be used to "cover" these events are not universal since such interpretations are limited to the context within which they arise. The latter is so because Dilthey stressed the need for the intuitive understanding of specific cultures or world views, as opposed to universal similarities present within all cultures.

Harold Kincaid suggests that a classic anthropological example of heuristic interpretation or explanation is found in anthropologist Clifford Geertz writings concerning his observations in Bali. Geertz interprets the Balinese cockfight as a "sustained symbolic

structure" which provides a commentary upon "sorting human beings into hierarchical ranks," an interpretation beyond that which the Balinese assert on behalf of their own sport or its participants. Kincaid suggests that while it is often difficult to discover whether Geertz is referring to symbols for the Balinese or for western observers when he is discussing cockfights, the same is not necessarily true for his interpretive work on Balinese kinship. There, Kincaid suggests that Geertz provides a "thick" description of how they see the world and the meanings they provide concerning their institutions, but he tests his insights against standard naturalistic methods while seeking causal explanations for his understanding of these meanings. It is for these reasons that Kincaid argues that while Geertz's symbolist commentary on Balinese cockfights may not qualify as a "heuristic explanation," Geertz's work on Balinese kinship should.

2. Problems that arise for heuristic interpretations/explanations. The following are problems that can arise for heuristic explanations:

a. The individualistic bias inherent in the arguments which are offered to justify the use of heuristic interpretation as an antinaturalist methodology. Recall that in looking at how heuristic explanation differs from D-N type explanation, we listed the arguments supporting interpretation as a methodological tool. Those arguments are used as premises for a larger argument to the effect that naturalistic forms of explanation are not appropriate for the social sciences. However, in those arguments, the meanings discussed are *individual* meanings, the interpretations made are by *individual* interpreters, the reference to categories used is made by *individual* subjects and the context alluded to is the context surrounding *individuals* as opposed to that surrounding the group.

All of this implies that there is only one way to look at human social explanation, i.e., in terms of individual behavior and individual cognitive states. Such a presupposition is contrary to the tenets of functionalism or macrosocial views such as holism. When social science is about the larger scale, it need not be about individual meaning. In fact, macrosocial science may even bypass any need or requirement that it explain at this level in terms of meaning. And, even if macrosocial science should seek some explanation in terms of meaning, such meaning would be *group* meaning, not *individual* meaning. Group meanings are attributed holistically. Thus, they are more capable of being tested against holistic accounts of social processes and structures. As our prior discussion of functional explanations displayed, many macrosocial explanations are consistent with naturalistic methodology.

b. The claim that since interpretation requires reference to the subjects's own categories that this inclusion of the subject's perspective makes interpretation subjective. The heuristic tradition seems correct in suggesting that subject categories are potentially relevant evidence for consideration by social scientists and that the effect of this will be to entertain much subjective evidence. However, to claim that this alone makes all interpretation subjective is overstatement. If there are objective views about a particular subject matter, then the holding of such an objective view by a subject does not thereby make the view subjective in the sense of being not factual or non-objective. And, for the heuristic tradition to go further and make the claim that there is no objective fact about any subject matter would seem to be a self-defeating argument.

c. The claim that since interpretation requires reference to the context within which the behavior occurred, interpretation is always contextual and not subject to being categorized within universal natural science claims. While it does seem true that

behavior is contextual, it also seems true that numerous claims and concepts in the natural sciences are also contextual, e.g., the concept of "fitness" as applied within the natural sciences is obviously contextual. Contextuality seems more of a complication for naturalistic confirmation and explanation than a condition that is impossible to fulfill by naturalistic methodology.

d. Rosenberg's argument from many models. Alexander Rosenberg argues that due to the holistic nature of belief-desire attributing, the same behavior can always be explained in different ways. In other words, we can always attribute different desires based upon different beliefs behind the same behavior. Each of these newly attributed systems have the same explanatory power, i.e., we won't have any good way of eliminating competing causes. While this argument seems too strong as a criticism of all interpretive practice, it does quite often seem particularly telling as a criticism of normative social explanation as well as symbolist social explanation (discussions of both follow).

e. Normative social science. Within both sociology and anthropology, much work is produced that refers to "normative" meanings. This generally refers to rules, norms, conventions, roles, or other similar concepts which separate human behavior into limited categories such as the following:

- (1) rules list prescriptions on behavior (even when they are not followed),
- (2) norms are prescriptives actually reflected within behavior,
- (3) conventions are voluntary agreements arrived at through the pursuit of self-interest, and
- (4) roles are some more complex combination of rules, norms and conventions.

Interpretivists often claim that understanding normative meaning involves special methods which provides antinaturalistic "explanations," i.e., not explanations of any kind covered in our prior discussions of explanation.

A common example within anthropology is the concept of "participant observation," where the interpreter participates with the cultural subjects in order to learn how to "act like a native." But, like any other scientific venture, the eventual object is insight into that particular behavior, in this case through a study of a particular rule or norm. If the anthropologist can formulate the rule explicitly as a result of such participation, then the anthropologist should also be able to articulate the evidence observed during participation which supports the new found ability to articulate the rule. Nothing about the process need abrogate the principles of scientific confirmation.

A related question is whether or not providing normative meaning counts as some special type of explanation. If we hold that explanations are causal in nature, then we would require normative "explanations" to trace out and tie the questioned behavior to causal processes underlying that same behavior. To the extent that normative explanations accomplish this task, they "explain." However, to the extent that normative explanations do not accomplish this task, they fail to "explain." Note, however, that the failure to explain does not mean that they do not provide valuable information anyway.

f. Symbolist social explanation. Symbolist anthropologists share the beliefs that (1) good social science involves the discovery of the symbolic meanings of everyday social practices and (2) that these symbolic meanings are not necessarily the ones explicitly expressed by the involved individuals. Symbolism has been criticized as seeking meanings used by no one, meanings not always acknowledged by the actors involved. However, in fairness, the same

charge could be made against some of the practices within the naturalistic tradition which refer to "hidden" meanings as well as unconscious or tacit beliefs in ordinary explanations of behavior, e.g., the difference between "latent" and "manifest" functions.

Still, a basic problem faces symbolism. That is the fact that we don't have a very good idea of just what implicit or unconscious belief amounts to. Thus, we don't have definite limits concerning its attribution. Multiple interpretations are therefore an ever-present problem. Different anthropological approaches often produce different results concerning the same subject matter. This creates a further inability to produce much common ground upon which testing can be used to eliminate competing hypotheses.

Lastly, symbolist social explanations face the same problem as does normative social science concerning whether or not such explanations expose or tie causal roles or forces.

3. Summary. There *is* a difference between physical laws and human meaning. One difference between the antinaturalists' search for explication of human meaning through interpretation and the naturalists' search for explication of nature through laws, is that physical laws and the states of affairs they constitute must be *discovered*, while humankind's meanings must be *constructed*. Thus it is argued that the methodology for discovering and explicating the physical aspects of the world, may not always be the best tools for constructing and explicating cultural conventions. Enculturated concepts such as God, heroism, honor, shame, nationalism, or other similar concepts have been present in all cultures and may well have been created by differing human activities that did not always follow historic patterns, much less naturalistic ones. As a result, other methods may sometimes better approach the causal ties or forces behind such constructed meanings. When interpretation intelligently ties our understanding to relevant causal forces, then arguably it has participated in explanation.

Yet, there seems a real problem in identifying in what way interpretation ties our understanding to underlying causal forces. Unlike any other views concerning explanations that we have discussed, interpretation does not promise a logical connection between the statement-to-be-explained and any type of universal law-like statement. This is because it seeks understanding specific to a non-universal, unique society.

Some human activity may be universal and thereby capable of naturalistic explanation, particularly at the macrosocial level. However, when we are unable to make such universal ties, an alternative approach, seemingly often used but seldom discussed, is the attempt to combine the best of both the naturalistic and the heuristic traditions. The primary element necessary for naturalistic explanation that is often missing in studies within the human sciences is the concept of *universality*. Thus, the inability exists within these studies to make a logical connection between a statement-to-be-explained and any type of universal law-like statement. However, within individual cultures, a logical connection *could often be* maintained between innumerable statements-to-be-explained and culturally contextual law-like statements that apply to that particular culture.

Since meaning was constructed within cultural contexts, perhaps that is where it often must *initially* be explained. The most promising area to find underlying causal forces should be within the history of the development of that meaning. An understanding of the causal forces which underlie cultural human activity within separate societies could provide a starting point for comparative studies aimed at discovering universal causal forces underlying human activity. Since these types of comparative studies would contrast much contextual information, it would seem critical that they be developed with the assistance of as much of the rigor developed within naturalistic methodology as is possible.

Section Seven

A personal overview

As the preceding summary indicates, it is tempting to argue, as Rosenberg and, to a lesser extent, Kincaid do, that a methodological rift exists between different forms of explanation, i.e., naturalism and antinaturalism. Such a view finds logical positivism on one extreme end of a continuum of positions with heuristic interpretation on the opposite extreme end. The question then becomes where the methodology of the social sciences should be located within that continuum. Rosenberg in effect argues that it is located closer to the heuristic polar position than to the positivistic position and thus, the social sciences cannot successfully participate in naturalistic science. Kincaid places the social sciences closer to the center of this continuum and argues that naturalistic methodology can be implemented within the social sciences.

While these arguments seem relevant, their shortcoming is that they emphasize the differences between the accounts we have discussed instead of focusing on what they share in common – the search for causality. I would want to argue that the architectural maxim, "form follows function" refers to an equally important fact in the field of scientific explanation. While all of the accounts discussed herein differ in matters of form, each is considered to function properly when it provides *causal* answers to "why?" questions. As Humphreys notes, real explanations must cite the *causes* while separating relevant and irrelevant causal factors. Accomplishing this task is what makes the explanation "scientific," not the nature of the form used to provide that explanation.

Thus, if we concentrate upon the notion of causation, we can employ different types of explanation to explain different "why?" questions and sometimes, we can even employ different types of explanation to explore different aspects of the same ideal explanatory text. Under such a view, we can receive causal explanations from formal D-N type explanations as well as their antithesis, heuristic explanations.

Contra to this argument is the justification used by many for the use of *interpretation* as an indispensable alternative to formal concepts of explanation. That argument is based upon the claim that interpretation is necessary to find *meaning* and that *meaning* is not a causal product (e.g., Rosenberg 1988:17).

This simply seems wrong.

Meaning is created by humankind. That creation process is both historical and social. It is *historical* in the simple but non-trivial sense that there was a time in which any particular meaning did not exist. It subsequently came into existence by human creation, perhaps evolving over time by the addition or subtraction of content. It is *social* in the sense that it arose in group settings. While an initial thought behind an eventual meaning can be created at the individual level, meaning only has significance when it is shared. It can only be shared in some form of social activity; usually, the greater the significance, the greater the social sharing.

An ideal explanatory text of a particular meaning will contain much individual, subjective and contextual data that will normally not be captured by any form of explanation. Nevertheless, much of the historical and social causal processes that were involved in the creation of humanly significant meaning *can* often be captured by macrosocial versions of explanation. However, we should note with a certain amount of caution that the concept of an ideal explanatory text is an *atemporal* concept in that it implies a conclusion, a looking back

from the end of time. Naturally, from our perspective, human *meaning* must be a temporal concept, albeit viewed at atemporal moments, but always subject to redefinition when it is confronted with new causal influences or when new effects are produced from similar causes.

These comments display my distinct bias toward indeterminism, a view shared by many social scientists who have long viewed the social world of humankind as indeterminant based upon the indeterministic nature of human conduct (Rosenberg 1988:19-20). For the social scientist, examples abound where the "same cause" behind human conduct does not always produce the "same effect," but instead may produce unpredictable and unprecedented forms of social action.

An example is the classic work on metaphor by anthropologist Victor Turner. In it, Turner provides an analysis of Thomas Becket's murder and subsequent canonization (Turner 1974:13) as an example of how metaphor and paradigm create new values and therefore new behavior. Becket's murder became the focal point for an ensuing new cultural meaning which transformed then existing conditions with unanticipated effects.

While Turner's discussion displays the indeterminate nature of human created meaning, it also provides an excellent example for our discussion in that despite the unanticipated meaning produced by Beckett's murder, Turner is still able to trace the historical and social origins of that meaning. He does this by tying the resultant meaning to the contributing and counteracting forces that created it while tracing them through social institutions and group activities.

In the sense illustrated by Turner, the search for human created meaning is strictly a causal process, while current *interpretations* are mere atemporal moments in this causal time line. When those atemporal interpretations are reliably tied to their historical, social and

temporal etiology, they become causal accounts. When they do, they become explanations, regardless of their form.

¹ The purpose of this chapter is to discuss current accounts of scientific explanation as a prelude to a specific discussion concerning functional explanation in the biological and social sciences. In that subsequent discussion I endorse a view of explanation that attempts to be consistent with theoretical insights discussed here. The collection of material for chapter one was made much easier by the fact of the existence of, and my substantial reliance upon, the classic collection of material in *Scientific Explanation, Vol. 13, Minnesota Studies in the Philosophy of Science*, edited by Philip Kitcher and Wesley C. Salmon.

I am particularly indebted to the discussion of existing theories of explanation and related problems as specifically treated in Salmon's article, "Four Decades of Scientific Explanation." Salmon's catalogue of the variety of philosophical accounts of explanation is remarkably comprehensive and I have not attempted to improve upon it here. My primary reason for retracing many of Salmon's steps here is that a least a portion of my audience, anthropologists and other social scientists, may be unfamiliar with this philosophical literature.

² Most of these counter-examples have been collected by Paul Humphreys. He uses them to illustrate the need for explanatory schemas to address causal issues.

Chapter Two

FUNCTIONALISM

It is the purpose of this chapter to set the table for subsequent discussions that ultimately will advance a version of functional explanation for particular use by social scientists. This account is based upon the broader view of explanation as causal, partial and cumulative. I begin previewing this perspective at the start of the chapter in order to provide an overview of the path upon which this argument will ultimately traverse. I touch briefly on the history of functionalism and begin an in-depth review of some of its classic criticisms. Functionalist theory has performed yeoman work within sociology and anthropology that has been well documented. It is my hope that a review of its equally well-documented theoretical shortcomings will act much like an architect's "punch list" of difficulties that a successful account of functional explanation must correct. A parallel objective is to rekindle the desire to recapture this type of social understanding that originally attracted scientists to this theoretical model.

I begin a discussion of the classic criticisms directed against functional theory in the social sciences by examining the multitude of problems created by the inappropriate organic analogy with which functional theory originated. First to be addressed among these problems is the metaphor problem and the resultant loss of causality which is produced. I next outline the nature of the system boundary criticism as it relates to the implementation of an organic analogy. Thereafter I outline the problems inherent in viewing the human system, and by analogy the social system, as a homeostatic process with a built-in cybernetic mechanism.

Following that discussion, I address the substantial charges leveled in the 1960s that functionalist theory was unable to handle the concept of change over time. Lastly, I address three unnecessary postulates: the postulate of the functional unity of society, the postulate of universal functionalism and the postulate of indispensability.

At the end of these discussions, I argue that an obvious first step in correcting these problems is to do away with the inappropriate organic analogy. I argue that the second step toward a viable concept of functional explanation begins by rethinking the roles played by paradigms in the social sciences. I suggest that the search for explanation in anthropology and sociology should focus upon the causal roles of events, behaviors and social institutions. In so doing, functional explanatory theory should be seen as causal, partial and cumulative.

Section One

The Nature of Social Theories

I want to begin with a review of the social doctrine of functionalism, but enroute I want to make some more general comments about social theories. Anthropologist Erick Wolf claims that anthropologists are "continuously slaying paradigms, only to see them return to life, as if discovered for the first time." Speaking to other anthropologists, he states "(w)e are all familiar, I trust, with Robert Lowie's image of 'diffusionism laying the axe to evolutionism.' As each successive approach carries the axe to its predecessors, anthropology comes to resemble a project in intellectual deforestation" (Wolf 1990: 588). Wolf's plea is that social theories should be used as *cumulative and compatible* tools for learning instead of *competitive* axes used for the destruction of prior views as well as for the destruction of the intellectual models

which produced them.

I want to suggest that this is possible by rethinking the roles played by paradigms in the social sciences. Implied in Wolf's lament for cumulative social theorizing is that new theories within anthropology are perceived as *exclusive* paradigms, thereby requiring the "axing" of prior theories. Exclusivity is not a necessary condition for successful social theorizing. I want to argue exactly the opposite — that social theories can and should be viewed as both partial and cumulative. Specifically, I am going to suggest that functional explanation (as later discussed herein) is one such partial and cumulative paradigm of social explanation.

This view of social explanation rests upon the following two central theses: (1) that causation, not mere form or structure, should be the principal focus of scientific explanation, and (2) that all explanations are partial, that complete explanation is an ideal that can never be attained. These theses underlie the discussion throughout the balance of this paper.

Wesley Salmon has long been a proponent of the causal basis of explanation. And Paul Humphreys has long argued that the weaknesses of Hempelian models in addressing causal issues demonstrates the need to be concerned with discovering explanatory causes rather than focusing on the linguistic form of explanations. Peter Railton's concept of an *ideal explanatory text* greatly enhances this view of explanation. He argues that given an event that we wish to explain, an *ideal explanatory text* would completely spell out all of the causal and nomic connections that were relevant to the occurrence of the event-to-be-explained. Such an account would be extremely large and complicated, an ideal that could never be completely realized. Scientists must be content with finding *explanatory information* that enables them to fill in parts of such an ideal explanatory text. While the ideal text is never completely filled in,

the ideal represents a framework that provides some understanding of the role played by various competing explanations.

These views suggest that the search for explanation in anthropology and sociology should focus upon the causal roles of events, behaviors, and social institutions. Further, since any explanation will only be partial, it will be important to assess where it fits into the broader ideal explanatory text. Thus, we may sometimes employ different types of explanation to explore different aspects of the same portion of a subject matter, as when we contrast the information provided by a present causal role analysis with the information concerning the origination of the function of the same attribute in the past. Such a view allows explanation in the social sciences to incorporate historical lessons from the past with ahistorical accounts of the same human problem or condition. This type of explanatory flexibility is critical to the social sciences since explanation there is sufficiently formidable without adding insurmountable conditions such as requiring explanatory models to provide exclusive and/or complete explanations.

Section Two

Functionalist Social Theory

For over a hundred years, social scientists, particularly those in anthropology and sociology, have employed versions of functionalist social theory as introduced by Emil Durkheim as the primary means of explaining social structures and actions. It is not an unrelated fact that sociologists refer to Durkheim as the "Father of Sociology," while anthropologists bear him similar intellectual reverence. "(T)he idea of 'a society' was

developed in functionalist social anthropology as a bounded universe of self-reproducing structures" (Keesing 1994:301). Just as different parts of the body work together to maintain physical health, social functionalists hold that social institutions support one another to maintain a healthy, working social order (Borofsky 1994:244). "To explain a social fact," wrote Durkheim (1938:97), "we must show its function in the establishment of social order." As Durkheim's theory was carried forward by Parsons in sociology and Radcliffe-Brown in anthropology, it emerged as a theory "that most or all institutions exist in order to maintain social equilibrium or societal survival" (Kincaid 1996:105).

This theoretical approach was so successful that functionalism has been the theoretical mainstay of these two disciplines ever since. Part of its attractiveness is the fact that functionalism introduces an interest in identifying and comparing functionally significant relationships among cultures. Its longevity in the social sciences suggests that it has provided valuable insights for social scientists. Yet, as a social paradigm it has been the subject of much criticism as an inflexible model which assumed present conditions as part of a non-changing system. For an ever-increasing number of social scientists, functionalism's rigidity makes it poorly structured to handle societal change.

Section Three

The Criticisms

Despite the significant role played by different accounts of social functions during the last century, these accounts have been subject to considerable criticism, much of which is justified. Functionalism has been charged with being grounded on an inappropriate organic

analogy; of being unable to define the boundaries of a subjectively created and assumed system; of being a homeostatic model which seeks to maintain social order or social survival; of being a static account which is unable to account for change over time; of making non-verifiable structural assumptions upon which purported social functions are grounded; and, of incorporating unsupported universal assumptions.

Each of these criticisms is to some extent meritorious. Part of the reason for this is the multitude of problems created by the inappropriate organic analogy. An obvious first step in correcting these problems is to do away with whatever inappropriate baggage is carried by the analogy. Although suggestive, the *analogy* can itself do no substantive work. If prior learning provided by the functionalist model is to be preserved as cumulative and compatible knowledge, some judicious pruning of this data must occur in line with the insights provided by many of the past criticisms of this model. In Chapter Four I will argue for an account of functional explanation that I there suggest can circumvent each of these criticisms. There I address how functional explanation can provide legitimate causal explanations. First, however, a closer look at each of the classic criticisms of social functionalist theory is in order.

1. **The inappropriate organic analogy: the metaphor problem and the resultant loss of causality.** The explicit nature of the organic analogy is clearly set out in Radcliffe-Brown's often quoted statement that "every custom and belief of a primitive society plays some determinate part in the social life of the community, *just as* every organ of a living body plays some part in the general life of the organism" (1964:229) (emphasis added). An obvious point of departure is to inquire as to why that should be the case. Why should the roles of customs within different societies be related to their societies in the same manner that parts of the body

are related to the body as a whole? What makes that relationship similar, what is the basis for such a claim?

Radcliffe-Brown's claim is supported solely by an argument from analogy, which either stands or falls on the appropriateness of the analogy. It surely must be conceded that there is no natural relationship between the development of customs within cultures and the development of organs within the human body. If it works as an argument, it works only as a pediological tool – a metaphor.

The problem here is one well illustrated by anthropologist Victor Turner in his seminal work on metaphor, *Drama, Fields, and Metaphors*. Turner concerns himself with the impact on our view of the world as a result of "root" or "core" metaphors with which we characterize social activity. Turner acknowledges the archetypal role played by metaphor as a method of proceeding by analogical extension from the known to the unknown. However, he cautions that "one must pick one's root metaphors carefully" (1974:25).

I borrow from that discussion the following two concepts: (1) that "(a) metaphorical statement has two distinct subjects—a principal one and a subsidiary one," and (2) that "(t)he metaphor selects, emphasizes, suppresses, and organizes features of the principal subject by implying statements about it that normally apply to the subsidiary subject" (Turner 1974: 19-20). Turner's point is that metaphor attempts to explain the *structural characteristics* of the principal subject (the new or less known subject) in terms of the better-known or more familiar subsidiary subject. For instance, a *mechanical* metaphor of the universe explains the structural characteristics of the universe (the less known subject) as organized in a manner similar to that of a machine (the familiar subject). This explanation in turn suggests further implications, e.g.,

that the universe is deterministic, like the action of a machine.

The first problem entailed by Radcliffe-Brown's approach was to organize the parts of the society to be studied (the principal subject) in a manner similar to the structure of the human body (the subsidiary subject). Thus, social parts needed to be labeled and organized like human body parts for the analogy to work. However, when this is done, 'structure' becomes something like an equivalent to 'organization' or 'arrangement.' In other words, 'structure' becomes a descriptive concept without explanatory significance. Thus, Radcliffe-Brown's and Malinowski's structural-functionalism has been accused of analyzing the patterns of social relations without any attempt to identify any underlying causal role for the given structure to play (Tilley 1990:5). Both begin with "the notion that a culture or a society is an empirical whole made up of a limited number of readily identifiable parts and that when we compare two societies we are concerned to see whether or not the same kinds of parts are present in both cases" (Leach 1961:6). But neither says much about what these parts are there for, what the parts do in maintaining the health of the whole.

While this system of classification provided convenient categories for separating accumulated data into smaller and more manageable groupings, it did so without providing an analysis of the role performed by these parts. Foregoing the search for a causal, functional role proved to be too high a price to pay. The creation of this type of noncausal, ahistorical "functional" categories explain too little. The only explanation offered by this methodology is the one assumed in the beginning, that the parts play some determinate role. Not only does this type of classification fail to answer *what* causal role the parts presently perform, it also fails to answer *why* these functions came to be, as well as *why* they persisted over time.

2. The inappropriate organic analogy: the system boundary problem. A more sophisticated version of the organic analogy was that proposed by Durkheim and rejected by Radcliffe-Brown. From the very beginning Durkheim had conceived of societies as complex natural systems. As the subsidiary subject (the human body) had different parts which worked together to maintain *physical* health, by analogy, the principal subject (society) had different social institutions which supported one another in order to maintain *social* health (order, stability, or integration). As different parts of the human anatomy could be understood to function in a manner to maintain physical health, by analogy, these social entities were also understood to function in a manner to maintain social health. In order to explain his concept of social 'facts', Durkheim argued that "it is not enough to show the cause on which it depends; we must also, at least in most cases, show its function in the establishment of social order" (1938:97).

But while the subsidiary subject (the human body) has very definite, objective boundaries, this is not so for the principal subject (society). The boundaries of social systems are not objectively formulated in as clear a manner as are the boundaries of the human system. The boundaries between groups, institutions and countries tend to blur more and more daily in the ever increasingly multicultural world within which we live. As the roles played by these groups, institutions or countries becomes more complex, the disanalogy between these social boundaries or roles and those of the human body escalates. The eventual consequence of this growing disanalogy is an ever-increasing loss of explanatory relevance. The analogy comes to be less and less objective in the sense of representing operative conditions within the social world.

It was this lack of an objective formulation of the principal subject's system (society) within which the social organs "function" that bore the brunt of much criticism from theorists committed to a linguistic social model as opposed to an organic model. This "network of causally and functionally related empirical entities forming natural systems" argued anthropologist Robert Murphy, was "based on a positivist belief in the autonomy and objectivity of the scientific observers" (Murphy 1990:332). Note that underneath the postmodernist suggestion of cultural subjectivism lies the specific target of Murphy's objection, system boundaries. This is not a trivial objection. To the extent that functionalist social theory rests on a biological *analogy* for its plausibility, it is only theoretically plausible to the extent that the analogy holds up. If the analogical argument for functionalism is that X performs function Y in system S like X1 performs function Y1 in system S1, then it would seem to be a serious problem for functionalist theory if system S cannot be individuated because its boundaries are completely indeterminate, particularly when its analogical equivalent has crisply defined boundaries.

This is a problem that has not gone away for anthropologists. It has recurred in later concepts. As Erick Wolf noted, "Functionalism overreached itself by claiming to depict organic entities, but returned in systems theory as well as in other disguises" (1990:588).

3. The cybernetic mechanism problem. The human system is often seen as a homeostatic process with a built-in cybernetic mechanism that functions to protect the body's physical health. Thus, by organic analogy, functional social health or unity "is a condition in which all parts of the social system work together with a sufficient degree of harmony or internal consistency, i.e. without producing persistent conflicts which can neither be resolved

nor regulated" (Radcliffe-Brown 1952:181). The projection of a cybernetic mechanism into a theory of social functions is extremely problematic. To illustrate why this is a theoretical dead end requires some detail.

In a cybernetic process, *information* is "fed" forward or back, thus producing the familiar term *feedback*. We normally talk about *feedback* as a process of information flow occurring within a *closed-loop*. The loop is referred to as "closed" to designate the interacting flow of information among specific components of the system. The information flowing through the loop pertains to the system's present state and to relevant features of the external environment being monitored by the system's sensors. The role of a component that receives the information is to exercise control in the attainment of established goals by minimizing error in the work performed by the system. *Negative feedback* thus occurs when information about the amount by which the system failed to accomplish its goal is fed backward to modify the behavior of the system by modifying the actions of its components.

The classic illustration of a negative feedback process is that of a thermostatically controlled heating system. The external environment of the system is effected by the output of the system (heat). Information about a relevant change in this environment (temperature) is fed back into the system where it is compared to an internal system state (preset temperature of thermostat). The difference between the two (the negative information which was fed back) causes a correction to be made to minimize the *error* by turning the heater on or off.

As the thermostatically controlled heating system example illustrates, the *control* process is involved with the interaction between the internal system state and its immediate external environment. The internal system is involved with the external environment at least to

the extent that external events intrude upon it in a way that causes an interruption in the system's equilibrium state, which then causes the system to react in an attempt to regain equilibrium.

This coordinated response of the parts of the system to external stimuli reflects the homeostatic condition of the system to tend to maintain a kind of internal stability. We can easily extend this to a physiological example, the 37 degrees C at which the human body maintains itself. When the human body is immersed in a cold environment and its temperature drops, this information is detected, passed to an internal system that responds, i.e., the body starts to shiver until the energy expended by this shivering raises the temperature back to its equilibrium state, 37 degrees C. To offer a functional explanation as to why the shivering occurred is to suggest that shivering has the function of returning the body to its homeostatic condition.

The tendency of the parts of the system to protect the stability or survival of the entire system in biological examples has made this seem an attractive analogy for a sociological model. In such a model, the parts of the social system are seen as attempting to maintain homeostasis within the system, regardless of the nature of the external environment. Durkheim was obviously attracted to such a biological analogy. Note the homeostatic control concept in the following excerpt from Durkheim's *The Rules of Sociological Method*:

Indeed, if the usefulness of a fact is not the cause of its existence, it is generally necessary that it be useful in order that it may maintain itself. For the fact that it is not useful suffices to make it harmful, since in that case it costs effort without bringing in any returns. If, then, the majority of social phenomena had this parasitic character, the budget of the organism would have a deficit and social life would be impossible. Consequently, to have a satisfactory understanding of the latter, it is necessary to show how the phenomena comprising it combine in such a way as to put society in *harmony with itself* and with the *environment external to it*. No doubt, the current formula, which defines social life as a correspondence between the internal and the external milieu, is only an approximation; however, it is in general true (1938:97) (emphasis added).

Despite their *prima facie* attractiveness, homeostatic processes are not good models for most functionalist explanations. In such processes the stability or survival of the system itself is what is being protected by the parts. In our example above only one thing protects the system, the shivering, the reactive event which raises the temperature back to homeostasis. The loop is merely detecting and transmitting information while the control mechanism is simply making adjustments. Thus, we see that the item whose presence is being functionally explained can only consist in a homeostatic reaction, i.e., a *reactive* event (the shivering above). But when the item-to-be-explained is an ongoing social *process*, there is no way in which the item's presence could be brought about and maintained over time as a reactive event which comes to exist only at moments of attempted adjustment to regain the system's equilibrium state (van Parijs 1981:38).

Additionally, functionalist explanations concerning subjects such as group size, birth spacing, food sharing, and hunting strategies of small-scale societies do not employ a homeostatic model but instead employ some sort of *selectionist* account (Kincaid 1996:108). These and most other social examples are not reactive processes that exist for the sole purpose of returning a social system to equilibrium, so the homeostatic model seems completely inappropriate.

4. **The inability to handle change over time problem.** The functionalist model of society, both for primitive and modern societies, has been repeatedly criticized for exaggerating the consensus present within those societies, the actual stability of those societies, and the integration of those societies. The theoretical consequence of the over-estimation of the extent of these factors has been the virtual disregard of the concepts of conflict, change and disorder

(Abrahamson 1978:41). Abrahamson suggests two reasons for this: (1) early theorists were concerned with answering Hobbes' question, "How is social *order* possible?" and (2) the fact that many of the initial anthropological studies were evaluations of more stable primitive societies. The failure to clearly discriminate between the causal origins of societal order and that of societal conflict was a common criticism leveled by many students of the early theorists such as Leech, Evans-Prichard, Firth and Turner.

In Robert Nisbet's analysis of structural-functionalism, Victor Turner has found much support for the view that functionalist theory seemed unable to handle the problem of change over time. Nisbet argued that "functionalism . . . from Durkheim through Radcliffe-Brown to Talcott Parsons, tried to present a unified theory of order *and* change based on a biological metaphor—it tries to draw the *motivational* mechanisms of change from the same conditions from which are drawn the concepts of *social order*. In other words, we have here the biological notion of immanent causation, an inner growth principle, as well as a homeostatic control mechanism. The simple, like the grain of mustard seed, *grows* into the complex, through various pre-ordained stages" (Nisbet 1969; as cited by Turner 1974:311).

Clearly one of the most conspicuous violators of exaggerating the extent of the existence of social order, which in turn produces elements such as consensus, stability and integration, was Talcot Parsons. He commonly stressed the simple or "manifest" roles and identities articulated by societal members while virtually ignoring the manner in which deeper or "latent" roles can provide a basis for the emergence of conflict such as that demonstrated in American society by both the feminist and black movements (Abrahamson 1978:43). Both of those ongoing social conflicts are difficult to explain as byproducts of a societal system

attempting to return to a pre-established equilibrium. Yet, such is the problem faced by Parsonian functionalist theory which greatly over-estimates societal order, stability and integration.

It is not that Parsonian theory does not seek causal explanations. It is, however, that it and similar theories err in their preoccupation with searching for this causality almost exclusively in conditions of regularity. Having assumed the importance of conditions that create social order, they look for the causal mechanisms of change in all the wrong places. Such philosophies are invalidated by their inability to recognize that causation is revealed in disequilibrium as well as in equilibrium, in change as well as in persistence, and in the breakdown of regularity as well as in regularity of sequence (MacIver 1965:30).

The above is easily illustrated by the attraction the authors of these theories seem to have toward hypothesizing the operation of social systems in a manner similar to a homeostatic control mechanism. The theoretical impact of Nisbet's point in contrasting the "biological notion of immanent causation" with a "homeostatic control mechanism" is easily demonstrated by contrasting the work performed by natural selection in biology with the discussion in the immediately preceding subsection on homeostatic processes.

Some of the overall confusion surrounding functionalist theories is a result of the multiple uses given some of the same terms within different theories. For instance, I earlier referred to the claim of functionalism that most institutions exist in order to maintain social equilibrium or societal *survival*. However, the term 'survival' can have very different meanings in different models of functionalism. In the homeostatic model just discussed, the term is used in an all or nothing concept of maintaining the equilibrium of the system. If the cybernetic

mechanism functions correctly, the internal system is protected or maintained from the external environment which threatened to causally intrude upon it and change its state.

The internal environment either is maintained at its equilibrium and "survives" or it does not and the internal environment is brought into equilibrium with the external environment. In the later case, the system loses its separate identity and "dies." In our example, if the reactive event of shivering fails to bring the internal environment back to 37 degrees C, then the body will eventually succumb to the external environment and cease to be a distinct and separate system.

Note that the two options available are to maintain status quo without change (survive) or die. Lost is any concept of evolution. Lost is any concept of incorporation of the two environments into a new entity. Lost is any concept of *change*. Again, we are talking about a cybernetic mechanism. It either works, or it does not.

Contrast this use of the term "survival" to its use within Darwin's theory of natural selection. There, "survival" is the very handmaiden of change. Biological survival is promoted by a large amount of blind, random inherited genetic variation. Survival among the fittest of a variety means reproduction of that species and its continuance into the future. But the initial field of candidates for survival were created by a random recombination of genes through processes such as sexual reproduction and mutation. So when Kincaid talks about social theories concerning group size, birth spacing, food sharing, and hunting strategies of small-scale societies not employing a homeostatic metaphor but instead employing some sort of selectionist account, one can appreciate the immense difference in these two uses of the term "survival." In both cases almost identical language is used, something to the effect that "it is

the function of the (attribute) to (perform a certain role whose performance results in survival)." However, almost opposite meanings are conveyed by the two contexts.

On the homeostatic model, growth is impossible, the system cannot change over time, while on this selectionist model, change is what provides survival. Looked at from the point of view of change over time, we can see yet another reason why a homeostatic model is inappropriate for accounts of social functions and why *order* and *change* need not be products of the same mechanism.

5. **The unnecessary postulates of functional unity of society, universal functionalism and indispensability.** Sociologist Robert K. Merton, in his classic work on functional analysis, critically reviewed three interconnected postulates adopted by anthropological functional analysts which he suggested are unnecessary. In summary these postulates hold that: (1) standardized social activities or cultural items are functional for the *entire* social or cultural system; (2) that *all* such social and cultural items fulfill sociological functions; and, (3) that these items are consequently *indispensable* (p. 25). We might initially note the breadth of these claims. As Merton's italics suggest, all three are universal claims. As such, a single counterexample to each is sufficient to refute each claim. Merton more than meets this requirement; he provides numerous counterexamples to each claim.

a. *Postulate of the Functional Unity of Society.* As defined by Radcliffe-Brown, this postulate claims the existence of "a condition in which *all* parts of the social system work together with a sufficient degree of harmony or internal consistency, *i.e., without* producing persistent *conflicts* which can neither be resolved nor regulated (Merton 1957:26, citing Radcliffe-Brown) (emphasis added). Note how *inconsistent* this assumption is with the social

reality demonstrated in America by the feminist and black movements as discussed in the prior criticism of Parsonian functionalist theory. Clearly, both of these movements have consisted of *excluded* parts of the system which saw conflict as a necessary pathway to traverse in order to accomplish goals they perceived as incapable of being resolved or regulated within the then existing social equilibrium.

Additionally, Merton argued that the assumption of functional unity as used within anthropology was disanalogous to integrated organisms in biology. His basis for this argument was the fact that for lower organisms the loss of some major body part may cause only temporary inconvenience, and only for the period of time it takes for regeneration of replacement tissues to occur. Another counterexample provided by Merton is the fact that social sentiments such as "increased solidarity of the community" and "increased family pride" are often cited by anthropologists as instances of functional adaptive sentiments, but can also be dysfunctional for other groups. An increase of individual family pride, while functional for family unity, may often disrupt the solidarity of a small community.

b. *Postulate of Universal Functionalism.* The assertion of this postulate is that *all* standardized social or cultural forms have positive functions. To Malinowski's claim that "in every type of civilization, every custom, material object, idea and belief fulfills some vital function", Merton responded that this was an example of the "splendid exaggerations" of anthropologists (p. 30, 32). This is simply too broad a claim to ever be confirmed. Nothing short of investigating "every custom" in "every civilization" (past and present) could produce sufficient evidence to confirm such a claim.

It is doubtful that such a postulate can perform valuable work for a theory of social

functions. Besides being impossible to confirm, it seems incorrect. All kinds of social customs seem less than "vital". What, for instance, are the *vital* functions of the standardized social form of particular institutions within the United States such as the political lobbying organizations? Political institutions seem ripe with customs, ideas and beliefs that do not perform "vital" functions for society as a whole.

c. *Postulate of Indispensability.* This assumes that (1) there are certain indispensable functions, without which society will not persist, and (2) certain cultural or social forms are indispensable for fulfilling each of these functions. As to the combined effect of these two claims, one need but recall the prior discussion concerning "functional equivalents" to realize how mistaken this type of functional claim is. Wesley Salmon succinctly illustrates the concept of *functional equivalents* with the example of the long ears of the jackrabbit. The ears provide an effective mechanism for cooling the animal but there exists other structural forms that can perform equivalent functions. Humans perspire and dogs pant (Salmon 1989:30). If different but equivalent forms can perform functions, it is hard to seriously argue that such forms are indispensable for fulfilling these functions. Additionally, if these assumed indispensable functions could be performed by a variety of structural forms, what is the logic in asserting that the functions themselves are not subject to similar variety?

One is left with the inquiry as to the basis for the enormous assumption that society will not persist without these functions. Such a claim should be the result of decades of research, not an initial presumption that precedes research. With this postulate in mind, it is easy to understand the much-repeated criticism of functionalist theory that it tends to reify the existing status quo. Cannot the persistence of societies also occur through the operation of functional

equivalents, or alternatively, persist in a different manner through the operation of entirely different functions? And, if the answer suggested is that they cannot, what is the empirical basis for that conclusion? Again, one doubts the existence of such empirical evidence.

An illustration of how functionalist theory can unintentionally reify status quo is provided by one of the classic criticisms of the conservative impact of functionalist theory. In commenting on Malinowski's view that native populations should be protected from outside influences in order to preserve their indispensable structural form, Alvin Gouldner noted that such a view dovetailed nicely with the policies of British colonialism. Gouldner adds his oft quoted observation that while some functionalists "conceived it as their societal task to educate colonial administrators, none thought it their duty to tutor native revolutionaries" (1970:132). In retrospect, it seems obvious that not all functions of British colonial rule were indispensable, particularly in the view of the native subjects who were the recipients of those functions.

Section Four

Summarizing the lessons

If functional explanations are to become part of legitimate and causal explanations within the social sciences, then the substantial and significant criticisms of the social doctrine of functionalism need to be addressed. What follows is a summary of some of the lessons to be learned from those well-documented theoretical deficiencies.

The classification problem. Functional explanation need not and should not rest upon a biological analogy. Other more appropriate ways to classify accumulated data need to be employed which make classified data available to address significant questions such as *what*

causal role a component plays within an existing system, *why* these functions came to be, and *why* they persisted over time. Functional categories can provide this classification work for social explanation, but they must be causal categories concerned with what is the typical similarities shared over time by members of the category. While biological analogy is inappropriate, there are substantial lessons to be learned about the methodology employed by biologists in creating such real and causal, as opposed to analogical, functional categories.

The metaphor problem. The social sciences need to cease thinking of social systems in terms of an analogy to the human body. This core metaphor of the social sciences has long outlived its usefulness. It contributes more ambiguity than clarity and the growing disanalogy between the principal and subsidiary subjects of the analogy guarantees its inability to serve as a successful pedagogical tool in the future.

The system boundary problem. While the accuracy of system boundary identification is always a descriptive problem, it is an empirical one subject to confirmation or correction. What exacerbates the problem for the social doctrine of functionalism is its comparative use *between* systems, especially in the case of an organic analogy. An awareness of the problems created by this prior analogical usage should at least be instructive in highlighting the impracticality of *assuming* similarity between systems as a point of beginning for research. If the existence of comparatively similar social systems is claimed, that should be a conclusion of empirical study, not an underlying assumption.

The cybernetic mechanism problem. As noted earlier, the projection of a cybernetic mechanism into a theory of social functions is extremely problematic. The operation of such a mechanism is inconsistent with the operation of social process and its use in social theory

seems completely inappropriate.

The inability to handle change over time problem. This well documented criticism again demonstrates the problems inherent in the nature of the initial assumptions that precede analysis. If one initially exaggerates the extent of the existence of social order, that existence will explain such social elements as consensus, stability and integration, which if present to the extent suggested will in turn support the continuation of social order over time. While such a theory attempts to be causal, its circularity only returns it to its assumed point of origin. A related and interwoven problem is the assumption that the causal origins of social order are the same as the causal origins of social conflict. Again, this problem has been worsened by the use of an organic analogy that has impaired the search for causality.

The unnecessary postulates of the functional unity of society, universal functionalism and indispensability. These postulates have only added to the resultant inadequacy of social research that has been compelled to be organized in a manner consistent with pre-existing and erroneous assumptions. That these assumptions have quite often been subjective extensions of Eurocentric thinking has been well document by post-Modernist criticism. Too often, by assuming the necessity of existing functions, functionalist theory has reified status quo and been unable to explain social change such as that illustrated in America by the feminist and black movements, or elsewhere by the numerous examples of global social unrest and upheaval.

It will be incumbent upon the view of functional explanation presented in chapter four to take advantage of these insights. I will there argue that it is possible to implement these lessons when social explanation is focused upon causation, not mere form or structure, and

when social explanations are seen as partial, cumulative portions of a larger explanatory text.

Chapter Three

BIOLOGICAL FUNCTIONS

My main thesis throughout this book is that functional explanations are central to the whole enterprise of the social sciences, and particularly to social anthropology, where the primary task is to explain cultural phenomena. Central to that task is explaining the causal history of the variety of cultural and institutional forms that have existed in the past, and may continue to the present. This includes explaining the development, operation, and roles of social institutions and of ritualized social activities. Such explanations will be functional through and through. But the real nature of functional explanation, and the issue of precisely how it properly applies to social phenomena are vexing philosophical issues. It is my purpose herein to attempt to resolve some of the central issues, or at least to make some progress towards such resolutions.

The past two decades of theoretical discussion of functional explanation in biology, where functional explanation has historically played a significant role, will I think shed substantial light on the nature of the kind of functional explanation required by the social sciences for its explanatory account of social functions. For, many of the same issues arise in both disciplines, but functional explanation in biology has been the subject of more significant analysis. So in this chapter I will explore in some depth the issues that arise in the debate regarding functional explanation in biology.

The theoretical exchange on explanation in biology contrasts two types of accounts of biological functions, etiological and dispositional. The development of the debate concerning these accounts provides an opportunity to analyze in-depth the nature of etiological and

dispositional accounts. Are these two types of accounts so very different? Or, are the explanations they produce different, yet not inconsistent portions of a more complete explanation of the function of a particular item? Addressing these questions provides the opportunity for a discussion of the primary features of the view of explanation I will advance.

In this chapter I will outline the explanatory goals of both the *etiological* and the *dispositional* accounts of biological functions, evaluate the strengths and weaknesses of the kind of work each account performs in different explanatory areas, and thereafter, address the question of whether or not the explanations produced by these accounts appear to be complementary parts of a larger explanatory text. The discussion of these issues is organized in Section Two under subtopics designed to highlight the differences between these accounts: the "exists/persists" ambiguity, the concept of "proper functions", the related topic of functional categories, the concept of malfunctions, and lastly, the notion of biological normality.

In Section Three I move to a discussion of the similarities in the accounts. I argue that, contrary to the received view, that both are "forward looking" as well as "backward looking" and that much of each account is presupposed as a basis for the other. I conclude this section with an argument that dispositional and etiological accounts are in fact not inconsistent, but rather complementary. I argue that the explanations produced by these accounts, while different, are often consistent portions of an idealized explanatory text and that each captures some information missed by the other (although there is much overlap between them). I discuss how the widely accepted distinctions known by biologists as "Tinbergens Four Questions" makes the case for such a view. I illustrate this point further by expanding some of the illuminating work of Sandra Mitchell and Peter Godfrey-Smith.

Section Four supplements these arguments with some analytical exercises. I provide a modification of Cummins' account of functional explanation which I suggest is helpful in illuminating the interwoven nature of dispositional and etiological accounts. Thereafter, I apply Cummins' explanatory schema to an extended example offered by Mitchell in a recent article. I suggest that such an exercise allows one to cash out the causal claims contained in the explanation produced by each account, thereby disclosing the fundamental similarity of the causal claims which underlie these accounts.

In Section Five I address the questions concerning the relationship between the history of a trait of a component in a system and its *function* in the system. To be performing a functional role, must it still perform the same work that historically caused it to be selected? Must a functional explanation of a trait depend on the nature of its origin? I also address the related issue of whether the traits that arise from natural selection are the only ones that may count as having a functional role, or whether other processes may give rise to traits with functional roles. I argue that while natural selection is the most widely recognized mechanism that gives rise to proper functions in biology, it is not the only mechanism that does so. In so doing, I argue that the preeminence of natural selection and adaptionism should not limit the use of functional explanations.

The theoretical exchanges within biology provide a splendid example for the social sciences to consider as they come to terms with their own applications of functional explanations in their own domains.

Section One:

The explanatory debate pertaining to biology's functional accounts

There has been a profusion of articles written mostly during the last two decades within the fields of biology and philosophy of biology. Those articles elucidate two different views of biological functions. The first of these views is an *etiological account* of function based largely upon Larry Wright's insightful work (1973, 1976), an account where the biological function of a trait is identified by *looking backward to its historical role* in promoting the survival and reproductive success of organisms that possess it. This account has continued to develop, thanks mainly to the writings of Ruth Millikan (1984, 1989), Karen Neander (1983, 1991 a,b), and Sandra Mitchell (1989, 1993, 1995). This etiological account as developed within biology has become "the standard line" in biology (Allen and Bekoff 1995).

An alternative, *ahistorical account* also survives based upon so-called Cummins-functions, first defined by Robert Cummins (1975). This and other accounts of this nature are often referred to as *dispositional accounts*. These accounts tend to identify the function of a trait by *looking forward via the propensities* or causal roles of those traits to enhance their own survival into the future. Thus, these two different sounding concepts are most often contrasted by the use of terms such as "backward looking" or "forward looking", historical or ahistorical, etiological or dispositional. However, dispositional accounts are most often presented as a component of an existing *pluralistic* explanatory practice within biology that also recognizes the *standard* etiological line (Griffiths 1993, although this should be read in conjunction with his 1994 which is an argument for a cladistic concept of biological traits; Godfrey-Smith 1994; Walsh 1996; and, Millikan).

I want to suggest at this point, contrary to many of the expressed views of the proponents of both accounts, that much of the difference between these accounts is a matter of focus, not so much a matter of substance. I shall argue for this position later in detail. However, I take this opportunity to plant the seed of that future argument, to forewarn the reader not only to note the distinction between these accounts, but also to anticipate a synthesis which exploits the similarity of these views.

1. **The Standard Line: Etiological Accounts.** Can the presence and persistence of properties of biological populations be explained by an appeal to biological functions? An etiological account of biological functions claims an affirmative answer to this question (Mitchell 1995:41). It holds that to explain why a biological trait occurs is to describe the causal history which led to the presence of that trait. A function is thus associated with a consequence in the past which is responsible for the attribute's presence today. Larry Wright suggested that when we ask questions such as 'Why do porcupines have a coat of quills?,' or, 'What are the quills there for?,' or, 'What is the function of the quills?,' that all of these questions are answered by 'To protect the wretched little beast from hungry predators.' The function is what it's there for. The quills may be good for sleeping on, avoiding certain irritants, or even tooth-picking. But the quills do these later things by accident if they do them at all. The function of the coat is that particular thing it's good for which explains why it's there. Protection is a consequence of the quills being there. That is why things with quills and consequently, the quills themselves, have survived (Wright 1976:91-92).

This illustrates Wright's formulation for functional ascriptions. According to Wright, the function of X is Z iff: (i) Z is a consequence (result) of X's being there, and (ii) X is there

because it does (results in) Z. In this example, the function of the quills of a porcupine is protection from predators iff: (i) protection from predators is a result of the quills being there and the quills are there because they provide protection from predators (and thus allow porcupines with quills to survive and reproduce other porcupines with quills).

Thus, in a continuation of the claims of Wright's original work in this area, the ascription of a function is, itself, considered explanatory. On this view an "ascription" of a function to a trait is really a sort of shorthand for a lengthier explanatory story that the scientist can fill in. It is this underlying story that produces the explanatory import and it is that underlying story that will be the subject of these discussions.

Allen and Bekoff (1995:612) describe this standard functionalist explanatory line in biology as having three components: (1) functional claims in biology are intended to explain the existence or maintenance of a trait in a given population; (2) biological functions are causally relevant to the existence or maintenance of traits via the mechanism of natural selection; and, (3) functional claims in biology are fully grounded in natural selection and are not derivative of irreducible teleological notions such as design, intention, and purpose.

2. The Alternative View: Dispositional Accounts. Within biology, the two most often discussed dispositional accounts are the account of Robert Cummins (1975) and the account of Bigelow and Pargetter (1987). These two accounts together with the etiological accounts represent a fairly complete picture of the range of functional theory within biology (contra, see Griffiths 1994).

a. Cummins' account. For Cummins, "to ascribe a function to something is to ascribe a capacity to it which is singled out by its role in an analysis of some capacity in a

containing system" (p.765). In other words, a function explains the propensity of a component in a larger system to contribute to the more complex system; its function is the component's contribution to the overall capacity of the system. This capacity of a component within a complex system has come to be referred to as a "Cummins-function."

Cummins called this type of explanatory practice "functional analysis." He saw this as a different explanatory project than that engaged in by other functional accounts. Functional analysis was appropriate, in Cummins's view, whenever the objective sought was an explanation of the overall capacities of a complex system. Paul Griffiths nicely illustrates Cummins' methodology:

One complex capacity which might be explained by functional analysis is the ability of an animal to survive and reproduce. This can be analyzed into a set of simpler capacities, such as the capacity to move about, to feed, to escape predation, to mate, and so on. Each of these can in turn be analyzed into even simpler capacities. In the case of feeding, the ability to ingest food, masticate it, break it down into simple nutrients, to absorb these, and so forth. These capacities in turn can be analyzed into still simpler capacities, arriving eventually at such simple capacities as that of a membrane to permit diffusion of some substance. These base level capacities are directly explicable by physical laws. Each capacity at each level can be attached to some sub-system of the organism. The function of this sub-system is the capacity which it realizes, and which contributes to the overall capacity of the organism (Griffiths 1993:410-411).

Thus, depending on our interests, we could focus in on any portion of the system and analyze the functions of its components. Further, we could focus in on a particular activity conducted by the component to the exclusion of other activities performed by the component. The result of this focusing is that our own particular interests define the boundary of the containing system, and select the field of activity within which the performance of a causal role defines a function. For these reasons, a Cummins function has been referred to as a "causal role function" (Neander 1991a:181).

b. Bigelow and Pargetter's account. The account of Bigelow and Pargetter takes

the role of functional ascription as based upon what an item has a tendency or disposition to do in the *future*, as opposed to what it has done in the *past*. They suggest that when we describe the function of some biological character, we do so by reference to some future event or effect which may never actually occur, e.g., while the function of the bee's sting seems obvious, the fact is that most bees never use their stings. This suggests to Bigelow and Pargetter that the character involved should be viewed in terms of the propensities it confers, not upon its prior causal history. Thus, Bigelow and Pargetter make the claim that etiological accounts are too backward-directed and that dispositional or propensity accounts are more forward-directed. They argue that their account accomplishes this forward-directedness because it is (1) ahistorical, and (2) relies on the tendency of an item to perform in the future that which is its function. "Functions should be forward-looking . . . something has a function just when it confers a survival-enhancing propensity on a creature that possesses it" (Bigelow and Pargetter 1987:191). It is this forward-looking disposition or propensity (or capacity in Cummins's language) from which these types of accounts derive their name.

Section Two:

Differences between the accounts

It will be easiest to analyze the strengths and weaknesses of these differing accounts of biological function by considering their differences first, saving for later a discussion of their similarities.

1. The "exists/persists" ambiguity. Since there are pluralistic accounts within biology, it is worth noting that there is a potential ambiguity regarding the subject matter to be

explained within these differing accounts. This difference can be illustrated by referring back to Allen and Bekoff's description of the first component of "the standard line" as intending to explain *the existence or maintenance of a trait*. While the concept of *maintenance of a trait* is customary only in biological functional explanation, the functional explanation of the *existence* of traits or attributes is common within functional explanations as more widely employed. It is the later terminology that is subject to ambiguity, both within biology and without.

Allen and Bekoff may be understood to either be offering an account that explains the *existence* of a trait *or* an account that explains the *persistence* of a trait (maintenance within a given population). These can be very different. What counts as an explanation of the *existence* of a trait *at a moment in time* (especially the present) may be quite different from what will count as an explanation of the historical *persistence over time* of a trait.

Obviously, the ambiguity between the nature of these two kinds of functional explanation turns on the interpretation of the term *existence*. We can clarify this ambiguity by either: (1) equating an account of the *existence* of a trait with an account of its origin and maintenance to the present (the history of its *persistence over time*), or, (2) equating an account of the *existence* of a trait with its present properties or disposition. Unfortunately, this *exists/persists* distinction is not always made clear in the various commentaries, and it is worth clarifying in any account that relies on the term *exists*.

In an etiological account, the term is almost always intended to refer to *persistence over some significant period of time*. Millikan, for instance, uses the term *exists* in her account, but she is quick to make clear that her definition "looks to history rather than merely to present properties or dispositions" (1989:289). Likewise, Godfrey-Smith's *modern history*

view references the "recent maintenance of a trait in a selective context" (1994:356).

Alternatively, the very nature of dispositional accounts focuses on the present causal role of the attribute and its future capacities. As a result, these accounts typically do not refer to a history of the causal role of the attribute, but tend to employ the term *exists* to refer only to a given moment. This is a critical difference between the accounts, and one that should not be glossed over by conflating different uses of the term. For example, Harold Kincaid points out that it is common to attribute to the large beaks of finches the function of eating large seeds. He suggests that this functional claim explains why the large beaks have persisted over time by giving a causal history of the role they played, while also supplying information as to why the size would have provided a survival advantage in the past. He contrasts the nature of this type of historical explanation with an ahistorical one. If large finch beaks (as a type) had just come into existence five minutes ago, they would not *exist in order to* eat large seeds, although the large beaks might take on that causal role, provide that ability, for present day finches (1996:114).

One simple way to avoid confusion with these very different types of explanation is to substitute *persists* for *exists* in etiological accounts. As Kincaid argues, *persists* is generally more perspicuous than *exists*. In this regard, the *etiological* claim that "biological function A *exists* in order to B" could be equated to "A is a type which causes B, and A persists (was selected and maintained) because it causes B" (p. 113). Such an interpretation tightens up the *exists/persists* ambiguity.

2. The concept of "proper functions". An etiological account of biological functions asserts that an explanation of why a biological trait occurs is provided by a

description of the causal history which led to the presence of that feature. As noted earlier, a function is thus associated with a consequence in the past which is responsible for the attribute's presence today. But there remains the problem that not all causal sequences create functionally important traits. Innumerable causal sequences lead only to idiosyncratic traits, which have no real functional role. In order for the existence of a trait to have explanatory power with regard to a functional role, there needs to be some *connection* between the consequences of the trait and the current presence of the trait. This occurs when some consequence of the trait gives rise to the selection of it over alternate traits.

An explanation that meets this explanatory burden *explains why* the attribute persisted over time; it did so because its consequences aided or enhanced its survival. Therefore, causal sequences which show how the trait was selected for its function, and how its function promoted the survival of the system to which it contributes, solve the problem of identifying which causal sequences create and are associated with *functions*.

It is worth noting that the problem of determining which causal sequences create functions is solved by discerning the underlying causal mechanism at work and thereafter incorporating a recital of its operation in the particular case being explained. Functions that satisfy this type of explanatory accountability are called "proper functions" by etiological theorists such as Mitchell, Neander and Millikan. Presumably the fact that the underlying mechanisms in functional explanations are often taken to be selection mechanisms motivated Larry Wright's suggestion that functions are partly determined by selection history. Sandra Mitchell and Karen Neander have expanded Wright's suggestion to the much stronger claim that functions are wholly determined by selection history.

Mitchell asserts two necessary conditions for the causal sequence to render a trait or behavior with a significant functional role: (1) that the trait or behavior has been selected over alternatives on the basis of its consequence, and (2) that it is produced or reproduced as a direct result of that selection process (1995:42). Or, stated a little differently, she says that the functional ascription requires "both selection *for* the functional consequence over alternatives and replication *of* the structure with that consequence as a direct result of selection" (p. 50).

Neander simply defines the *proper function* of a trait to be whatever it was selected for. As she is primarily concerned with biological traits, she claims that the biological proper function of such an item can only be to do whatever items of that type did that caused them to be favored by natural selection (1991a:173-174; 1991b:455). Thus, on her view, an etiological account of *biological* functions looks solely to a trait's selection history to determine its function. According to such an etiological theory, traits with functions are necessarily *adaptations* since the trait became fixed in the population due to past contributions to fitness. However, the trait may or may not be *adaptive* in that it may or may not contribute to the fitness of the organism in its current environment (Neander 1991b:458, fn. 6).

Thus, "proper functions" are seen to be the product of an underlying mechanism, a selection mechanism, and, according to the standard line in biology, they are the specific product of the mechanism of natural selection.¹ Conceptually, they are perceived to operate in such a manner as to form the basis for the creation of functional categories.

3. Functional categories. According to Karen Neander, the simple idea that "the proper biological function of a trait is to do whatever it was selected for" has permitted the teleological notion of function to remain the 'conceptual glue' of biology. The basis for her

claim is the fact that a great proportion of biological categories are functionally defined (1991b:467). Functional categories are generated by the notion of what like components of a system are *supposed* to do. As such, they are tied to the concept of biological norms. It doesn't matter whether a particular heart *can* pump blood, what matters is what the heart is *supposed* to do in a *properly functioning* organism. Thus, biological norms and the concept of proper functions are determined by the history of traits.

Biological categories do not have necessary and sufficient properties which define them. This is because in biology traits and the groups of organisms that possess them are both variable and evolving. Because of the variability or evolution of the members, or of the trait possessed by the members, what might once have been a necessary or sufficient condition for group membership may no longer be so.

Because of this, something besides necessary and sufficient properties is needed to establish membership in these functional categories. Instead, group membership is determined by possession of whatever historically did provide normal members with a survival advantage in a given environment. As Ruth Millikan noted, functional categories are *essentially* categories of things that *need not* successfully fulfill their functions in order to have those functions (1989:296). A malfunctioning heart does not fulfill its function but still meets the historical identity conditions for type-classification.

It is understandable how the functional categories created by etiological accounts came to be seen as the 'conceptual glue' of biology. Their link with biological normality and with biological types allows the identification of these stable categories. What is normal within the type is determined by natural selection and this history of selection reflects the appropriate

historical identity conditions. Thus, functional categories are identified in terms of traits in the members of groups that meet historical identity conditions for type-classification (Mitchell 1995:45).

4. Malfunctions. The ability of a theory of biological functions to make sense of the notion of *malfunctioning items* is critical. For, the biologist not only needs to explain the benefits conferred by correctly functioning systems, she must also be able to explain detrimental effects due to malfunctioning systems. Millikan, Neander and Mitchell extol the virtue of etiological accounts within biology in handling the issue of malfunctions, while at the same time criticizing the inability of dispositional or propensity theories to appropriately deal with this problem.

An obvious counter-example to Bigelow and Pargetter's claim that functions are survival-enhancing propensities is the almost entire class of traits categorized as dysfunctional. These traits do not have a disposition to perform their proper function – an impaired kidney or heart does not have a disposition that is apt for selection, nor does it possess a survival-enhancing propensity. Therefore, if a malfunctioning kidney or heart fails to be apt for selection or to have a survival-enhancing propensity, then that kidney or heart *does not have a proper function* at all under Bigelow and Pargetter's account (Neander 1991a:183). But, since *malfunction* makes sense relative to a (proper) function, (i.e., having a proper function, but failing to perform it), items with *no function* cannot be said to malfunction too. Cummins' account fares no better. For Cummins, an item must function as something or other, or have the capacity to do so, in order to have a function (Millikan 1989:294). But the diseased heart that is not capable of pumping blood surely has a function to do so despite Cummins' claim that

in order to have the function of pumping, a pump "must be capable of pumping" (Cummins 1980:185).

Etiological accounts avoid the problem of malfunctioning items in that they rely on natural selection to identify "proper functions." Natural selection operates at the level of types, not tokens. Thus, the functional categories created in biology are categories of types. A particular token does not reside within such a category on the basis of its present capacity to perform a function, nor on the basis of possessing a disposition that is survival -enhancing or apt for selection. What places a particular token within a functional category is the "normal" or "proper" function of items of that type in a historically continuous process. "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" (Mitchell 1995:45). In the diseased heart example, the heart is of a type that has the historical function of pumping blood. The diseased heart simply fails to be able to perform that function, thus earning it the label of being malfunctional.

5. **Biological normality.** An understanding of what is the "normal" function of items of a particular type allows etiological accounts to comfortably handle the problem of malfunctions. Given the importance of the work done by the concept of biological normality, it is worth underscoring the etiological nature of this concept. "Normal", in the sense used here, is descriptive of that *conduct* by which functional types are identified. And, those functional types are ultimately picked out by their connection with natural selection. Biological normality is determined by a history of selection, which thereby explains the stability of biological categories relative to demographic, environmental, or pathological variables (Neander

1991a:183).

It is worth noting that the biological concept of normality is not the statistical concept of normality concerned with averages, means, medians, or modes; nor is it the evaluative concept of normality concerned with conventional, cultural or ethical norms. Additionally, it has been argued that the biological concept of normality is distinctive because nothing like normality in its biological sense can be found in the physical sciences, even as a derived concept (Wachbroit 1994:579).

It seems, and I will later argue, that the uniqueness of the biological concept of normality is due to the fact that it is grounded in a well understood mechanism, natural selection. The types of traits selected reflect what is normal for the type regardless of whether or not some of the members of the categories are defective and therefore unable to perform the function (with the historical role) by which they are categorized. Normality is not a statistical average, median or mode of the functioning and non-functioning tokens which belong to the functional category. It is instead a description of what functioning tokens within the category do (or did historically) to promote survival and reproductive success of organisms within a species (i.e., to promote fitness). Likewise, biological normality is not established by convention, cultural preference, or ethical norm. It is instead grounded by nature through selection over alternatives on the basis of the consequences of a trait reproduced as a direct result of the selection process (Mitchell 1994:42).

Section Three:

Similarities between the accounts

1. The "forward looking/backward looking" nature of functional accounts.

Contrary to the claims of Bigelow and Pargetter that their account is 'forward looking' while etiological accounts are too 'backward looking', Wesley Salmon suggests that their account "is not very different from Wright's". Salmon asserts that Bigelow and Pargetter seem to conflate teleology and function. He argues that they seem not to notice that Wright's account handles teleology and functions separately, yet acknowledges their close connection. He further suggests that Wright's analysis of teleological behavior "is as much a propensity theory, and is just as forward-looking as is the Bigelow-Pargetter theory of functions" (Salmon 1989:113).

Consistent with Salmon's view is Karen Neander's observation about the nature of etiological accounts: "We do not understand teleological explanations correctly, as a species of ordinary causal explanation, unless we understand that they are not only explicitly 'forward-looking' but also implicitly 'backward-looking', and it is only in virtue of this implicit looking back to prior causes that teleological explanations are explanatory" (1991b:463). This seems exactly right, but in need of explication.

2. The assumptions behind the accounts. It seems to me that each of the two types of accounts of functions, forward-looking and backward-looking, make certain assumptions that are central to the operation of the other. Although this relationship between the two kinds of accounts has gone largely unrecognized, Bigelow and Pargetter's account implicitly relies on presuppositions regarding the conditions that prevailed within the historical past, while etiological accounts implicitly rely on a presupposition concerning the fitness of a functional

trait which is historically related to its propensity to cause the survival of the organisms that carry it.

a. Past conditions. Peter Godfrey-Smith (1994) performs an impeccable dissection of the assumptions underlying the survival propensity account of Bigelow and Pargetter. He points out that while Bigelow and Pargetter's account seeks to explain survival in terms of a creature's propensity to survive, it faces the problem of contextualizing this propensity for survival by defining the future environment into which the propensity is projected. Bigelow and Pargetter concede this problem but suggest that a survival-enhancing propensity should be "relativized" to the creature's "natural habitat" (1987:192). But, what does that really mean? While we may know what the natural habitat was in the historical past, how can we know what that term means in the ahistorical present or in the future? Are present conditions "natural", or might they be momentary deviations from an otherwise continuous historical pattern? Or, what are the conditions which constitute a "natural habitat" in the future? That such conditions have meaningful consequences seems obvious. Dinosaurs' size provided strong survival-enhancing propensities for their future until their natural habitat changed radically, so as to favor species weighing less than fifty pounds.

These types of questions begin to challenge the basis of Bigelow and Pargetter's claim that their account is ahistorical. Indeed, as Godfrey-Smith points out, it appears that "natural habitat" is understood historically by Bigelow and Pargetter. In order to give meaning to such a term, their account seems to draw on the historical facts it sought to avoid (Godfrey-Smith 1994:352). For instance, when a survival enhancing propensity is projected into some future environment, the question arises as to what competitors the propensity will tend to oust from

the population by the more survival-enhancing trait. The propensities for traits to be selected are relative to an environment, and they must be selected over some range of alternatives (Godfrey-Smith, *ibid*). But, while we understand what those alternatives were in the past, how can we know what random genetically mutated alternatives may appear as competitors to the relevant trait in the future? Is the *function* of the present trait given by its propensity to promote survival in some (presently indeterministic) future environment? As Godfrey-Smith so clearly argues, if the explanandum is how things are *now*, nothing present, or future can be the explanans — only the past will do (p. 355).

At the end of Godfrey-Smith's inquiry into questions such as these, one is left with the observation that Bigelow and Pargetter's account is actually not ahistorical but, instead, makes unstated historical assumptions. Thereafter, these historical assumptions are projected, as propensities or dispositions, into the future. This suggests that etiological and propensity accounts are much more closely connected, and more compatible than is often intimated.

b. "Fitness" as a propensity. Sandra Mitchell, a proponent of the etiological account, points out the extent to which etiological accounts, and Darwinian theory in general, rely on the conceptualization of "fitness" as a propensity. For, as Mitchell argues, to do otherwise would concede "survival of the fittest" to be tautological. "If, 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" (1995:48). But, when "fitness" is seen as a *propensity* to have reproductive success, we can make sense of the idea that twins, for instance, are equally fit but one of them may successfully reproduce while the other might not.

Mitchell thereafter discusses whether it is appropriate to also treat functions as conferring propensities. But let us also inquire into how "fitness" itself may act as a propensity within etiological accounts. Mitchell emphasizes the central role of natural selection in producing traits with "proper functions" within etiological accounts, while conceding the role that the concept of "fitness", *as a propensity*, plays within the historical account of the natural selection and thus the function of the particular trait. Of course etiological accounts do not list this evolutionary history ancestor by ancestor. Even if they could, this would not provide the sought after explanation of reproductive success. This observation suggests that etiological accounts cannot be entirely historical if they are to explain the phenomena they are intended to explain. Rather they must appeal to (usually unrecognized) propensities that existed in the past in order to explain historical survival. If ahistorical accounts must draw on historical context, while historical accounts are partly dispositional, then the explanations produced by these different accounts do not seem to be so dissimilar and unrelated after all. I will try to show just how interrelated they must be.

3. Dispositional and etiological accounts are interconnected. I want to begin this section by defending the claim that dispositional and etiological accounts get at different portions of an *ideal explanatory text* and that the explanations they produce are interconnected. Specifically, I want to argue that the primary difference between a functional explanation in terms of a Cummins-function and an etiological functional explanation are the aspects of the biological phenomena on which they focus.

For Cummins, to provide a causal description of an item is to explain its present capacity to perform a particular causal role in a system. This ascription of a capacity to the

performance of that role is considered in relation to some capacity of the containing system. Thus, the Cummins-function of an item is its capacity to contribute to the overall functioning of a complex system. Note that this description includes:

1. a description of a particular complex system,
2. a description of some particular aspect of that particular system, and
3. a description of the role played by a particular item or trait in terms of its capacity to contribute to that aspect of that system and, thus, to the system as a whole.

If we concede that the particular system, the particular aspect of the system, and the role of the item may all change over a sufficient period of time (e.g., due to evolution), then we must also concede that a Cummins-analysis may provide different functional explanations of a given item for different occasions in time. If significant changes occur within the item, the aspect, or the system, then the explanation produced by a Cummins-analysis will reflect only the function of the item in the system at one particular time in history. We could think of Cummins-functions as arising within time slices relative to the particular capacity of the item to function in relationship to a particular aspect of a particular system *at that particular moment in time*. By necessity, these time slices must satisfy appropriate descriptions of the item, the aspect, and the system *at that particular moment in time*.

This allows us to think of the history of the function of an item as a continuous strand of Cummins-functional accounts which, if detailed enough, would constitute an *ideal explanation* of the function of the item. Thus, we can see that such a sequence of Cummins-accounts must contain much of the same information as would an etiological account of that

function, since both would contain the causal history related to the item, the aspect, and the system.

The nature of the questions we attempt to answer produces different types of explanatory accounts with different emphasis. A Cummins-account describes a capacity of the item to have a certain consequence while an etiological account describes the history of how the function and its capacities contributed to the persistence of the item and the containing system over time. Thus, if dispositional and etiological accounts of the same component of the same system are consistent with a common description of the phenomena, they cannot be inconsistent with each other, but rather should contribute to one another.

4. **Tinbergen's Four Questions.** I have been arguing that the nature of the questions we ask about the function of traits alters the nature of the explanations that are given *without* necessarily making those different explanations inconsistent. This is easily illustrated within biology by the distinction among questions that may be asked about a particular behavior. According to Tinbergen (1963), biologists typically engage in four distinct kinds of inquiries about traits or behaviors. They want to know:

1. the immediate physiological mechanism and the physical stimuli that lead to
the trait or behavior,
2. the current survival value or functions of the trait or behavior,
3. the evolutionary history of the trait or behavior,
4. the developmental process in the life of an individual organism that produces
the trait or behavior.

Thus, depending on which of these questions is being addressed, the relevant explanation will

emphasize a different aspect of the ideal explanatory text regarding the behavior (or trait) at issue. The fact that these different questions call for different kinds of explanations does not in any way suggest that the resultant explanations should be at odds.

Tinbergen's Four Questions have recently been discussed in exchanges between proponents of a *modern history theory* of functions (Godfrey-Smith 1994:351) and a *relational function theory* (Walsh 1996:557). Walsh and Godfrey-Smith disagree about the appropriate theoretical approach to employ when a trait's utility and its selectional history diverge. Walsh argues for a view of functions which is *relative* to a particular selective regime while Godfrey-Smith emphasizes the *recent* history of the utility of the trait. Godfrey-Smith attempts to show how the modern history view is within the intended scope of Tinbergen's second question while Walsh argues that Godfrey-Smith only succeeds by sleight of hand and that etiological accounts do not properly mesh with Tinbergen's distinctions.

Both writers raise interesting points about the ambiguity involved with Tinbergen's second question, concerning the current survival value or function of a behavior or trait. Godfrey-Smith argues that Tinbergen's Questions should be reduced to three questions, that questions two and three should be combined. In effect, that would divide question 2 into two parts, making question 2a a *what* question and question 2b a *why* question about question 2a:

(2a) *What* is the current function of the behavior or trait?, and

(2b) *Why* is the current function of the behavior or trait present?

Under this view, (2a) could be explained by an ahistorical account while (2b) would require an etiological explanation. Walsh argues that this is not always appropriate since it forces utility and selection history to be part of the same question. He argues that no matter how *recent* the

evolutionary past, an evolutionary answer is only appropriate to a question of type three. Walsh argues that questions of type two should only provide an account of the *utility* of a trait, especially in those cases that he sees as ahistorical, cases where no evolutionary history is available to answer inquiries of type three.

It is interesting to note that while both writers argue about the specific breadth of Tinbergen's Four Questions, neither challenge the concept behind Tinbergen's formulation, that each question requires different but not inconsistent answers. Walsh in fact specifically argues that "any complete biological concept of function must incorporate historical and ahistorical function alike" (p. 554). Tinbergen's distinctions provide a working example within biology of Peter Railton's concept of an *ideal explanatory text*. An appropriate answer to any of Tinbergen's questions provides a partial, causal explanation which may be combined with responses to Tinbergen's other questions to form a larger more complete text. Tinbergen's is a view of explanation as partial, causal, and cumulative.

In a recent article, Sandra Mitchell reaches a similar conclusion in comparing the dispositional account of Bigelow and Pargetter with the etiological account of biological functions. In recognizing the plurality of those explanatory projects she concludes that "the dispositional account does not offer a competing analysis to the etiological theory of functional explanation" (1995:51). She cites Nancy Cartwright's observation that "Explanations give answers not only to *why* questions, but also to *what* questions. They say *of* something, what it really is" (1986:203). It is this kind of *what* question that lies at the heart of a Cummins-analysis. Alternatively, etiological accounts ask of something, *why* is it there, *why* has it persisted over time?

I want to come back to Mitchell's discussion, and specifically to Mitchell's illustration of these points. However, I first want to investigate more fully Cummins' account of functions and its bearing on the issues at hand.

Section Four:

Analytical Exercises

1. **Modifying Cummins' account.** If dispositional and etiological accounts are not inconsistent with each other, but rather get at different aspects of the same *ideal explanatory text*, then it may be beneficial to try to capture both conceptions within a common explanatory schema. If this can be successfully done, it should demonstrate their interwovenness. One way to attempt to schematize both dispositional and etiological functional accounts is to begin with an explanatory schema for one and attempt to modify it by adding essential components of the other.

Cummins' account seems the likely candidate as a foundation for such a modification in that his schema is more rigorously formulated and detailed in composition. Additionally, Cummins' schema should be easier to work with because it provides appropriate slots which may be filled in a variety of ways to turn the schema into specific explanations. For each of these slots we may provide something like a set of *filling instructions* suitable to the subject matter. Phillip Kitcher (1981) has suggested an account of explanation in which the explanatory mode of a theory in a scientific domain essentially reduces to a relevant set of explanatory schemata and a set of filling instructions appropriate to the slots in each schema. I will adapt Kitcher's idea to my purposes here.

Kitcher suggests: "A set of *filling instructions* for a schematic sentence is a set of directions for replacing the dummy letters of the schematic sentence, such that, for each dummy letter, there is a direction which tells us how it should be replaced" (Kitcher 1981:516).

Kitcher's conception of explanatory schemata and filling instructions seems ideal for use in tailoring Cummins' explanatory schema to specific types of explanatory tasks. With that in mind, one way to modify Cummins' explanatory schema is to supplement the rather general *filling instructions* Cummins provides. The idea is to specify one schema for functional explanations in general, and then to give different *types* of functional explanations from it by applying different *types* of filling instructions.

To create such a modified Cummins-analysis, I want to accomplish the following tasks: (a) segregate the various components of Cummins' schema for purposes of clarity, (b) add the requirement that the schema pick out "normal functions", (c) supplement this with an additional etiological component, and (d) create filling instructions for this modified Cummins-analysis to be used in biological explanations. Thereafter, I want to apply this schema to several biological examples.

a. Cummins' schema. On Cummins' schema, it is appropriate to say that the function of x in s is to *phi* when we are speaking against a background of an analytical account A "of s 's capacity to *psi* just in case x is capable of *phing* in s and A appropriately and adequately accounts for s 's capacity to *psi* by, in part, appealing to the capacity of x to *phi* in s " (1975:762). The following breaks out the individual elements contained within Cummins' schema:

The function of *x* in *s* is to *phi* (relative to analytical account A) *just in case*:

1. *x* is capable of *phūing* in *s*,
2. *s* is capable of *psūing*, and
3. *s*'s capacity to *psi* is increased by *x*'s capacity to *phi* in *s*,

as specified by analytic account A.

As it is my purpose to modify Cummins schema, breaking out these components in this manner will be helpful in highlighting subsequent additions to the schema and in illuminating the impact they have on the schema itself.²

b. Adding the requirement that the schema pick out "normal functions."

Cummins' account has been criticized for being unable to adequately handle the concept of "malfunctions." This criticism calls attention to the fact that since Cummins' schema is only concerned with a *current* analysis of some causal role of an item within a containing system, the schema thereby fails to address "normal functions" which must be cashed out in terms of historical context (Millikan 1989:294). To expand the scope of this schema to specify that *x* be the "normal function" of *s*, both *x* and *s* must be operating under normal circumstances. For instance, the above schema could not be successfully applied to a defective heart which was incapable of pumping, since the capacities of both *x* and *s* are considered in the present system by Cummins. Thus, "if something functions as a pump in a system *s* or if the function of something in a system *s* is to pump, then it must be capable of pumping in *s*" (1975:757).

Cummins' concept is preserved by the parenthetical additions reflected below while at the same time, those additions begin to address Millikan's critique.

The function of *x* in *s* is to *phi* (relative to analytical background account A) *just in case*:

1. *x* is capable (under normal circumstances, when *x* is not defective in *s*) of *phūing* in *s*,
2. *s* is capable (under normal circumstances, when *s* is not defective) of *psūing*,

- and
3. *s*'s capacity to *psi* is increased by *x*'s capacity to *phi* in *s*,

as specified by analytical account A.

Note that while Cummins' account still describes a capacity of the item to have a certain consequence, the addition of the requirement concerning "normal circumstances" begins to place this analysis within some type of historical context of what is "normal" for both the item and the system.

c. **Adding an additional etiological element.** The next issue to be addressed in relationship to Cummins' analysis is whether it can be expanded to produce a larger explanatory scope by encompassing subject matter addressed by etiological accounts. Cummins' schema has been criticized for frequently failing to create appropriate functional categories (Neander 1991a:181). Of primary concern is not only the creation of these categories but also the explication of why the categories are appropriate.

While Cummins suggests that his analysis shows that functional analysis may be applied in biology independently of evolutionary considerations (p. 756), my present objective is to enlarge the focus of Cummins' schema to include evolutionary considerations rather than avoid them. Evolutionary considerations often provide the explanatory punch in an explanation of the persistence over time of the item *x* whose function provides the survival advantage to a member of a species or genus of type *s*. This is normally so because within biology those evolutionary considerations demonstrate the workings of the selective mechanism which in turn supplies the explication of the persistence of the component.

The lesson produced by etiological accounts is that such an expansion of Cummins account should be possible by the addition to his schema of the concept of *proper functions*.

This concept can be easily added to Cummins' account. A Cummins-analysis is designed to help explain a trait's present capacity to perform a particular role in a system in relation to some capacity of the containing system. Cummins' account can be modified to incorporate etiological functional explanation by supplementing his schema to include the relevant historical context for the functional role. This requires that the schema include a causal history of the component (*x*) of the system (*s*) which gives its functional role. Both Cummins and Mitchell agree that "a function is a consequence of some component of a system". Mitchell only adds the requirement that for "(t)he consequence to be a function, (it) must have played an essential role in the causal history issuing in the presence of that very component" (Mitchell 1995:39). This is accommodated by the addition to Cummins' schema of sentence four below.

The function of *x* in *s* is to *phi* (relative to analytical background account A) *just in case*:

1. *x* is capable (under normal circumstances, when *x* is not defective in *s*) of *phi*ing in *s*,
2. *s* is capable (under normal circumstances, when *s* is not defective) of *psi*ing, and
3. *s*'s capacity to *psi* is increased by *x*'s capacity to *phi* in *s*, and
4. the increased capacity of *s* to *psi* ultimately contributes to the persistence over time of *x*,

as specified in analytical account A.

d. Adding filling instructions. This modified Cummins-analysis needs *filling instructions* appropriate for replacing the dummy letters of each schematic sentence. Since the present subject matter is biology, the following filling instructions should be applied to the modified Cummins-analysis when it is applied to the analysis of biological systems, at least when natural selection is the appropriate selection mechanism:

s is a typical member of a particular species or genus,

x is a trait or behavior of members of that species or genus,

phi is some trait or behavior of *x*

psi is *s*'s capacity to survive and/or reproduce successfully, or some trait or behavior of *s* that ultimately contributes to such survival and/or reproductive success in an environment.

2. Applying the modified Cummins-analysis to biological examples. With this completed, the next task is to apply this schema with filling instructions to some biological examples.

a. Larry Wright and the function of porcupine quills. I begin by applying the modified Cummins-analysis to one of Larry Wright's biological examples (1976). Wright suggested that the function of quills in porcupines is to protect the little beasts from hungry predators. In Wright's formulation "*x* is there *because* it does *z*, and *z* is a consequence of *x* being there," where *x* is the porcupine quills and *z* is their capacity to provide protection from predators. With Wright's lead, the following filling instructions are created:

s = porcupines
x = quills
phi = provide protection from predators (in environment *E*)
psi = survive and have reproductive success (in environment *E*)

When these filling instructions are inserted into the modified Cummins-analysis, the following explication of Wright's explanation is produced. To say that the function of quills in

porcupines is to protect the little beasts from hungry predators is just to say:

1. Quills are capable of providing protection from predators for porcupines.
2. Porcupines are capable of surviving and having reproductive success.
3. The capacity of porcupines to survive and have reproductive success is increased by the capacity of quills to provide protection from predators for porcupines.

4. The increased capacity of porcupines to survive and have reproductive success ultimately contributes to the persistence over time of quills in porcupines,

as specified in analytical account A which incorporates environment E.

Even in this simple example, the use of a modified Cummins-analysis explicates the *because* in Wright's account by the explanatory impact of sentences three and four above. These illuminate the underlying selection mechanism in terms of the capacities of the normal causal role of the particular item within a particular containing system over time. This provides substantial meaning to Wright's claim that "x is there *because* it does z."

b. Bigelow and Pargetter and the function of the bee-sting. Bigelow and Pargetter suggest that when we describe the function of some biological character, we do so by reference to some future event or effect which may never occur, such as the sting of a bee. They argue that since most bees never use their stings, that the character involved should be viewed in terms of the propensities it confers, not upon its prior causal history. Intuitively, this answer seems partially correct but incomplete. The application of a modified Cummins-analysis should help to illustrate why that is so.

The following filling instructions are used for such an exercise:

s= beehive

x= bee-stings

phi= provide protection of the beehive from outside invaders in environment E

psi= survive and have reproductive success in environment E

When these filling instructions are inserted into the modified Cummins-analysis, an explication of the following functional ascription is produced. The function of bee-stings is to provide protection of the beehive from outside invaders *just in case*:

1. Bee-stings are capable of providing protection of the beehive from outside

invaders,

2. Beehives are capable of surviving and having reproductive success,
3. The capacity of beehives to survive and have reproductive success is increased by the capacity of bee-stings to provide protection of the beehive from outside invaders, and
4. The increased capacity of beehives to survive and have reproductive success ultimately contributes to the persistence over time of bee-stings (since the production of workers with defensive stings is heritable),

as specified in an analytical account A which incorporates environment E.

This application of the modified Cummins-analysis provides the focus to identify the problem with the analysis of Bigelow and Pargetter, i.e., they have identified the wrong system in discussing the causal role of the bee-sting in protecting *beehives*. Bigelow and Pargetter talk about the individual bee as a system and highlight the fact that a particular bee may not use *her* sting, and therefore it must be the propensity of being able to sting, not the use of the sting that is important for the bee. But, as the above analysis points out, it is the beehive, not an individual bee, that receives the survival advantage. This is additionally confirmed by the fact that the use of the bee-sting causes the death of the individual bee, a fact that confirms the intuitive response that Bigelow and Pargetter were partially correct in asserting that the bee-sting did not provide a survival advantage for the individual bee. It doesn't. But it does provide a survival advantage for the beehive, which is its function within that system.

c. **D. M. Walsh and the function of the wings of penguins.** In an article (1996) promoting his view of relational function attribution, D. M. Walsh raises the issue of functional attribution when utility and etiology come apart, such as when the way in which a trait contributes to fitness (its utility) in the recent past is different than the way it contributed to fitness in the not so recent past. He notes that this issue is raised when we ask a question such

as, 'why do penguins have wings?' In the more distant past, the functional response would have been to the effect that the wings enabled them to fly. However, in the more recent past, the answer is that the penguins' wings function as swimming appendages that propel them through and out of the water.

This example provides the opportunity to apply the modified Cummins-analysis to an example termed by some as 'variants' (Wright 1976:112) and by others as 'vestigial traits' (Griffiths 1993). While Wright warned that "an analysis would be suspect if it included them too centrally," it remains of interest to address such an example. Beginning with Walsh's language, the following filling instructions are created:

s = penguins
 x = wings
 ϕ = propel them through and out of the water, thus avoiding predators in environment E
 ψ = survive and have reproductive success in environment E

When these filling instructions are inserted into the modified Cummins-analysis, the following explication of Walsh's example is produced. The function of the wings of penguins is to propel them through and out of the water, thus avoiding predators *just in case*:

1. Wings are capable of propelling penguins through and out of the water, thus avoiding predators.
2. Penguins are capable of surviving and having reproductive success.
3. The capacity of penguins to survive and have reproductive success is increased by the capacity of wings to propel penguins through and out of the water, thus avoiding predators.
4. The increased capacity of penguins to survive and have reproductive success ultimately contributes to the persistence over time of wings in penguins (since wingness is differentially heritable)

as is specified in an analytical account A which incorporates environment E.

This application of the modified Cummins-analysis focuses the explication upon *why the wings continue to be present in penguins*, despite the loss of their original function for which they were originally selected. Of importance is the fact that the wings continue to be selected as a result of the causal role they play for penguins within their environment. While this partial explanation does not address the question of why penguin wings originally functioned in order to allow penguins to fly, it none the less provides an appropriate partial response to the question of 'why do penguins have wings?' However, it provides a nearly complete explanation to the lesser question of 'why do penguins *still* have wings?'

3. A return to Mitchell's observation that dispositional and etiological accounts are not incompatible. Earlier I had referenced the conclusion reached in a recent article by Sandra Mitchell (1995) that dispositional and etiological accounts are not inconsistent. I deferred a more thorough discussion of Mitchell's observations in order to first discuss the process of modifying Cummins' account in order to be able to have that modification available for use within a more detailed discussion. I now return to a discussion of Mitchell's claims.

a. Biological function is a discoverable property. Sandra Mitchell argues that the biological function of a trait is a real, discoverable property of natural organisms (1995:42). Mitchell illustrates this point by reciting the history of the discussions directed to the case of male primates who are often larger than their female counterparts. The question arises as to what function the larger size served for the males. Two dissimilar answers were presented at different times in history. Darwin (1871) suggested that large size was a consequence of sexual selection in that larger size resulted in greater success in male/male competitions for mates,

while Selander (1972) suggested that large size allowed the males to exploit different food resources. If Darwin's functional attribution was correct then this dimorphism should be found in polygamous species of primates where strong competition produces a "winner" who acquires a harem of females while the "loser" acquires none. Alternatively, if Selander's functional ascription was appropriate, then this dimorphism should be found among monogamous species of primates that feed together. Clutton-Brock and Harvey's research (1977) furnished evidence that the dimorphism appeared in polygamous species thereby providing support for Darwin's hypothesis.

Against this background of a discussion concerning different selection possibilities, Darwin's functional ascription of increased size as providing greater success in male/male competitions for mates is seen by Mitchell as being causally responsible for the continuation of large size in male primates since that selection process provides an increased capacity for genetic transmission of the structural characteristic of large body size. This consequence is properly ascribed as a function in that large body size is selected over alternatives on the basis of its consequence, and large body size is reproduced as a direct result of that selection process (Mitchell 1989, 1993, 1995). In other words, the function of large body size to provide greater success in male/male competitions for mates is seen by Mitchell as a "proper function."

b. Applying the modified Cummins-analysis to Mitchell's example. Since Mitchell's concept of "proper function" was added to the modified Cummins-analysis in order to enable that schema to pick out "proper functions," this example presents the opportunity to see if the modification does what it was designed to do. To do this, the following filling instructions are created:

s = primate populations that contain males with large body size
x = large body size in male primates
phi = capacity to provide greater success in male/male competitions for mates
psi = survive and contribute to the genetic transmission of the structural characteristic of large body size

Thus, the claim that the function of large body size in male primates is to provide greater success in male/male competitions for mates amounts to (on this modified Cummins-analysis) the claim that:

1. Large body size in male primates is capable of providing greater success for larger males in male/male competitions for mates among other male primates.
2. Primate populations that include male primates with large body size are capable of *surviving* and contributing to the genetic transmission of the structural characteristic of large body size (reproductive success concerning this dimorphic gene) to succeeding generations.
3. The capacity of primate populations containing male with large body size to *survive* and contribute to the genetic transmission of the structural characteristic of large body size is increased by the capacity of large body size in males to provide greater success in male/male competitions for mates.
4. The increased capacity of primate populations containing male with large body size to *survive* and contribute to the genetic transmission of the structural characteristic of large body size ultimately contributes to the persistence over time of large body size in male primates.

It would appear from the above application that modified-Cummins analysis does explicate Darwin's functional ascription in response to the question, 'what is the function of large size in male primates?'

The italicized portions of the analysis highlight a fact that makes this example more complex than it first may have seemed. Large body size, as used by Mitchell above, explains an increased reproductive advantage, not an increased survival advantage. But surely, the later is also true, that large body size in male primates provides a survival advantage due to an

increased capacity to provide protection from predators. To include this information would require that the functional ascription be expanded to be something like the following. The claim that the function of large body size in male primates is to be successful in combative encounters, both with predators as well as in male/male competitions for mates amounts to the following claim:

1. Large body size in male primates is capable of providing greater success for larger males in male/male competitions for mates among other male primates in primate populations.
2. Primate populations that include male primates with large body size are capable of *surviving* and contributing to the genetic transmission of the structural characteristic of large body size (reproductive success concerning this dimorphic gene) to succeeding generations.
3. The capacity of primate populations containing male with large body size to *survive* and contribute to the genetic transmission of the structural characteristic of large body size in males is increased by the capacity of large body size in males to provide greater success in male/male competitions for mates.
4. The increased capacity of primate populations containing male with large body size to *survive* and contribute to the genetic transmission of the structural characteristic of large body size ultimately contributes to the persistence over time of large body size in male primates.

This seems a more complete explanation of the function of large body size in male primates.

And, while it is somewhat expanded in scope, it does not diminish the points made by Mitchell.

c. **Mitchell's conclusion that dispositional and etiological accounts are not competing accounts.** Mitchell contrasts the claims of dispositional and etiological accounts as those accounts may be applied to her example of sexual dimorphism in primates. For simplicity, I will restrict my discussion here to Mitchell's consideration of "success in combat" arising in male/male competition for mates (as opposed to the broader consideration of

"success in combat" which would additionally include the causal role of protection from predators).

(1). **Mitchell's claim regarding the dispositional account.** The dispositional account tells us how having the structural characteristic of large body size provides a propensity to have a certain consequence (success in male/male competition for mates) and how that contributes to the reproductive success of individuals with that trait. In other words, individuals with this trait have a propensity to dominate the competition which produces a "winner" who acquires a harem of females while the "loser" acquires none. As Mitchell states, "What function explains on the dispositional view is why a large male primate reproduces more successfully than a small one." That provides an answer to question, 'What is the function of the trait in the current system?'

(2) **Mitchell's claim as to the etiological account.** The etiological account tells us how having the structural characteristic of large body size with the function of contributing to success in male/male competition for mates contributes to the persistence of the trait. In other words, it explains why the males are larger by appealing to the selection history which took the differential effect on male/male competition as a cause for differential female acquisition and therefore as a cause for differential reproductive success. Because of the heritability of this structural characteristic, this selection process *would propagate*³ larger body size over smaller body size through time. As Mitchell states, "What function explains on the etiological view is why male primates are large." That provides an answer to the question, 'Why is the trait there?'

(3) **Confirming Mitchell's claim as to the dispositional account.** The result of the

modified Cummins-analysis using this example confirms Mitchell's claims. The first three components of this schema are derived directly from Cummins' dispositional account and retain their dispositional inquiry into the causal role of the trait in the system. Component three produced the following statement concerning this example:

3. The capacity of male primates with large body size to survive and contribute to the genetic transmission of the structural characteristic of large body size is increased by the capacity of large body size to provide greater success in male/male competitions for mates.

This is nearly identical to Mitchell's claim that, "What function explains on the dispositional view is why a large male primate reproduces more successfully than a small one."

(4) Confirming Mitchell's claim as to the etiological account. Recall that the fourth component of the modified Cummins-analysis was added in order to incorporate the concept of proper functions into this schema. This was done by the requirement that the schema include a causal history of the component (*x*) of the system (*s*) which gives its functional role. Component four produced the following statement concerning this example:

4. The increased capacity of male primates with large body size to survive and contribute to the genetic transmission of the structural characteristic of large body size ultimately contributes to the persistence over time of large body size in male primates.

This is nearly identical to Mitchell's claim that, "What function explains on the etiological view is why male primates are large."

4. Cashing out the causal claims in Mitchell's example. If ahistorical accounts are partly historical while historical accounts are partly dispositional, then the explanations produced by these different accounts do not seem to be so dissimilar in nature. Consider, the following comparison of the causal claims made in support of each type of account for

Mitchell's example.

a. The dispositional argument. Rather than appealing to the causal history of the propagation of the trait through natural selection, the dispositional account simply identifies the causal role played by large body size to be its reproduction-enhancing propensity. In order to identify the causal role of the trait as a reproduction-enhancing propensity, the dispositional account implies that large body size *is the causal basis for*:

1. a propensity for future successful male/male competition, which in turn *is the causal basis for*:
2. a propensity for future successful female acquisition, which in turn *is the causal basis for*:
3. a propensity for future successful reproductive success, which in turn (given the heritability of the trait) *is the causal basis for*:
4. a propensity for future successful propagation of larger body size over smaller body size.

b. The etiological argument. Here, if we begin by comparing the dispositional term 'propensity' with Mitchell's use of the term 'differential effect', then we can quickly note that the significant difference between the two terms is that the later is a *realized* propensity. And, that is the only important dissimilarity in the causal claims of the dispositional and etiological arguments. For, on the etiological account, the causal history of the consequence(s) of possessing large body size explains that this trait, over time, *is the cause of*.

1. a past realized propensity for successful male/male competition, which in turn *is the cause of*:
2. a past realized propensity for successful female acquisition, which in turn *is the cause of*:

3. a past realized propensity for successful reproductive success, which in turn (given the heritability of the trait) *is the cause of*:
4. a past realized propensity for successful propagation of larger body size over small body size.

It is hard to imagine how these analyses could be much more similar.

Section Five:

Other biological mechanisms for functional accounts.

Part of the debate about functions within biology is related to questions concerning the history of a trait or functional component in a system. To be functioning properly, must it still perform the same work that historically caused it to be selected? Must it have persisted over time up to and including the present in order to be labeled a "function"? Must this history include the story of the origin of the trait? These concerns are closely related to another. Are there other mechanisms besides natural selection available for picking out biological "functions"?

1. **Biological selection mechanisms other than natural selection and the adaptionism issue.** The leading proponent of the view that mechanisms other than natural selection give rise to important traits *within biology* is Stephen Jay Gould. He argues for a "pluralist" view of evolutionary theory. He argues that in order to explain the basic patterns and regularities of the evolutionary pathways of life, we must do more than *just* rely on the principle of natural selection. While "pluralists" accept natural selection as a paramount mechanism, they also argue for the presence of a set of mechanisms, as well as a large role for

history's unpredictable contingencies (Gould 1997b:47).

To explain the observed patterns of evolution, Gould argues that the theory of natural selection must work in concert with at least three other mechanisms: (1) punctuated equilibrium, (2) nonadaptive side consequences, and (3) contingency and chance. Punctuated equilibrium is the view that most new species originate in a geological "moment" and that species generally do not change in any substantial or directional way during their geological lifetimes. Since a geological "moment" of a single rock layer may represent many thousands of years, this theory does not challenge accepted ideas about the rates at which species emerge, however, it does challenge conventional Darwinian thought about gradual change over geological periods as being slow and continuous transformations of single populations. While this idea suggests an alternative *way* in which natural selection may occur, it does not represent a *new* mechanism by which evolution occurs, but offers an alternative account of how selection works.

Gould employs the term "spandrels" to describe traits arising from his second corrective to natural selection. Traits may arise as nonadaptive side consequences of other, selected traits. These constitute the major category of important evolutionary features that *did not arise as adaptations*, i.e., they arose as structural byproducts which were later co-opted for useful purposes. One illustration of this category is the human brain which reached its current size and conformation tens of thousands of years before reading and writing were developed. The brain's capacity for reading and writing was not selected for. Rather, the ability to read and write was probably an evolutionary by-product of the brain capacity needed for tool-making, which was selected for. Note that Gould defines "spandrels" as being co-opted

features that fulfill useful purposes. Thus, despite the fact that reading and writing did not arise as an adaption, *it has now become highly adaptive for humans.*

This raises the question of whether traits that *arise* as nonadaptive side consequences are proper subjects for functional explanations even if they later become adaptive. The first part of the answer to this question is that these traits can be the subject of functional explanations at the dispositional level. In Gould's example of reading and writing developing as a side consequence of increased human brain capacity, we can easily observe that the consequence of the trait can be the subject of a functional explanation in a dispositional account such as that of Robert Cummins. Reading and writing is *one* of the brain's contributions to the fitness of modern humans -- it contributes to the propensity to survive and reproduce more successfully than individuals and species who lack the ability. Thus, it is clear that reading and writing is a "Cummins-function" of the human brain. Reading and writing is *adaptive* as that term is normally used in biology, i.e., the trait contributes to the fitness of the organism in its current environment.

The more controversial question is whether reading and writing is a function of the human brain in the etiological sense. Does the history of the trait explain its persistence over time? In other words, is reading and writing an *adaptation* as that term is normally used in biology, i.e., has it been selected for due to past contributions to fitness? Critical to answering that question is a clearer understanding of what is meant by the term "selection." Does "selection of a trait" encompass the origin of the trait or may a trait become selected at a time long after it originates for other reasons? The significance of which of these views is employed can be illustrated by contrasting this language with the standard functional claim that something

"exists in order to". If "selection" and "exists" are taken to be interchangeable, then functional explanations may be limited by the breadth of the definition given to "selection".

Recall my prior discussion about the ambiguity related to the "exists/persists" issue in dispositional versus etiological accounts. There I suggested that Kincaid was correct in suggesting that "persists" is a more appropriate term than "exists" in etiological accounts. But the "adaptationism" issue adds a further ambiguity, since some biologists hold that a proper explanation of the persistence (existence) of a trait requires an account which includes the origin of the trait, while for other biologists the persistence or existence of a trait only requires an explanation of its function in the recent biological past. Peter Godfrey-Smith clearly identifies the main philosophical issue when he states that the most important difficulty involved with this issue is "the extent to which functional characterization requires a commitment to some form of adaptionism" (Godfrey-Smith 1994:344).

Returning to the question of whether reading and writing is a *function* of the capacity of the human brain in an etiological sense illustrates the problems related to the adaptationism issue. Certainly the story of how something became a "spandrel" has explanatory significance, i.e., it explains the initial creation of the component as a byproduct which was later co-opted for a useful purpose. But does this explain the item's function? For Gould it does not. Gould apparently sees the explanation of a trait's function as inclusive of an explanation of the trait's origin (Gould and Lewontin 1978; Gould and Vrba 1982).

Gould perceives adaptations as being shaped by natural selection to perform their present causal roles and he restricts functional explanations to this category. He argues that we would commit "an egregious logical error if we argued that these secondary uses explain the *existence*

of a spandrel" (1997:52) (emphasis added). It appears that Gould requires a functional explanation of a trait's *existence* to include an account of "how something came into being". In other words, Gould's argument appears to be that something does not *exist in order to* perform some function unless it was a product of natural selection.

Godfrey-Smith does not require such a commitment, arguing that functional explanation applies in the broader sense of whether "a function has some link to an explanation of why the functionally characterized thing exists, in the form it does" (1994:344). Godfrey-Smith argues for a "modern history" concept of biological functions as "dispositions or effects a trait has which explain the recent maintenance of the trait under natural selection" (ibid). Although Godfrey-Smith has not written on the subject of reading and writing as a function of the human brain, it appears that under his view the original development of the capacity of the human brain (assumedly for the purpose of tool making) is the "ancient history" of this functional story, and the development of reading and writing is the "modern history" of the same story.

Both positions make important points about the nature of biological functions. Gould's description of the various mechanisms that give rise to traits adds explanatory depth to our understanding of the operation of these traits. That reading and writing is the product of a structure created and selected for by nature for a different reason is an important aspect of the detailed causal story, the ideal explanatory text. But, Godfrey-Smith is also correct in arguing that "modern histories" as well as "ancient histories" can furnish genuine *functional* explanations for why something exists now. In the example of the capacities of human brains, although the "ancient history" tells an important story of creation and selection for tool making

purposes, it would surely not be correct to claim that such capacities are presently, or were in the *recent* past, being selected for the survival advantage provided by the ability to make tools for killing animals and harvesting their byproducts.

Isn't the "recent history" of the causal role of the "wings" of penguins satisfactory for an ascription of their current function, despite the fact that these "wings" have a different "ancient history" related to flight? Both the recent and the ancient histories of penguin "wings" provide functionally explanatory information which is important to and a part of a larger more complete explanation about why penguins' "wings" exist now. The same is true about the histories related to reading and writing.

2. The consequences of acknowledging both "modern" and "ancient" functional histories. The above discussion makes it easier to understand the significance of the question as to why a trait of a component of a system may have a biological function even though it does not still perform the same work that historically caused it to be selected. An ideally complete explanation would encompass both "ancient" and "recent" functions. Under such a view, while many traits are both adaptive and adaptations, some will not be adaptive in that they do not have current functions and some will not be adaptations in the strict sense that the history of their origin is not part of their functional explanation.

Regardless of how these issues are resolved within biology, these are theoretical issues that should be of interest to those outside of biology who have an interest in functional explanation. While both "ancient" and "modern" explanations are only partial explanations, they are part of a more complete, cumulative explanation. This is illustrated by the more complete explanations (e.g., the functions of the "wings" of penguins, or the capacities of the

human brain) that are created when partial explanations are combined. It should be the primary business of scientific explanation to fill in as much of an ideal explanatory text as possible. Obviously, there will be times when more specific information is useful, and on those occasions scientists can employ whatever explanation of the function of a trait of a component in a system is to the point. But because explanation is cumulative, nothing is lost and much gained from additional functional information about a trait.

Section Six:

Some Summary Thoughts

While Ruth Millikan, a linguist, is credited with beginning the theoretical discussion concerning the concept of "proper functions", it should not be surprising that such a concept was picked up and discussed at length in biology. The interrelationship between proper functions, functional categories, malfunctions, and biological normality has allowed functional explanation to perform important work for etiological accounts within biology. Most often the widely recognized selection mechanism that underlies such proper functions within biology is the mechanism of natural selection. Thus, tying proper functions to the operation of this selection mechanism has seemed an obvious move.

Yet this linkage need not be exclusive. Stephen Jay Gould makes a compelling argument for a pluralist view of evolutionary theory that includes recognition of the operation of other selection mechanisms. However, his view should not place a severe limitation upon functional ascriptions.

One of the functions of the human brain is reading and writing. The ability to read and

write has played an important role in the causal history of human beings in their present form. An account of functional explanation that employs the concept of "proper functions" should label the function of reading and writing as a "proper function" despite the fact that the underlying mechanism responsible for its persistence into the present is the co-opting of structural byproducts for useful purposes, rather than the direct product of the most often cited mechanism, natural selection.

I have suggested that when etiological information is added to the filling instructions for Robert Cummins' explanatory schema that in a resultant explanation the causal claims behind both etiological and dispositional explanations become more transparent. Those explanations are perfectly consistent and even complementary. Those explanations are interwoven portions of a larger *ideal explanatory text*. Such a conclusion is sustained by the broader view of explanation discussed herein, the notion of explanation as causal, partial and cumulative.

1. I want to return later to a discussion of other selection mechanisms that could form the basis for proper functions *in biology*, but it seems more important not to divert the present discussion from the clear relationship between proper functions and functional categories.
2. This section on analytical exercises arose largely out of a series of discussions with Jim Hawthorne. Along with many of the ideas, the concept for breaking out the components of Cummins' schema was his along with the initial draft of the language for incorporating the concept of "proper functions" into the modified Cummins-analysis.
3. The tense of this verb as well as the abbreviated nature of this account confirms my earlier observation that the selection mechanism of natural selection is historically applied as a propensity. The etiological account would not explain in terms of function if it merely listed the history of the trait from generation to generation.

Chapter Four

SOCIAL FUNCTIONS

In the prior chapter we saw how natural selection can perform the work in biological theories of picking out *biological* proper functions. Its ability to distinguish between idiosyncratic and functionally important traits is the primary reason for the success of functional explanation in biology, or so it has been argued within biology. The ability to solve the problems concerning which causal sequences create functionally important traits leads to the identification of functional categories that are the basis for the concept of biological normality. Etiological accounts of functional explanation help identify these categories and when this information is combined with a Cummins-analysis of the current function of a component of a system, the resulting explanation should be a significant part of an ideal explanatory text of the evolution and role of specific biological functions.

In this chapter I argue that for functional explanation to succeed in the social sciences it must employ a concept of *social* proper functions. As in biology, this requires the ability to make the connection between the consequence of the trait and the current presence of the trait. Showing how the differential sorting advantage created by the consequence gives rise to the selection of the trait over alternative traits, thereby explaining why the attribute persisted over time does this. This in turn leads to the creation of functional categories and implements the sociological equivalent of the concept of biological normality – i.e., the concept of what properly functioning members of a social category share in common with each other. When this analysis is added to

information provided by an analysis of the current function of a component of a system, a causal explanation can be provided that forms a significant part of an ideal explanatory text of the subject matter of interest.

I begin the argument for the concept of *social* proper functions by discussing the critical role played by selection mechanisms in functional explanations. I review the obvious reasons why natural selection cannot be the selection mechanism at work in the social sciences. I argue that Harold Kincaid is correct in asserting that several other mechanisms can make the explanatory linkage necessary for functional explanations. What is important for this view of functional explanation is the ability to make the contribution-to-survival/persistence-over-time connection.

I review and respond to Elster's "missing mechanism" criticism of functional theory within the social sciences. While Elster is obviously correct in suggesting the importance of explicating selection mechanisms, his claim that they are necessary conditions for social explanation is too strong.

I next address two recent arguments that I suggest are false dichotomies. I argue that both Rosenberg's 'Darwin or Durkheim' argument and Pettit's 'Intentionality or Natural Selection' argument fail for the same reason – their disjunctive premises do not present jointly exclusive alternatives. I argue that there can be meaningful explanation within the social sciences and that these arguments fail because they miss more moderate theoretical alternatives.

This is followed by a discussion of the problems presented by viewing intentional human selection as a mechanism for picking out social proper functions. I argue that intentional human design too often employs goal-directedness, creates proper functions at

the level of tokens and can make selections without alternative candidates – contrary to the lessons learned from biology. However, I note that when: (1) human selection is restricted to circumstances where its selection is the result of reasons different than those employed in its initial design and (2) the selection mechanism is allowed to operate over an extended period of time, that these two conditions appear to cure the major theoretical concerns, i.e., that functional ascriptions not be teleological, that functional categories be created at the level of types, and that an actual selection mechanism be at work.

Such a division already exists within the category of human selection – the division between latent and manifest functions. I argue that this is a significant theoretical distinction since this isolates intentional and teleological consequences from unintentional and non-teleological consequences, which in turn allows the latent function grouping to create functional categories at the level of types.

I discuss R.K. Merton's methodological approach to this subject matter and attempt to further explicate the nature of the claim made when a scientist describes something as a latent function. I begin a defense of the *core* elements of Merton's concepts against Elster's criticism and in so doing I employ a series of analytical exercises using Merton's example of the Hopi rain dance.

At the end of those exercises, I suggest that the concept of social proper functions can be successfully employed within the social sciences. These exercises demonstrate a methodology for explicating the differential sorting processes that can undergird functional explanation in the social sciences. I suggest that this strategy builds on the work of earlier functionalists such as Merton, is causal in nature and can produce an ever-increasing portion of a larger ideal explanatory test. Lastly, I argue that this approach

solves the social science's long existing problem of explaining change over time. The use of such an explanatory schema allows the social sciences to employ a view of functional explanation that is causal, partial and cumulative.

Section One

Selection Mechanisms

In Chapter Three I noted that the concept of proper functions distinguishes between idiosyncratic and functionally important traits. Functionally important traits are exactly that, traits whose consequences help explain their continued presence. In order to possess this explanatory power, there needs to be a connection between the consequence of the trait and the current presence of the trait. This occurs when some consequence of the trait gives rise to the *selection* of it over alternate traits. An explanation that meets this explanatory burden *explains why* the attribute persisted over time; it did so because its consequences aided or enhanced its survival.

Thus, a functionally important trait may be revealed by the selection history of the trait. Functional explanations which explicate such selection processes should reveal the causal sequences which show how the trait was selected for its function, and how its function promoted the survival of the system to which it contributes. Discerning the underlying causal processes at work and identifying the operation of the traits in particular cases solves the problem of determining which causal sequences create these functionally important traits.

Mechanisms that connect a consequence of a trait with its *selection* over alternative traits demonstrate the causal basis for *selection*. At the heart of this selection

process is the fact that the attribute contributes to the survival of the system, thereby resulting in its increased or “differential” sorting over alternate systems. In biology, functions that satisfy this type of explanatory accountability are called “proper functions”. The selection mechanism, which normally performs this work in biology, is natural selection. However, as the discussion in Chapter Three concerning reading and writing as a function of the human brain demonstrates, there are other mechanisms at work, even in biology.

This is easier to discern by focusing on the standard functional claim that “A exists (persists over time) in order to B”. Basically three components create this claim, (1) the assertion that A causes B in the system, (2) the assertion that the presence of B in the system produces a survival advantage for the system, and (3) the assertion that this survival advantage helps A persist over time. This concept clearly links contributions-to-survival with persistence-over-time. While a differential sorting model such as natural selection can make this connection, it is not the only mechanism capable of performing this work.

Recall that in Chapter Three I argued that reading and writing was a proper function of the human brain despite the fact that natural selection was not the causal process at work. While natural selection was involved in the “origin” of the creation of a certain capacity of the human brain (perhaps for the purpose of tool making), a *different mechanism* explains the more recent history of this capacity being co-opted for a different useful purpose. Reading and writing *did* make a contribution to survival for humans, and that enhanced survival propensity *did* help humans persist over time.

Harold Kincaid, whose discussions in this area seem particularly lucid, argues that “(a)ny process that allows A to persist because it causes B will do – regardless of whether the process involves entities that reproduce, compete and inherit traits” (1996:117). Kincaid’s analysis emphasizes the vital role of the consequence of the trait creating a differential sorting process. Mechanisms that accomplish this provide the survival advantage that A receives as a result of causing B. This in turn explains that A persists over time through the help of this survival advantage. And, while natural selection can perform this differential sorting, Kincaid notes that it is “really just a special case of the more general process that these conditions pick out”.

Critics have suggested that functional explanation in the social sciences fails because it does not have available a mechanism similar to the mechanism at work in biology (Elster 1979; Rosenberg 1988; Pettit 1996). However, if Kincaid’s claim that other differential sorting mechanisms can perform similar work in explicating the contribution-to-survival/persistence-over-time connection is correct, and I think it is, then these arguments are mistaken. Kincaid suggests that other possible social mechanisms that might make this connection are acquired characteristics passed on through learning or mimicking, complex combinations of intentional action, unintended consequences of intentional action, and differential survival of social practices (1996:112).

Section Two

Natural selection is not a social mechanism

Social Darwinism’s short-lived existence as an anthropological theory has been generally attributed to its ill-conceived foundation of Eurocentric elitism. More

specifically, it tended to define survival of the fittest in terms of social and financial success. This created the tautological axiom that; “Those individuals or organizations which have actual social and financial success will produce social and financial success more successfully.” Despite the theoretical shallowness that led to the demise of this application of natural selection to social explanation, it is worth re-emphasizing the substantial reasons why the underlying mechanism at work in human cultural change *cannot* be natural selection.

In Chapter Two reference was made to anthropologist Robert Lowie’s image of “diffusionism laying the axe to evolutionism.” At the center of this classic paradigm shift within anthropology was the concept that change in human culture was often the collateral consequence of diffusion instead of a direct evolutionary consequence. As Stephen Jay Gould notes, human cultural change is not basically Darwinian since much of human behavior arises by culturally induced rather than genetically coded change. Human cultural change need not even follow genealogical lines, the most basic requirement of a Darwinian evolutionary process. It is not necessary for individuals or cultural groups to be related in order to borrow from each other, as is easily illustrated by the example of the effects upon Italian culture of Marco Polo’s trip to China (Gould 1997b:52). The large differences between the methodologies of change involved in human cultural transformation versus genetic Darwinian evolution graphically demonstrate that the underlying mechanisms need not always be the same.

Another way of illuminating the difference between these mechanisms arose within the debate concerning the nature of the development of science. Toulmin applied Darwin’s Theory of Evolution as a model for the historical development of science.

Cohen responded that there is an important disanalogy between the evolution of science and the evolution of species that makes the application of a Darwinian model inappropriate (Cohen 1973). Cohen argued that in science, the generation and selection among alternative concepts is “coupled” in the sense that the judgments of scientists are responsible for both. However, the same coupling does not hold for natural selection in organic evolution. There mutations arise in a spontaneous, random manner while selection may occur in an ecologically different future environment than that which generated the mutation. Thus, Cohen refers to the processes of mutation and selection in organic evolution as “uncoupled”.

Finally, it seems an equally obvious point that human cultural transformation occurs at an extraordinarily faster pace than that of genetic Darwinian evolution. And, the difference between the operational speed of these selection mechanisms only increases with technological change and the resultant explosion of increased access to cultural information. A modern day Marco Polo looks to the World Wide Web, not to a dessert caravan, for instantaneous passage that allows him to borrow cultural information from the now not-so-far-east.

Section Three

Elster’s Missing Mechanism Criticism

Jon Elster has long been a critic of functionalist social theory specifically and of functional explanation in general. In a direct attack on functionalist explanation in sociology, he argues that these explanations fail because they rest upon an ill-conceived analogy from biology (a meritorious criticism) and that resulting social explanations

almost always lack one or more of the defining features of functional explanation (1979:28-35). He attacks the functionalist theory of Robert Merton as well as Merton's specific discussion of latent functions (1956). Elster redefines Merton's concept by adding the necessary condition that the "unrecognized" and "unintended" benefits involved in *all* latent functions be maintained "by a causal *feedback* loop passing through (group) Z" (pp. 28-29).

This particular criticism is troublesome in that concept of a feedback loop is unnecessarily complex. All that is needed to demonstrate the creation of a survival advantage appropriate for Merton's examples is to show that A causes B and that as a result, B causes A, which in turn results in A causing B. While this may also occur within a system that makes use of a feedback loop, it can occur in the more direct causal chain outlined above. Elster does not argue for why a feedback loop should be a necessary condition and the existence of an alternate direct causal process demonstrates that it is not a necessary condition.

Four years after the above criticism, Elster again argued that most functionalist social science is unconfirmed because functionalists do not cite the mechanism connecting beneficial practices with their persistence (Elster 1983). And, five years thereafter, Elster expanded the scope of this criticism by asserting that in giving causal explanations, "To cite the cause is not enough: the causal mechanism *must* also be provided, or *at least suggested*" (1989:4). While "must provide" and "at least suggest" are extremely different standards, they both are supposed to be obligatory standards. However, the major problem with this overstatement is that regardless of which of the two standards Elster intends to impose upon the field of causal explanation, it is

extremely difficult to assess the level of detail required in the specification of the causal mechanism.

Ideally, the goal of causal explanation should be to explicate as much explanatory information as is possible, including underlying mechanisms. However, while providing or suggesting mechanisms is obviously one method of facilitating understanding, Elster makes no argument for why it should be the exclusive method. Given Elster's fascination with necessary conditions as well as his descriptive emphasis on "nuts and bolts, cogs and wheels" (1989:3), one might suspect that the core metaphor behind Elster's view of the social sciences is a *mechanistic* one.

I would want to suggest a much "messier" view of the social sciences. In a world of conflicting individual and group meanings where complex institutions and intricate rituals are created over extended periods of time by multiple causal agents, the explanation of different aspects of changing social behavior will often vary depending upon the focus of our particular question. Given that, the best we may be able to provide in the social sciences are incomplete models of social functions that illuminate the roles and capacities of those institutions and rituals. In such a world, explanations created by following lists of necessary and sufficient conditions will certainly be the exception instead of the rule.

Section Four

False dichotomies

In making the argument for the role of functional explanation within the social sciences, it is inevitable that I do so in relation to some of the existing philosophic

debates concerning explanation within the social sciences. It is not my intent to seriously enter these debates, for to do so would take me too far from my stated task. However, some of the sweeping claims in these debates are hard to sidestep. Often these discussions are set up as contests between the two outermost and opposite views of a continuum of possible views. Thus, it is often implied that explanation within the social sciences must either take the form of deduction from naturalistic laws, as in physics, or be based in some antinaturalistic concept of meaning. Or, we are offered the choice between methodological individualism on the one hand and holism on the other. As one commentator has observed, such posturing "between interpretational social science and naturalistic social science reflects the same issues that were debated among Weber and Durkheim, Dilthey and Comte, Mill and Marx, Hegel and Hobbes" (Rosenberg 1988:205).

While these debates are convenient pedagogical tools for understanding the history behind these classical but often immoderate views, what is lost is a serious engagement with the more realistic middle ground, the continuum of positions that fall between the polarized extremes. For instance, one such debate concerns the unity-of-science argument that the same reductive process unifies all sciences and therefore sociological systems can be reduced to psychological systems that ultimately are reduced to physical systems. Juxtaposed to that view is the Durkheimian view that all sociological facts are holistic and collective and thus are not reducible to psychological processes.

Both positions are extreme. Moderate middle ground views need not adopt *either* of these views as classically presented. Both polar positions are too strong in that: (1)

even if sociology were (ontologically) reducible to psychology and then ultimately to physics (a position they need not embrace), there would still be important sociological generalizations that were not available to us (epistemologically) at a reductive level, and (2) as one commentator has noted, the Durkheimian concept of a *societal* motive also seems to involve reification, a sort of reductionism in reverse (Abrahamson 1978:39).

Too often, proponents of one of the polar positions overlook these less ostentatious intermediate positions and an argument is presented which contains a disjunctive premise which asserts that only the extreme options are possible. Such arguments tend to show either: (1) if one of the choices is eliminated, then the other will be true by *necessity*, or (2) that both alternatives are false and therefore there can be no scientific explanation within the social sciences. These arguments, thus, present a false dichotomy.

Such arguments also tend to suggest that explanation must be complete. As I have been arguing for a view of explanation as partial, causal and cumulative, I will pause to address two examples of such argument.

1. **Rosenberg's rendition of the Darwin or Durkheim argument.** A classic argument often presented by methodological individualists takes as a premise that a social account of functions must either be grounded in a group consciousness or in a Darwinian evolutionary view, "according to which all long-lasting social institutions arose through variation and selection for their beneficial functions" (Rosenberg 1988:134). The idea is that functions must either be intentional (requiring a mind), and thus teleological, or natural (requiring a selection process), and thus non-teleological. I

want to argue that this rendition overstates this limitation upon the available alternative selection mechanisms.

I have been arguing that what is important about natural selection in the biological account is the work it performs. It performs that work for biology because it concerns biological phenomena and their survival *over time* as a result of a nonteleological, causal selection process. Natural selection is able to ground biological proper functions in a causal and nonteleological manner because of:

1. the random process of creating survival candidates,
2. the causal nature of the selection process at producing survivors, and
3. the absence of a goal-directedness in the selection process.

This work performed by natural selection is what makes a functional explanation in biology nonteleological.

A theory concerned with social phenomena that performed the same work by explicating the survival/persistence connection in a non-teleological, causal manner would be quite adequate for social explanation. If a functional explanation in the social sciences is grounded in a similar non-teleological manner, it need not embrace a social version of a Darwinian evolutionary view. In other words, a differential sorting process capable of undergirding satisfactory and non-teleological anthropological or sociological functional explanations must perform the same type of explanatory work that Darwinian evolutionary theory does for functional explanations in biology. However, it need not be limited to genetic Darwinian evolution, since such an account involves an improbable *social* selection mechanism and since alternative social mechanisms that make the contribution-to-survival/persistence-over-time connection are available.

Likewise, an etiological account of social functions need not be grounded in a Durkheimian group consciousness, at least not in the sense of some sort of group intentional goal-directedness. As is suggested by the biological account, the absence of goal-directedness in the selection process is one of the reasons for the success of that account. This is so because that absence of intentionality guarantees that the resulting explanation does not ascribe present consequences to future causes. Again, making the contribution-to-survival/persistence-over-time connection accomplishes this without resorting to group intentional goal-directedness.

2. Pettit's argument for intentionality or a mechanism akin to natural selection. In a recent and engaging article, Philip Pettit (1996) suggests that functional explanation in the social sciences ought to be talking about the *resilience* of social phenomena (e.g., customs, institutions, traits, and behaviors) instead of trying to explain the presence or emergence of them. Pettit's expressed reason for developing this explanatory view is his suggestion that while the explanation of presence or emergence would be subject to Elster's missing mechanism criticism, an explanation of resilience would not. He illustrates this idea with the following example:

Suppose that golf clubs are functional in enabling business people, bankers, and various professionals like lawyers and accountants to get to know one another, establish networks, and reinforce their mutual confidence. The functionality of such clubs in this respect might make them very resilient features of our sort of society: and this, even if the resilience had never actually been put to the test. For it is transparent that were such clubs to come under various pressures – still they might be expected to survive; we might not find people leaving the clubs in the numbers that such pressures would normally predict. The members of the clubs would be forced to reconsider their membership in the event of this sort of pressure but that very act off reconsideration would make the functionality of the club visible to them and would reinforce their loyalty, not undermine it (p. 296).

What Pettit seems to suggest is that *no* selection process is at work in the golf club example until some external pressure challenges the club's continuation. Then, and

apparently only then, a selection process comes into effect when the members reconsider their membership and in so doing the functionality of the club becomes *visible* to them. Thereafter, they make some type of *intentional* selection concerning retaining their membership, which allows the continuation of the club, as well as reflecting the fact that the functionality of the club had provided a survival advantage for the club. Pettit sees this survival advantage as a propensity to be resilient – a propensity that is present even if it is untested.¹

Pettit suggests that such an explanatory of resilience would not be subject to Elster's missing mechanism criticism. I have previously suggested that the scope of Elster's missing mechanism criticism is less than clear, even in Elster's own language which vacillates from *requiring* the explication of a mechanism at the *individualist* level, to noting that good social explanation should *suggest* the presence of a mechanism. Pettit's language is not so unclear. He clearly expresses the view that explicating the underlying mechanism is a *necessary* condition for functional explanation in the social sciences. Pettit argues that, (1) either people in the past created their institutions by intentional design, *or*, (2) the only mechanism available to underpin functional accounts is a mechanism of selection akin to that which is invoked in biology (Pettit 1996:293). Pettit explicitly rejects intentional institutional design, thereby leaving the alternative (a mechanism akin to natural selection).

The disjunctive premise in Pettit's argument creates a false dichotomy. Why should these two alternatives be the only available mechanisms to underpin functional explanation? At the very least, the burden of sketching an argument that would support

such a claim lies with those who would assert such a restricted set of alternatives. What would such an argument look like? Pettit does not provide any suggestion.

As I have previously argued, a mechanism akin to natural selection cannot underlie social explanation. While social mechanisms will belong to a *set* of selection mechanisms that includes natural selection, they will not be “akin” to natural selection in a substantive Darwinian manner. They will not require genetically coded transmission, they need not follow genealogical lines, nor will the operational speed of these selection mechanisms be similar to those of natural selection. Likewise, as I set out in detail hereafter, intentional design cannot be the mechanism that underlies functional explanation in the social sciences. In short, Pettit offers only unrealistic alternatives as mechanisms to support social explanation. Amidst this suggested theoretical vacuum, Pettit proffers his theory of *resilience*. The lack of viability of these alternatives as well as the existence of other mechanisms undermines his conclusion that there cannot be successful functional explanation in the social sciences. (For an extended discussion of why such a limitation cannot be the case, see Kincaid 1996: 136-141; 179-182).

Section Five

Intentional human design and social proper functions

A major difference between functional explanation within biology versus that within the social sciences is the importance of the element of intentional human action in the latter. In the social sciences an item can have a function as a result of intentional human design. As such, items normally have intended uses, and in the cases where that is so, these intended uses could be ascribed as their functions. This raises the question of

whether intentional human design is an underlying mechanism through which functional traits may arise that can contribute to survival, and therefore be maintained and persist over time. That is what would be required for such a mechanism to perform the same sort of work for a social theory that is performed in biology almost exclusively by natural selection.

Part of the record of the functional debate within biology is the exchange between Paul Griffiths and Karen Neander concerning the possibility of using intentional human design as a ground for a theory of artifact functions. I want to broaden the scope of that discussion to include objects of human design, be they artifacts, inventions, rituals, institutions, etc. Griffiths suggests that one could argue that human selection impacts the selection of artifacts by one of three methods: (1) the conscious design decisions among hypothetically envisioned alternate designs of these artifacts, (2) trial and error which provided actual alternative survival candidates for this selection process, or (3) some combination of these two (1993:418-422).

However, consider the following three observations about the hypothesis that intentional human design could perform the same type of principled grounding for an account of social proper functions as natural selection does for biological proper functions:

1. We can immediately note that at least the initial creation of the functional object is to accomplish some goal of the designer (contrary to what happens in biology). If the functional object persists (is reproduced over time) in order to accomplish the same goal as led to its initial creation, then the goal-directedness of the function seems to continue into the future.

2. We can also note that intentional human design need not create categories at the level of types, in fact, they may create idiosyncratic cases without category. This is illustrated by Neander's example of the unique additions to James Bond's brief case. It may be said that these additions have proper functions peculiar to them because that is what they were individually selected for (Neander 1991b:462).

3. Lastly, as Neander also points out, we can imagine that some inventions are created because they are the logical answer to a problem which was envisioned correctly the first time, without design alternatives being contemplated or created. In such a scenario, there is no field of survival candidates, much less a selection among them (Neander, *ibid*).

These three observations suggest that intentional human design may on occasion:

1. employ goal-directedness,
2. create proper functions at the level of tokens, and
3. make "selections" without alternative candidates.

This seems to indicate that a very different selection process is at work in our discussion of human selection than is involved when natural selection is at work. If we rely on the lessons learned from biology, the above points should alert us to the fact that intentional human design may differ in that:

1. its functional mechanism may be teleological (involve real intentionality),
2. its functional categories may not be at the level of types, and
3. it may not employ an actual "selection mechanism" in all cases since in some cases there may not be a field of alternate candidates.

In short, intentional human design does not seem to guarantee the very things that made functional explanation based on natural selection successful in biology.

That is not to say that many designer-intended uses of items when ascribed as functions may seem to be confirmed by this form of human selection. However, that does not mean that those ascriptions will be social proper functions. The explanation of their selection may be teleological, they may not create functional categories or the selection process involved may not be both random and causal. Additionally, there are innumerable cases that cannot be covered by an account of intentional design – intentional explanations just don't cover enough – so, for those cases a non-intentional account is needed.

Obviously intentional human design occurs over considerably less time than does natural selection. However, an account of social traits and institutions that implements social proper functions would need to refer to the operation of the mechanism over some extended period of time in order to make the case that its benefits causes its persistence. Such a “survival” process occurring over time would require the selection of successor components, i.e., new artifacts to replace broken old ones, a subsequent version of a ritual occurring in a prior year, or a similar but altered institution that reflects change to or within the system, etc. Such successor components could be organized within categories of type, i.e., the type of function that each component is supposed to perform, given normal conditions. Thus, one advantage of looking at intentional human design as a *mechanism operating over time* would be the ability to employ the use of functional categories in a manner similar to that within biology.

There are other benefits that may be produced by concentrating on a survival mechanism that operates over time. Survival may occur due to a consequence that is different from the one that caused its initial selection, i.e., it survives due to subsequent intentional human design for reasons initially not intended. When this occurs, there is a

separation between the initial goal-directed selection and the subsequent selections that are causally responsible for the survival of the component over time. This prevents a functional explanation of the persistence of the component from being truly teleological. Additionally, because the initial and subsequent selections are *for* different effects, those effects constitute a field of at least two alternate survival candidates upon which the mechanism operated.

Thus, in cases where intentional human design is only contemplated as a selection mechanism for proper social functions when it occurs over an extended period of time, it appears to operate in a manner consistent with the lessons derived from the use of functional theory within biology. This seems appropriate. For, to suggest that persistence over time occurred as a result of human selection for reasons different than an object's initial intended use is only to suggest that there is a causal story of this survival which is not goal-directed. Thus, "human selection" can be seen as a source of variation upon which *social selection* operates.

Section Six

Latent Functions

As the previous section suggests, one method of individuating social proper functions involves identifying social mechanisms that survive over time due to their selection for consequences different than those that gave rise to them initially. The idea is to separate the selection process from the goal-directedness of human wants and desires through which they initiated; it "uncouples" the process of the creation of survival candidates from the process of selection among them (to use Cohen's term).

One of the major differences between a theory of social functions and a theory of biological functions is the fact that human participants can articulate what they believe to be the function of the social activities in which they participate. However, social scientists often suggest other, deeper or latent functions that the activity performs. As Durkheim observed, "it is a fertile idea that social life must be explained, not by the conception of it created by those who participate in it, but by profound causes which escape awareness . . ." (Durkheim 1978:127).

This distinction performs important work for functional explanation by dividing the category of human selection into more workable subcategories. It does this by distinguishing between "manifest" functions, which most often deal with intended consequences, and "latent" functions, which normally deal with unintended consequences. What is significant about this separation of intentional from unintentional consequences is the fact that it situates intentionality and teleology in the category of manifest functions and creates the separate category of latent functions which excludes intentionality and teleology.

It seems appropriate to begin a discussion about Manifest and Latent Functions with Merton's insightful work by the same title (Merton 1957:60-84). Merton's method of analysis was to compare whether the purpose of the behavior (the intended consequences) and the actual consequences coincide. If they do not, then one should look to see if there are collateral consequences which occurred at the group level which form a plausible hypothesis about why this activity persisted over time while not accomplishing its intended consequences.

Merton argued that such an analysis, employing the Manifest/Latent function distinction clarified the role of seemingly irrational social patterns, such as the Hopi rain

dance (his example). Since the manifest function of the rain dance was to produce rain, the attainment of the avowed purpose was an empirical question, one answerable by a meteorologist. As the actual consequences (meteorological evidence) did not support the purposed behavior, Merton suggested the existence of collateral consequences at the group level that could better explain the persistence of this activity. This approach led Merton to hypothesize that the rain dance has the latent function of reinforcing the group identity through ritual performance, whereby the members of the group could engage in collective expression of sentiments found basic to group unity during times of distress. This explains the survival of a practice that fails miserably to fulfill its manifest function (see Salmon 1988:28).

Exactly what lies behind functional explanations involving latent functions? What is it that a scientist like Merton is actually saying when he describes something as a "latent function"? I want to suggest that to call a trait or behavior "a latent function" is a sort of promissory note, a shorthand description that is to be redeemed as follows:

1. the participants in A (the thing or event-to-be-explained) believe that A can help to cause C, and,
2. the participants in A believe that they participate in A in order to attempt to produce C,
3. A does not in fact help to cause C,
4. A causes B,
5. the effect of B is an unintended benefit to those who participate in A,
6. this unintended benefit causes more of the participants in A (or in subsequent copies of A) to flourish, and to reselect A (or copies of A) more often than they

would have if A had not caused B, even though the participants may not be consciously aware of receiving those benefits, and

7. this activity helps A (or subsequent copies of A) to persist over time.

In Merton's example the attribute of ritual rain dancing was hypothesized to have the latent function of promoting social cohesiveness in times of distress. Another example of seeking deep unintentional explanations of human social practices is the kind of explanations offered by Marxist theories. Society is viewed as a system composed of classes competing for supremacy. Social institutions are analyzed in terms of their functions in fulfilling the needs of the competing classes. The important point here is that such examples illustrate how the event-to-be-explained in these social sciences is taken to have a latent, as opposed to a manifest, function. Yet, the attribute involved in the functional explanation arises historically out of its manifest function – rain dancing arises as a ritual with the manifest function of (something like) appeasing the forces of nature in order to produce rain. Social institutions, e.g., the church, arise to fill manifest desires of individuals, e.g., spiritual salvation.

If we attempt to apply Wright's schematic formulation (F) for parsing functional ascriptions to Merton's rain dance example we can better see how the classic teleological criticism conflates the roles of manifest and latent functions:

The function of A (rain dancing) is B (social cohesiveness) iff:

- (i) B (social cohesiveness) is a consequence (result) of A's (rain dancing) being there (this is precisely Merton's claim),
- (ii) A (rain dancing) is there because it does (results in) B (social cohesiveness).

The above ascription *is teleological*. Rain dancing is connected to the result it produces in the future, i.e., social cohesiveness. Note however, that this is not an explanation. (F) provides only a partial analysis of the ascription of a function, it does not fully cash out the ingredients required by a social functional explanation. This is so because we have not yet attached a consequence-etiology to the ascription of the function.

Any adequate consequence-etiology of A (rain dancing) will tell us that A originally arose as a result of C (appeasing nature in order to produce rain). Thus, A is present in (ii) above because it originated with the manifest function of appeasing nature in order to produce rain. To obtain an etiological functional explanation (ascription + consequence-etiology), we need to slightly expand Wright's original formulation (F) and in fairness to Wright give it a different designation, say (FE). I suggest that (FE) should look something like the following:

The functional explanation of A (rain dancing) is B (social cohesiveness) iff:

- (i) A (rain dancing) was there originally because it was believed to produce (result in) C (appeasing nature in order to produce rain),
- (ii) B (social cohesiveness) is a consequence (result) of A's (rain dancing) being there,
- (iii) A (rain dancing) is still there today because it does (results in) B (social cohesiveness).

The point of this discussion is that the explanation for our example about rain dancing is not teleological when one considers that a proper consequence-etiology of the attribute of rain dancing uncouples a claim that the explanation is teleological. This is because the

uncoupling exposes the fact that the social practice developed as a result of past group beliefs (manifest function) not as a result of fulfilling future group needs (latent function).

In spelling out (FE), we listed the historical origin of the practice (manifest function) in (i), listed the present-day consequence of the social practice (latent function) in (ii), and tied the two together in (iii) by citing the case of the practice's persistence over time. The success of such an explanation is due to its ability to provide an historical, causal answer to why the item persisted to the present by evolving from its initial, manifest design. As Hegel says, "We are what we have become, and the act of our becoming is our history."

Thus, functional explanations of social practices (as in the example of ritual rain dancing) provide causal and nonteleological explanations because, although the attribute arose as a result of manifest functions, it survived historically because of latent functions that "selected" it. It bears repeating that fulfilling these deeper needs was not among the original goals when the practice was implemented. This "survival" process was an unintended causal process, and, therefore, a nonteleological process.

The historical change of a social practice from its manifest function to an unintended latent function performs much of the same work of causally and naturalistically grounding anthropological and sociological explanations as Darwin's evolutionary theory does for functional explanations in biology. This is due to two extremely important elements. First, the classic teleological criticism is laid to rest. This is illustrated by returning to Cohen's description of the processes of mutation and selection in organic evolution as "uncoupled". Cohen's terminology is helpful in understanding the nature of the explanatory work performed by natural selection for functional explanations in biology. There, the underlying mechanism provides the

"uncoupling", the mechanism separates the causal history of the random creation of alternate survival candidates at an initial time from the causal history of the selection among those candidates at subsequent time. This "uncoupling" refutes the claim that functional explanations are not legitimate explanations because they are teleological. Recall the example of the long necks of giraffes. The criticism there against teleological explanation was that functional types could not be causal in such explanations. For, a future event (eating from tall trees) could not explain a past event (developing a long neck). By "uncoupling" creation at an initial time from selection at a subsequent time, the teleology in the explanation is dissolved.

Second, the relevant causal process occurs over an extended period of time, thereby requiring subsequent reselections during this period of time. Note that this means that the individual tokens selected during this period can easily be seen as members of a type. This same process of creating functional types is necessary for the non-teleological notion of function to provide the "conceptual glue of biology."

This second point can be illustrated by the Hopi rain dance example, where the selection process occurs over time thereby requiring annual reproductions of the original rain dance that requires annual reselection of the ritual because of its normal consequences. There is no necessary requirement that the contribution to social cohesiveness occur every year; perhaps a dance fails this purpose on occasion and something other than solidarity occurs between the members of the group. What is important are the benefits that the properly functioning trait normally bestows – the creation of social cohesiveness – and the fact that this consequence causes its selection. Thus, this selection process picks out proper functions which in turn results in the creation of functional categories – categories that are

created based upon what the component of that system normally does, regardless of whether or not a particular token actually performs that function.² Individual Hopi rain dances are members of a class of Hopi ritual dances that meet the historical identity conditions for that type-classification.

Section Seven

Latent Functions as Social Selection Mechanisms

The previous Hopi rain dance discussion suggests that a differential sorting process for a trait is at work and that the relevant functional explanation explicates the connection between the contribution-to-survival made by the consequence of the trait and its effect upon the persistence-over-time of the system to which it contributes. I suggested earlier that I would demonstrate a methodology capable of assisting in "excavating" the underlying causal mechanisms at work in functional explanations. It is worth re-emphasizing that doing so does nothing more than make more perspicuous the details of the contribution-to-survival/persistence-over-time connection.

In Chapter Three I discussed at length the interwoven nature of etiological and dispositional accounts. Those lessons are equally important for functional explanation in the social science. Both types of information are part of the intuitive sense we invoke when we employ the use of functional talk. In my view that involves answering both of these questions: "What is the work being performed by the component now?"; "How has that work contributed to the presence of the component?" A Cummins-analysis explains what an item is by describing its present capacity to perform a particular role in a sociological system. It can do that work as adequately for social explanation as for biological

explanation. A modified version of a Cummins-analysis contributes to the explication of such functional explanations by showing how social scientists may explain the presence of an item by describing the history of how the function of the item contributed to the persistence of that item over time.

Earlier, I applied a modified Cummins-analysis to Mitchell's example of a functional explanation concerning a biological component, large size in male primates in a specified environment. There Cummin's filling instructions were modified to include instructions that would create a biological proper function as part of that analytical exercise. Here, I want to do the same thing to Merton's example of the Hopi rain dance. These modified filling instructions should create a social proper function that allows this explanatory schema to provide both dispositional and etiological information for this sociological example.

Recall that the modified Cummins-analysis that we employed in Chapter Three in relation to biological examples was in the following form:

The function of x in s is to phi just when:

1. x is capable (under normal circumstances, when x is not defective in s) of phiing in s,
2. s is capable (under normal circumstances, when s is not defective) of psiing,
3. the propensity (capacity) of s to psi is increased (supported) by the propensity (capacity) of x to phi in s, and
4. the propensity (increased capacity) of s to psi ultimately contributes to the presence (persistence through time) of x

This modified Cummins-analysis needs *filling instructions* appropriate for replacing the dummy letters of each schematic sentence as did the one used in Chapter Three. However, there the subject matter was biology and here the subject matter is the social sciences. The

following filling instructions should be applied to the modified Cummins-analysis when it is applied to the analysis of biological systems, at least when natural selection is the appropriate selection mechanism:

s is a particular culture or subsystem of that culture

x is a trait of behavior or social custom

phi is the social effect of *x* in society or culture

psi is the consequence of this effect on the culture over time

In accordance with those filling instructions I have assigned the following meanings to Cummin's variables in accordance with Merton's claims:

x = rain dance(*s*)
s = Hopi culture
phi = produce cultural solidarity
psi = retain cultural identity over time

This produces the following:

The function of rain dances in Hopi culture are to produce cultural solidarity (relative to analytical account A) just in case:

1. rain dances are capable (under normal circumstances, when rain dances are not defective in Hopi culture) of producing cultural solidarity in Hopi culture,
2. Hopi culture is capable (under normal circumstances, when Hopi culture is not defective) of retaining cultural identity over time,
3. the propensity (capacity) of Hopi culture to retain cultural identity over time is increased (supported) by the propensity (capacity) of rain dances to produce cultural solidarity in Hopi culture, and
4. the propensity (increased capacity) of Hopi culture to retain cultural identity over time ultimately contributes to the presence (persistence through time) of rain dances,

as specified in analytical account A which incorporates cultural environment E.

An important aspect of a modified Cummin's-analysis is its ability to outline the nature of the contribution-to-survival/persistence-over-time connection. Naturally, this type of analysis can be "fleshed out" in an analytical account A which incorporates environment E. Confirming such empirical detail is a normal part of the process of confirming the correctness of an analysis. In this anthropological example, a more empirically detailed explanation might look something like the following:

Hopi culture includes a system of calendared annual ritual events, one or more of which is a rain dance. Members participate in these events which occur on preset dates that approximately coincide with important events for an agrarian society such as planting seeds, harvesting crops or properly acknowledging the hoped for brief but vital rainy season in this arid climate. The members participate in ritualized pre-event conduct such as extended fasting in kivas (ritual centers) for dancers, periods of baking by other participants for a celebration that may follow, and/or painting of kiva mural walls for kiva ceremonies restricted to culture members. During such pre-event participation as well as during participation in the ritual event itself, the participants renew contact with long-held common beliefs about the ontological nature of the Hopi world and the relationship of the members of the Hopi culture with that world. This system of annual ritual events thus has the capacity to allow members to renew their awareness of their cultural beliefs as well as their cultural bonds with each other. The system has the capacity for allowing Hopi culture to retain cultural identity over time. Individual events within the system have the capacity to produce cultural (social) solidarity.

The length of this account is a result of modifying the filling instructions to require the addition of a causal history of the system and the function of its components. Alternatively, such a modification might require an even more extended account. How detailed? Arguably it could also require the following:

A causal story of the development of corn in Central Mexico and its introduction into the Southwest around 800 A.D. by traders from Northern Mexico. The then current technology on growing corn would arguably have also been introduced, i.e., an assemblage of rain gods, the use of pathos (prayer feathers) in planting, as well as the concept of annual calendared

ritual events. The next 7-800 years saw the development of ritual centers, a calendar of ritual events, masked kachina dances appearing within the ritual centers, and prolific kiva wall mural painting. Beginning with the pressure of cultural conversion from Spanish priests, up to and including the time that role was assumed by the U.S. Bureau of Indian Affairs, the causal role of the ritual events probably became more defensive in nature, i.e., protecting the cultural integrity of the Hopi from cultural intrusion. That these ritual practices are still alive today speaks to their effectiveness in producing cultural solidarity and in retaining the cultural identity of the Hopi over time.

The above incorporates the requirements imposed by the concept of social proper functions in that the original ritual concepts were changed over time, presenting multiple survival candidates. Originally, they were created over a period of time for different but specific purposes. Later, when outside cultural pressures mounted, a selection process occurred which was unrelated to these original purposes and which created a fairly defined ritual system with well-defined individual component events. This separation of the random creation of survival candidates from the eventual selection process prevents the explanation from being teleological. But, most importantly, this account begins to spell out the underlying causal mechanism at work and incorporates a recital of sufficient facts to demonstrate its operation in the particular case being explained. When that can be done, explanatory power is gained.

I have attempted to defend Merton's original work against Elster's criticism that Merton's explanation was fatally flawed. In defense of Merton, his work suggested a methodology for forming a plausible hypothesis about why an activity persisted over time while not accomplishing its intended consequences. Thus, Elster's criticism should be seen as claiming that the ability to confirm such a plausible hypothesis was undermined by the failure to cite any underlying causal mechanism. I have tried to cash out functional

explanations in causal terms to show how functional explanation in the social sciences may be understood as ultimately non-teleological and causal, as it is in biology.

Section Eight

Summary

In Chapter Two I discussed a number of meritorious criticisms that have been directed against the social doctrine of functionalism. At the heart of many of those criticisms were one or more of the following:

1. the inappropriate application of the organic metaphor,
2. the problems associated with the organization of the parts of society in a manner similar to the human body without concern for analyzing the causal roles played by those components within the greater social system,
3. the problems associated with viewing the social system as a homeostatic process with a built-in cybernetic mechanism,
4. the tendency of functionalist theory to reify the status quo and therefore to be unable to handle the concept of change over time,
5. the problem of making certain universal assumptions employed in creating a functionalist model, i.e.,
 - a. the assumption that standard social activities or cultural items serve a function for the entire social or cultural system,
 - b. the assumption that every type of civilization, every custom, material object, idea and belief fulfills some vital function,

c. the assumption that there are certain indispensable functions without which society will not persist and that certain cultural or social forms are indispensable for fulfilling these functions.

The first obvious step for a social doctrine of functional explanation is to do away with the inappropriate organic analogy. The only substantive work still performed by this analogy is that it creates organizational categories in a manner analogous to the parts of the body and then uses those for classifying regularities. However, such a categorization process is not causal and is of questionable utility, especially since it abandons concern about the causal roles played by the components of the system. The lesson from the success of functional explanation within biology is clear. At the center of meaningful functional theory is the creation of functional categories, categories that are generated by the notion of what like components of a system are supposed to do. Explicating the contribution-to-survival/persistence-over-time connection that establishes the historical identity conditions for type-classification does this.

The criticism concerning the imprecise nature of social systems boundaries as compared to the precise nature of the boundaries employed within an *organic* model seems right. Likewise, the criticism that functionalist theory lacked a basis for assuming a built-in *mechanistic* homeostatic cybernetic mechanism in social systems seems correct. The use of both of these conflicting, comparative metaphors brought with them their own explanatory problems, often much greater than the utility of the analogical illumination which fostered their use. Functionalism became a classic illustration of the problems that can arise when analogy is used as the basis of an explanatory theory. The lesson for the social sciences is

that the major focus of inquiry should be upon the causal roles of social phenomena, not upon pre-existing assumptions created by analogical extension to an auxiliary subject.

Additional classic examples of this perceptual problem are the universal postulates that tended to reify the status quo thereby making functionalist theory unable to handle the concept of change over time. These universal assumptions should be abandoned. These *assumptions about functions* should be replaced by the *analysis of functions*. The lesson from biology is that the clarity produced by the initial use of a Cummins-analysis is helpful in addressing *what* an item is by describing its present capacity to perform a particular role in a particular system. After initially answering this descriptive *what* question, a more complete explanation is sought by addressing the question of *why* an item is present. This is accomplished by describing the history of how the function of the item contributed to the persistence of that item, and to the system of which it is a component. This is a lesson easily implemented within the social sciences. The use of a modified Cummins-analysis directs attention toward explicating, to the extent possible, the differential sorting processes that undergird this type of functional explanation in the social sciences.

Functionalist theory has long been criticized for its consistent inability to explain change over time. Why that is not a problem for functional explanation can be easily demonstrated. Recall that in Chapter Three, while arguing that dispositional and etiological accounts were not inconsistent, I illustrated the point by suggesting that one could visualize dispositional analyses as descriptive explanations within time slices. With that in mind, I further suggested that etiological analyses could be seen as a collection of descriptive explanations from a continuous sequence of those time slices. Attention to this chain of

explanations allows social scientists to study the changes that have occurred over time in both the component and the system.

The dual elements of this explanatory power should be familiar to those within the social sciences. The first is related to the concept of *social structure*, i.e., the nature of the component, the nature of the system, and the contribution-to-survival created by the function of the component within the system. The second is based upon *historicity*, i.e., persistence-over-time. Social explanations that combine these two elements can successfully make the contribution-to-survival/persistence-over-time connection, or, alternatively, they can show the changes which did not allow the component to persist over time.³ In doing either, they solve the problem of explaining change over time that has plagued the social sciences for over a century.

With these lessons in mind, I have argued that the second step toward a viable concept of functional explanation for the social sciences begins by rethinking the roles played by paradigms in the social sciences. I have suggested that the search for explanation in anthropology and sociology should focus upon the causal roles of events, behaviors and social institutions. The goal of such resultant explanations will therefore be to provide causal information about these phenomena. When this explanatory information is pragmatically viewed as part of a larger and more complete ideal explanatory text, it can be understood more realistically as a partial explanation. Additional partial texts can be added to such information to create cumulative, more complete explanations. Under such a view, information produced by different explanatory models can often be combined. Paradigms need not compete for explanatory exclusivity. I have argued that implementation of such an approach within the social sciences can create a viable concept of explanation in general and

of functional explanation in particular – explanation which should be seen as causal, partial and cumulative.

¹ Pettit's description of his concept of resilience is much like Bigelow and Pargetter's ahistorical concept of propensities, a similarity Pettit acknowledges (p.295). A golf membership club has a certain fitness (read propensity) to survive in our society. Pettit implies that the propensity lies inactive or undiscovered until external pressures force members to rethink the benefits provided by the membership (read function) and such reconsideration makes the advantages *visible* to them, which allows them to initiate a selection process by electing to retain their membership.

² It is in precisely this fashion that the implementation of the concept of social proper functions creates categories based upon what I want to call "sociological normality" as a social facsimile of the concept of "biological normality" that underlies the creation of functional categories there.

An interesting question for a social account of functional explanation is whether such an account can retain the flexibility associated with the concept of biological normality and its related functional categories. Recall that one of the unique qualities about biological categories is the fact that these categories do not have sets of necessary and sufficient properties. In biology this is because traits and groups are both variable and evolving. However, in the social sciences, both traits and groups seem similarly variable and evolving. Thus, instead of necessary and sufficient properties, it would appear that a social account could have categories that contain members who meet the historical identity conditions for type-classification.

³ In a similar vein is the argument within biology of Paul Griffiths, who argues that adaptive generalizations cannot explain form (structure) except in conjunction with a rich set of historical initial conditions. He argues that this is so because of the fact that the relative fitness of characters is a function of the historical conditions in which selection takes place and of the complete range of alternative characters present (Griffiths 1996:515). Griffiths goes on to note that the use of the concept of 'historicity' within biology is seen by some as one of the three guiding insights of modern biology, along with 'mechanism' and 'natural selection' (citing Williams 1992).

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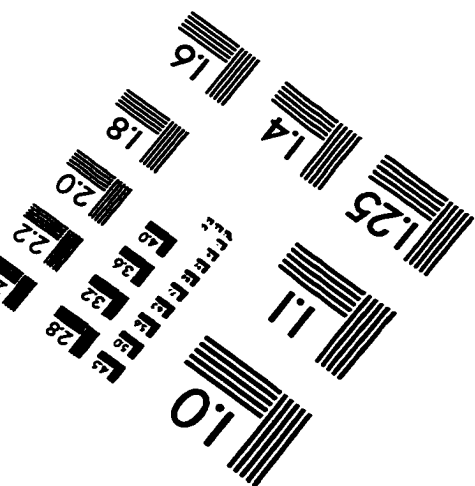
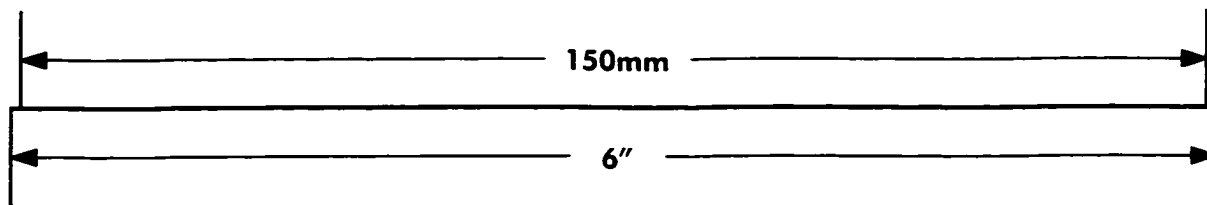
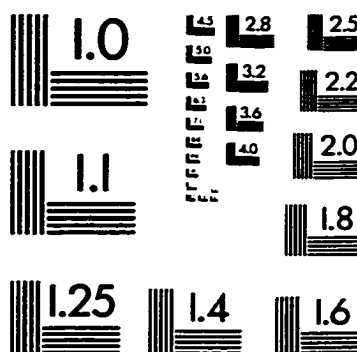
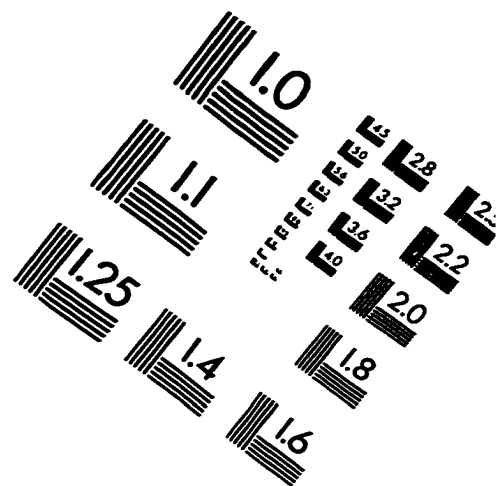
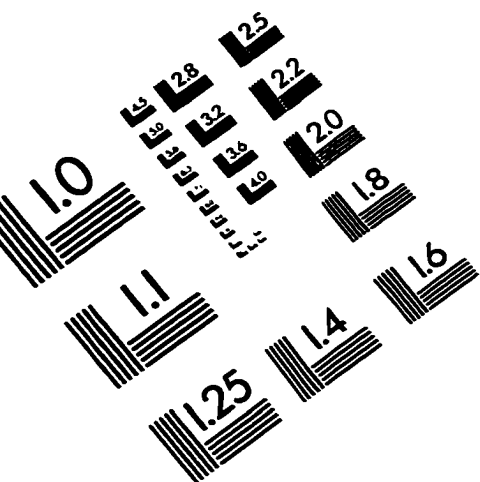
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