5-2013

# Causal Explanation of Human Behavior in the Social Sciences

Maria R. Zavada
*University of Nebraska-Lincoln*, mrzavada@yahoo.com

# CAUSAL EXPLANATION OF HUMAN BEHAVIOR IN THE SOCIAL SCIENCES

by

Maria R. Zavada

A DISSERTATION

Presented to the Faculty of

The Graduate College at the University of Nebraska

In Partial Fulfillment of Requirements

For the Degree of Doctor of Philosophy

Major: Philosophy

Under the Supervision of Professor Jennifer McKitrick

Lincoln, Nebraska

May, 2013

# CAUSAL EXPLANATION OF HUMAN BEHAVIOR IN THE SOCIAL SCIENCES

Maria R. Zavada, Ph.D

University of Nebraska, 2013

Adviser: Jennifer McKitrick

The social sciences have something to offer our understanding of human behavior. However, the social sciences have been subjected to a great deal of criticism, both internally and externally. Cultural anthropology provides a microcosm of the problems within the social sciences and serves as an apt case study. There are many problems with the social sciences, some as fundamental as whether or not the social sciences are indeed sciences, and others that address specific issues with goals, methods, and data collection.

Using anthropology as a case study, I articulate the connection between the methodological problems in anthropology and the philosophical problems that underlie them. I argue first that the most basic goal of anthropology, understanding human behavior in a cultural context, is a legitimate goal. Second, I argue that a radically skeptical epistemology, like postmodernism, cannot be the basis for the theory and method in anthropology or any other social science. Third, I argue that anthropology cannot be replaced by sociobiology or evolutionary psychology. Fourth, I argue that the root of the disagreement about the best methods to use in anthropology stem from faulty assumptions about reductionism and implicit assumptions about the causal relevance of intentional states. Finally, I argue that the challenges to the scientific status of explanations in the social sciences are the result of philosophical disagreements about the causal relevance of intentional states. I argue that these disagreements can be resolved in

a way that provides anthropologists, and other social scientists, with a path to a coherent scientific theory of explanation.

# ACKNOWLEDGMENTS

I would like to thank my husband, Rodney Cupp, and my family for their patience and support as I completed my dissertation. I could not have completed this project were it not for the help and encouragement of my advisor Jennifer McKitrick. Thanks also to my committee members Mark Van Roojen, Joe Mendola, and Raymond Hames. I would also like to thank the graduate students and faculty members in the Department of Philosophy at University of Nebraska for their comments on presentations of material in this dissertation for the graduate student colloquia series. Janice Dowell and David Henderson helped direct my research into fruitful areas.

# TABLE OF CONTENTS

# CHAPTER 1

# EPISTEMOLOGY, THEORY, AND METHOD IN CULTURAL ANTHROPOLOGY

## Introduction

In recent years there have been many heated debates about the correct theory and method for ethnographers to use. These debates have been sparked by the postmodern critiques of traditional epistemological assumptions, and the methods that result from these assumptions. Traditionally, anthropologists from Durkheim to the present have assumed that it is possible to record and interpret human culture in terms of social laws, or in terms of certain kinds of social relationships. Recently, the optimistic epistemological underpinnings of the traditional view have been taken up by a hard science oriented group of anthropologists to some degree.

The postmodernist critique of the traditional goals and methods of anthropology, if accepted, would require that the enterprise of anthropology as a social science be abandoned. I will argue that anthropologists should not, and do not have to, adopt the postmodernist's skeptical epistemology in order to respond to their criticisms of the traditional method of anthropology. I will begin by making a distinction between two types of epistemological issues. I will then state some of the traditional goals and methods of anthropology. Then I will explain the postmodern critique of traditional anthropology. I will argue that there are two types of criticisms of the traditional method of anthropology, and that they are independent of one another. One type of criticism of the method is rooted in the postmodern epistemology. The other type of criticism of the

method is rooted in a desire to make the methods more accurate. I will also argue that anthropologists must have an overarching optimistic epistemology to preserve anthropology as a social science. As an illustration of some of these points I will discuss the work of Jill Dubisch, a self-proclaimed postmodernist, feminist anthropologist. I will show that although she uses the language of postmodernism, she does not adopt the radical skepticism of postmodernism and is therefore not really a postmodernist. Although she is not really a postmodernist, she uses some postmodernist ideas in an effective way. Finally, I will address what these things mean for cultural anthropology.

**Sorting out the Epistemological Issues**

A result of the recent debates over the method of anthropology is that there are at least two types of anthropologists, those that believe that anthropologists collect factual data that aims at the truth, and those that believe anthropologists cannot collect factual data that aims at the truth, and so anthropology has a literary purpose. I will call the first group traditional anthropologists and the latter group postmodern anthropologists. The dichotomy can most generally be put in terms of those with an optimistic epistemology and those with a pessimistic epistemology.[1] Those with an optimistic epistemology believe that it is possible to understand and represent the lives of people in other cultures

[1] This distinction has been made by many people in different ways. Rik Pinxten, in *When the Day Breaks: Essays in Anthropology and Philosophy* (Frankfurt: Peter Lang Verlag, 1997), makes the distinction between the objectivist view of anthropology and the subjectivist view. James Lett, in *Science, Reason, and Anthropology:A Guide to Critical Thinking* (Lanham, Md.: Rowan & LittleField, 1997) makes the distinction between science-oriented anthropology and interpretive anthropology. Marvin Harris, in "Science, Objectivity, Morality," in *Anthropological Theory in North America*, ed. E.L. Cerroni-Long (Westport, Conn.: Bergin & Garvey, 1999), 77–84, makes the distinction between traditional anthropology and the moral model. Both Pinxten and Lett take postmodernists to be in the interpretive or subjective camp. The distinction I am making is more general.

in a factual way, a way that aims at the truth. Those with a pessimistic epistemology believe that it is impossible to accurately represent and understand the lives of people in other cultures: they believe that either in the act of representing other cultures, or in the analysis of their behavior, or both, anthropologists *cannot*, due to the nature of language or the methods employed, say anything true or factual about other cultures.

This division stems from the fact that anthropologists have not distinguished between two sets of epistemological issues. There are two ways to talk about epistemological issues in anthropology. The first way is to talk about the anthropologist's overarching attitude toward knowledge. The second way is to talk about the best methods of acquiring knowledge. Since there are two ways to talk about epistemological issues, there are also two types of criticism. One type of criticism addresses the overarching attitude toward knowledge. The other type of criticism addresses the methods of anthropology. Given that there are these distinctions, anthropologists should be clear about the type of criticisms they are giving.

The overarching attitude that anthropologists have toward knowledge affects the goals and methods of anthropology. Historically, anthropologists have had an overarching optimistic epistemology. Since anthropologists have had an optimistic epistemology, they have developed methods for describing and understanding the lives of other people. The primary method of cultural anthropology is participant observation. Participant observation requires the anthropologist to live with a group of people, observe them, describe their lives in a variety of detailed ways, and to participate in the daily life of the people with whom they live. If an anthropologist did not think that it was possible

to understand and represent the lives of people in other cultures in a factual way, then they would not adopt participant observation as a method of accomplishing the traditional goals of anthropology.

Recently, traditional anthropology has been criticized by postmodernists. They criticize both the overarching attitude and the methods of traditional anthropologists. Postmodernists have an overarching pessimistic epistemology. Postmodernists, in philosophical terms, are skeptics about knowledge. As a result, they think that the traditional methods of anthropology cannot help to achieve the traditional goals of anthropology.

It is important to understand the relationship between the overarching attitude toward knowledge and the goals of anthropology. If one thinks that it is possible to gain knowledge and accurately describe events, and that there are or could be methods for doing so, then one will probably include these things or these sorts of things as part of one's goals.[2] However, if one believes that it is impossible to gain knowledge or accurately describe events, one should also believe that there are no methods that would allow someone to do these things, and so would not include them as part of their goals. Postmodernists think that traditional anthropologists have the wrong overarching epistemology. They support their claims by trying to show that the methods of traditional anthropology fail to achieve the goals of traditional anthropology. The criticism of the methods, for the postmodernist, is rooted in their overarching pessimistic epistemology. Postmodernists think that showing that the traditional methods of anthropology do not

[2] One should note that there is not a clear division between goals and methods. Some things, like writing accurate descriptions of events for example, are both methods and goals.

work shows their pessimistic epistemology is the correct one. They also make claims about language that, if true, undermine a human being's ability to say anything about the world as it is. In order to fully understand these criticisms it is helpful to have an understanding of the traditional goals and methods of anthropology. To these we now turn.

**Traditional Goals and Methods of Anthropology**

The primary traditional goal of cultural anthropology, generally described, is to understand human culture. This goal is achieved by accomplishing smaller goals when doing fieldwork, for example, describing the daily lives of the people, documenting social structures, and providing quantitative data. This information is then used as the data for writing an ethnography. When anthropologists write ethnographies, they usually focus on specific aspects of a culture. For instance, an ethnographer might focus on pilgrimage, hunting and gathering, marriage, gender, or religious rituals. No matter what the focus of an ethnography, the anthropologist will describe the lives of the people they are studying, document social structures, and provide quantitative data. By doing this, the anthropologist attempts to increase our knowledge of a culture or some aspect of a culture. Given that these are their goals, traditional anthropologists have developed methods that they think allow them to accomplish them to an acceptable degree. Some of these methods include collecting quantitative data and participant observation.

Even though the goals of anthropology are diverse, most anthropologists think that an ethnography should contain some quantitative data. Most, if not all, ethnographies contain an account of the population size, a map of the area, some statistics about the

surrounding land and population, and possibly, an inventory of material culture. Every ethnography contains some quantitative information about the people being studied. Providing quantitative data is expected and essential to writing a credible ethnography.

An anthropologist's primary method of studying other cultures is participant observation. Participant observation requires the anthropologist to live with the people they are observing. During their stay they will describe the aspects of the culture they are interested in. They will also participate in various aspects of the lives of the people they are studying. They may help with harvests, prepare for and participate in religious rituals, or learn how to hunt. It is through the process of living with the people they are studying that anthropologists will gain an understanding of family structures, religion, the roles of men and women, and many other aspects of a culture. Anthropologists record their observations and use these notes to write an ethnography. Traditionally, an ethnography is written as an objective account of the anthropologist's fieldwork. Usually, the first chapter of an ethnography explains the theory and methods of the ethnographer. The rest of the ethnography is dedicated to describing and explaining certain aspects of the group the anthropologist studied. Traditionally, an ethnography does not include an account of the anthropologist's personal struggles, feelings, or reactions to being in the field. Often anthropologists will have two sets of notes: one that reflects their professional observations and another that reflects their personal reactions. The two notebooks are supposed to help the anthropologist write an objective account of their fieldwork.

One can see that having an overarching optimistic epistemology leads traditional anthropologists to have certain kinds of goals and methods. If traditional anthropologists

did not believe that it is possible to understand human culture, they presumably would not have it as a goal. The kinds of goals a discipline has shape the kinds of methods they use and vice versa. For instance, if a method was shown to distort or produce incorrect data, those using the method would either redesign the method to fix the problem, or if that could not be done, abandon the method. If the method could not be fixed, anthropologists would look for another method, and if no other method could be found, they would abandon the goal at which that method was aimed (or set the goal aside until another method was found). The kinds of goals that any discipline has are based on, at least, the implicit agreement that those goals are attainable to some degree. If there was not this sort of agreement it would not make sense to devise methods to achieve these goals. The evidence that anthropologists believe that their goals are attainable to some degree is that they are expected to use certain methods, which they believe will produce good data, in order to write credible ethnographies. There must be some at least implicit agreement among anthropologists that the current methods of anthropology work to an acceptable degree because they use those methods and are expected to use those methods by their colleagues. Because methods are methods that aim at goals, if there is agreement that the methods work to an acceptable degree their must also be agreement that the goals are attainable. Let us turn now to the postmodernist's critique of these traditional goals and methods.

**The Postmodern Critique**

Postmodernists usually begin their critique of traditional anthropology by attacking the method. The methods of cultural anthropology are different in significant

ways from the methods of the natural sciences and other social sciences. While the natural sciences focus on the collection and use of quantitative data, the anthropologist's main method is participant observation. Anthropologists use quantitative data in every ethnography, but the vulnerable part of their method is participant observation. There are a number of concerns that postmodernists, and anthropologists in general, have about participant observation. The first area of concern is the anthropologist's position as participant observer. There are many reasons that both postmodernists, and anthropologists in general, find participant observation worrisome.

The main reason participant observation is worrisome is that it requires the anthropologist to become immersed in the culture. The anthropologist, as a participant observer, must do many things. They must learn and participate in the rituals, ceremonies, meals, harvests, etc., of the group. They must write detailed descriptions of the daily life of the people. They must be able to interview all members of the group. They must attempt to get an insider's perspective while remaining an outsider. Each of these activities can be called into question if we recognize that the anthropologist's values, gender, and topic of study can all influence how well or to what extent they can be a participant observer. For instance, the anthropologist must put aside their feelings about ceremonies or rituals they might consider barbaric (head-hunting, for example). One can wonder if it is possible to set aside feelings of disgust or even pleasure entirely. The anthropologist's attitudes toward a ritual will influence how they choose to write about the event. Similarly, who the anthropologist associates with can also be a factor in the characterization of the events or social structure. Many societies are stratified in some

way and do not allow associations among the strata. If an anthropologist is to talk to everyone, they may have to violate the rules of the society which may have adverse effects on later interaction with the group. It is not clear that an anthropologist will, in all situations, be able to give a well-rounded picture of a group for this reason. Also, gender can create a problem in groups that have strict gender roles. Some societies have rituals that can only be attended by men. Gender may also affect what a person of the opposite gender is willing to tell the anthropologist. Another problem for anthropologists is both observing and participating at the same time. Sometimes rituals require that a person ingest mind-altering substances which impair judgment. In this case it would be difficult for the anthropologist to give accurate descriptions of the ceremony. All of these things can affect the anthropologist's ability to give accurate descriptions.

The second area of concern is how the anthropologist describes and characterizes their experience in the process of writing an ethnography. One of the main goals of fieldwork is to compile detailed notes about the daily lives and/or specific aspects of the lives of a group of people. Many aspects of the task of describing are problematic. First, there is a concern that anthropologists will impose their western academic values on the culture in order to make sense of it, but in doing so they distort the real meaning of rituals and power structures in the culture. Anthropologists require a theoretical framework, such as structuralism, functionalism,[3] etc., in order to make sense of the group or cultures

[3] According to Robert Layton, Structuralism and Functionalism were both popular and influential theories in the 1950's and 1960's. They are similar in that they both focus on social structures like the family, religion, government etc. Functionalism focuses on customs that stabilize a social structure, customs that help individuals get what they need for survival, and the interconnections between customs and social structures. Structuralism claims that social structures are the expression of a group's belief system. For more see Robert Layton, *An*

they study. They use theories to organize and interpret their data. The problem with this, some think, is that it does not accurately represent the group the anthropologist is studying and in this way makes the descriptions non-representational. This objection is exacerbated by the fact that anthropologists do not agree on the best theoretical framework. This being the case, it seems difficult for anthropologists to examine whether or not their theoretical framework helps to accurately represent the cultures they study.

Another concern with descriptions is the issue of gender. Many anthropologists feel that the tradition of anthropology has either neglected or misrepresented the role and importance of women. Due to sexist attitudes of some researchers, or cultural biases that downplay the importance of women, researchers have not studied, with equal consideration, the lives of women.

There are many concerns about the method of anthropology because so many aspects of participant observation involve giving detailed descriptions. The anthropologist is in the unique position of being both a member of the group which is the subject of their study and at the same time being a scientist studying the group. The main questions that arise from all of these criticisms, and because of the nature of the unique tenuous position of the anthropologist, are: Can the methods of anthropology yield appropriately accurate descriptions of a culture? If so, how? If the methods of anthropology do not, and cannot, yield accurate descriptions of a culture, what are the alternatives and implications? The postmodernist answers these questions by first denying that there are any methods that can yield accurate descriptions. Second, they

---

*Introduction to Theory in Anthropology* (Cambridge: Cambridge University Press, 1997), especially chapters two and three.

think that the only alternative for anthropology is to write in a way that does not require accurate descriptions. It is interesting to note that postmodernists do not usually attack anthropologists' use of quantitative data (although their criticisms would also apply to this sort of data). This is an important fact because every ethnography (including those that claim to be postmodernist) contains this sort of information. Ethnographic writing is different from literary writing in at least this respect. It is also similar to other scientific writing in at least this respect. If postmodernists are serious about their criticisms, they would need to give up the assumption that quantitative data is an essential part of writing an ethnography. Many anthropologists who claim to be writing postmodernist ethnographies do not, and do not seem to be willing, to give up that assumption. Although many anthropologists are worried about their ability to reach the goals that they have set for themselves, they are in other ways faithful to the basic assumptions of traditional anthropologists.

**Postmodernist Arguments**

Postmodernism takes many forms, some more radical than others. The most radical strain is represented by the views of Jacques Derrida. In *Of Grammatology*, Derrida espouses the view that language is a human construct which distorts what it describes. Because it distorts what it describes we cannot know the world as it really is. So any attempt to describe the world as it is fails. In addition, because we know what we know about the world using language, we really can't know anything about the world as it is. A result of this argument is that since there are no techniques for description or explanation that do not involve language, there are no techniques for accurate description

and explanation. Postmodernists move from the claim that there are no methods that yield accurate descriptions and correct explanations to the position that there are no perspectives or methods that are 'privileged.' By 'privileged,' postmodernists mean something like "are any better than any other." Given their position on methods of description and explanation, postmodernists argue against using methods that purport to describe the world as it is. In particular, postmodernists tend to be skeptical of truth, meaning, and reason.[4] They think that "truth" and "reason" are "meta-narratives"[5] that the powerful use to "appropriate" the culture, identity etc. of the weak.[6] Because of this, many postmodernists think that anthropologists have oppressed the people they study by claiming to describe their lives in an objective, or representational way. Postmodernists would say that by making sense of other cultures in terms of a theoretical framework, the anthropologist has 'privileged' their own values and perspective.

Postmodernists conclude from these considerations that the only goals left for anthropologists are literary goals. Postmodernists would argue that because any attempt to represent the world as it is will fail, we should not attempt to represent the world as it is. Rather, we should realize that what we say about the world is a non-representational construct. In this sense an ethnography is fiction. Ethnography is fiction because it does not describe the world as it is; it describes it as the *anthropologist* sees it, and not as the

[4] See Dominic Strinati, *An Introduction to Theories of Popular Culture* (London: Routledge, 1995), 222–228.

[5] As I understand it a "metanarrative" is a theoretical framework of any kind that attempts to integrate or synthesize a multitude of information. Some examples from Dominic Strinati are science and religion. But sometimes reason or truth are also considered metanarratives.

[6] See E.L Cerroni-Long, "Anthropology at Century's End," in *Anthropological Theory in North America*, ed. E.L. Cerroni-Long (Westport, CT: Bergin & Garvey, 1999), 5.

people being studied see it or as it is. So, although we might describe what we see we do not produce scientific data. By suggesting that the only possible goals for anthropology are literary, postmodernists suggest that anthropologists give up all of the traditional goals of anthropology. Given that they suggest the only goals for anthropology are literary goals, and literary goals are not scientific goals, they are asking that anthropologists quit being social scientists. This is why their critique is both radical and devastating to anthropology as a social science.

Despite the fact that the postmodernist critique, if accepted, requires the anthropologist to give up doing anthropology as a social science, many anthropologists find the postmodernist view very appealing, and claim to have accepted it. Some explanation of why this view that is antithetical to anthropology (and any science) is so appealing to so many anthropologists is needed. I think the appeal of the postmodern view is that the criticisms of the method seem right. There are a lot of difficulties with how participant observation can produce objective and accurate results. These difficulties are inherent in the nature of participant observation. It is apparent that an anthropologist's feelings and values affect what they focus on, what they record, and the way they characterize what they see. It is also clear that human subjects might lie or be self-deceived, which can and does affect the data. These weaknesses in the method of participant observation seem to be what makes the postmodernist position persuasive to those who accept it.[7] Rather than accept the postmodern view, a more productive way to

[7] Although I am focusing on postmodernists here, anthropologists that are scientifically-minded are also motivated by these weaknesses to approach their fieldwork differently. The scientifically-minded anthropologists react to the weaknesses by either de-emphasizing

address these problems is to take seriously the methodological criticisms, but separate them from the overarching pessimistic epistemology of postmodernism.

**Separating Methodological Criticisms from Criticisms of the Overarching Epistemology**

What has not been articulated well by anthropologists criticizing the methods of anthropology is the difference between criticizing the overarching epistemological claims of anthropology as a discipline, and criticizing the ways that anthropologists acquire knowledge and what counts as knowledge in anthropology. If this distinction was made it would be easier for the two groups of anthropologists to start a dialogue about their discipline. Since much of the appeal of the postmodern view is centered on the criticisms of the method, it is important to be clear about what exactly motivates a criticism about the method. For the postmodernist, criticisms about the method of anthropology are rooted in their overarching skeptical epistemology. Because these criticisms stem from their overarching skeptical epistemology, someone who makes these criticisms should be committed to that kind of skepticism. Although the postmodernist criticisms of the method do lend some support to their claim to an overarching skeptical epistemology, they by no means provide enough support to warrant the acceptance of an overarching skeptical epistemology. This is because one does not have to provide criticisms of the method rooted in skepticism even though one can. Another type of criticism comes from a concern that the methods are not producing good data and a desire to make the methods more accurate. These types of criticisms will focus on the same sort of problems that

---

interviews with individuals or not relying on them at all, which is a strategy I also find problematic.

postmodernist criticisms do, but where the postmodernists dismiss any solution to the difficulties, those who make criticisms that are not rooted in skepticism think that there are, or can be, solutions to the difficulties with the method. Clearly, one can criticize the method without adopting an overarching pessimistic epistemology. Many of the disagreements among anthropologists stem from the fact that those who criticize the method do not clearly distinguish between these sorts of criticisms. Because these distinctions have not been made, there is an incongruity between what some anthropologists say and what they do. This point is demonstrated in the following section about Jill Dubish. If these distinctions were made we would find that many anthropologists do not take issue with the overarching epistemology of anthropology. Rather, they are concerned with how to reach the traditional goal of gaining knowledge through the methods of anthropology.

The fact that there can be two different kinds of criticisms of the method in anthropology is very important because the consequence of accepting the postmodern version of the criticisms requires abandoning doing anthropology as a social science. This consequence of adopting the postmodernist's epistemology has not been widely acknowledged by postmodernists. In fact many anthropologists that claim to be postmodernist also claim to be doing anthropology. However, what would it mean to do anthropology with a skeptical epistemology? There would be no attempt to provide accurate descriptions, quantitative data, or to explain or understand culture. Instead, one would be restricted to recording one's personal feelings and observations without the pretense of authority that academic work usually has. But what would separate this from

travel writing? A travel writer usually records their feeling and observations about places they visit without the pretense of authority. Even anthropologists that claim to be postmodernist seem to have more ambitious goals than travel writing. Postmodernist anthropologists generally include quantitative data and conform to many of the standards that traditional anthropologists do. Further, they continue to work as *anthropologists*, rather than abandoning the discipline altogether. It seems that if one were truly committed to skepticism about knowledge, one would state one's case, as many postmodernists have, and then abandon endeavors that are clearly in conflict with one's fundamental philosophical commitments. The fact that postmodernist anthropologists do not quit the field but have tried instead to change it indicates either that they do not realize the consequences of their position or they are not truly postmodernist. I think that the latter is the case for most anthropologists that claim to be postmodernist. Some additional evidence for this claim is that many postmodernists take issue with just the methods of anthropology. They claim that new methods need to be developed to deal with the problems of the current method.[8] The goal of these new methods is to create a more accurate ethnography. There is a basic optimism in trying to come up with new methods to write more accurate ethnographies. If anthropologists that were concerned with the problems of the traditional methods of anthropology recognized that one does not have to adopt the postmodernist overarching epistemology to be sympathetic to their

[8] This is discussed in great detail by George E. Marcus and Michael M. J. Fisher in *Anthropology as Cultural Critique: An Experimental Moment in the Human Sciences* (Chicago: The University of Chicago Press, 1986).

concerns, there would be much less conflict among anthropologists about the appropriate direction for anthropology.

Although there is a great deal of disagreement over the best methods for cultural anthropology, anthropologists should be able to agree on some basic theoretical points. First, they must agree that anthropology aims at producing knowledge. Second, they must agree that if anthropology aims at producing knowledge, then it must deal in facts. Third, they must agree that it is possible to collect facts and data from some method of fieldwork. I think that many anthropologists that *claim* to be postmodernist would actually agree to these things. I think they would agree to these things because many of their criticisms are strictly concerns about the methods used to accomplish the traditional goals of anthropology. Their criticisms illuminate many problems with the method and the theories that have guided the interpretation of data. The goal of using new methods is to create a better, more accurate, ethnography. This goal is incompatible with radical skepticism.

To illustrate these points we turn to the work of Jill Dubish. Dubisch is an example of an anthropologist that finds something appealing about postmodernism and calls herself postmodern, but her stated goals and methods are not those of a skeptic. Her ethnography *In a Different Place* explores many aspects of pilgrimage in Greece. Dubisch's discussion of her goals and methods illuminates what she finds appealing about postmodernism; while at the same time her work demonstrates the point that even though she claims to be postmodernist, she does not really adopt an overarching skeptical epistemology.

**Jill Dubisch**

*In A Different Place*, an ethnography by Jill Dubisch, is supposed to be an example of a postmodernist, feminist, experimental ethnography. If in fact it is a postmodernist ethnography then Dubisch has made some contradictory claims about her goals and method. Since I do not think Dubisch intended to make contradictory claims, I will show how her ethnography is not postmodernist. However, she does take the criticisms of postmodernism seriously, while maintaining the traditional goals of anthropology. Dubish uses non-traditional methods for both collecting data and in the process of writing her ethnography. But she aims at producing a more accurate ethnography by using these experimental methods.

In the introduction to *In A Different Place*, Dubisch establishes her goals for writing the ethnography. The first two she groups together. She says, "First among these aims is the presentation and interpretation of the material on pilgrimage I have collected in the course of my research."[9] The first goal then is simply to present the data and the second to interpret it. These are both traditional goals. Dubisch's third goal is "to explore certain facets of my own experience as a 'participant observer.'"[10] The facets she hopes to explore include (a) gender, as it relates to the subjects she is studying and her position in the field of anthropology; (b) previous fieldwork done in Greece by other anthropologists, and (c) how dealing with chronic pain influenced her data. Her fourth goal is to consciously examine the practice of ethnographic writing. In addition to these

[9] Jill Dubisch, *In A Different Place: Pilgrimage, Gender and Politics at a Greek Island Shrine*, (Princeton: Princeton UP, 1995), 3.

[10] Ibid, 3.

goals, Dubisch sets these goals within the context of a reflexive approach to research. To approach something in a reflexive way is "to recognize that we are part of the social world we study."[11] This may automatically bring to mind postmodern criticisms about our inability to have an objective view of anything, because a reflexive stance is one which acknowledges the dual position of the anthropologist as an insider and an outsider. However, reflexivity does not automatically lead to skepticism about accurate or objective descriptions. In fact, Hammersley and Atkinson, who propose this view as a method anthropologists should use, also say:

> There is no way in which we can escape the social world in order to study it, nor, fortunately is that necessary. We cannot avoid relying on 'common-sense' knowledge nor, often, can we avoid having an effect on the social phenomena we study. . . . We must work with what knowledge we have while recognizing that it may be erroneous and subjecting it to systematic inquiry where doubt seems justified. Similarly, instead of treating reactivity merely as a source of bias, we can exploit it. How people respond to the presence of a researcher may be as informative as how they react to other situations.[12]

One can see how the position the anthropologist finds themselves in, though reflexive, can be productive. Dubisch seems, in the end, to be proposing that reflexivity is helpful in producing good data.

---

[11] Martyn Hammersley and Paul Atkinson, *Ethnography: Principles in Practice* (London: Tavistock Publications, 1983), 14.

[12] Ibid, 15.

The first two goals that she has established are traditional anthropological goals. She accepts that anthropology should be aimed at collecting data in the form of accurate descriptions. Some postmodernist critics would like to replace this traditional goal with the goal of creating literary narratives that are focused on the feelings and reactions of the anthropologist. However, Dubisch seems to reject the postmodern version of the goals of anthropology on many levels. First, she says, "All of my experiments, however, are ultimately directed toward the goal of presenting to the reader a place and a way of life as experienced in the particular set of activities that cluster around a pilgrimage site,"[13] which indicates that she is aiming at the traditional goal of most ethnographies—to give the reader an accurate sense of the place and people being studied. Second, she very clearly establishes that, "Although the account that results from these efforts has at times a highly personal dimension, the result in not intended as autobiography or confessional but rather as an elucidation of the relationship between the anthropologist and the field situation on the one hand, and the anthropologist's relationship to the theory and practice on the other."[14] One of the problems that has made postmodernism attractive is that traditionally anthropologists have had to remove any personal feeling from their ethnographic writing. In doing this many anthropologists feel that something significant is missing from the ethnography. Dubisch, in an attempt to deal with this concern, has added certain personal aspects of her experience. But, she says, the goal of this is to help the reader more clearly understand her relationship to her field experience and her

[13] Dubisch, 7.

[14] Dubisch, 6.

relationship to the theory that she uses. She acknowledges that she uses non-traditional *methods* that are aimed at traditional goals. Thirdly, she rejects the claim that the product of anthropology is fiction in the postmodernist sense. However, she does agree with Paul Rabinow that anthropology is fiction in the sense that it is constructed out of data. She says:

> Paul Rabinow...speaks of anthropology as "fiction." By this he means not that it is false or that we "make it up," but rather that our accounts of other cultures are constructed; they are made. . . . For field work never presents us with a coherent account but rather with information and images that we must select and arrange and present and write about . . . using our own creative skills.[15]

It is clear that what she accepts is that, in writing an ethnography, the anthropologist is responsible for the way the information is presented. It is in this sense that anthropologists create ethnographies, however, they do not intend to write things that are false or fabricated. Since Dubisch agrees that the anthropologist ultimately aims at accurate descriptions, which may involve a complex process, she cannot accept the postmodernist claim that the product of anthropology is fiction. Clearly, then, Dubisch does not reject the traditional goals of anthropology, and hence, is not really a postmodernist.

A postmodern critique of the method of anthropology would reject any attempts, through participant observation, or traditional ethnographic writing, to arrive at scientific data. Postmodernists reject the idea that there is scientific data that is connected to truth (in any sense). It is clear that Dubisch does not accept these claims, but, she does have

[15] Dubisch, 16–17.

concerns about the method. Her concerns are taken from postmodernist criticisms of the traditional writing style of ethnographers, feminist concerns, and the problem of reflexivity discussed earlier.

The most radical claims Dubisch makes are about the traditional writing style. She says, "In my writing I have attempted to move away from the sort of 'realist' account of 'objective' facts recorded by an omniscient and invisible observer that was the characteristic mode of much of 'traditional' ethnography. . . . Instead I punctuate my account with analyses of my own writing as I experiment with different modes of presentation for different ethnographic purposes."[16] This quote is somewhat ambiguous. Dubisch is claiming to be "moving away from" realist, objective accounts of facts which makes one wonder what she means by "moving away." Does she mean that she is rejecting realist and objective accounts? I do not think so. What she seems to mean is that she will not write in a style which conveys her observations as if they were indisputable or detached from the observer. What she is critiquing here is the traditional way anthropologists *describe* and *record* their observations. Traditionally the ethnographer has been expected to entirely separate their emotions, biases, theoretical underpinnings, and presence from the narrative.[17] By doing this the traditional ethnographies tend to be presented as if they were written by an omniscient and entirely objective observer. Dubisch is claiming that anthropologists should reject this traditional practice in favor of one that explicitly includes the emotions, biases, theoretical underpinnings and presence

[16] Dubisch, 4.

[17] Often when anthropologists are in the field they keep two journals, one to record the ethnographic data and another to record their feelings and reactions to their experiences.

of the anthropologist. Dubisch writes that one reason she adopted this view is, "I found that I required a variety of strategies in order to arrive at any sort of understanding of, or feeling for, that which I was studying."[18] Overall, the main motivation for moving away from the traditional writing techniques of anthropology is that including the things usually left out provides the reader with a more accurate view of the place and people.

In addition, Dubisch's feminist critiques aim at improving the method, which in turn improves the data. Dubisch says, "I felt no universal sisterhood as the basis of my interest, only the concern that women and certain features of their lives were being overlooked or misrepresented by male anthropologists."[19] Her concern here is for the inclusion of women and a more accurate depiction of the lives of women.

Further, Dubisch is worried about making generalizations about a culture. She appeals to her own difficulties in trying to write an ethnography that made generalizations about its subjects from particular instances. Traditional writing encourages the ethnographer to make these sorts of generalizations. Dubisch, while being uncomfortable with this practice, says, "While I think that a certain amount of generalization is necessary (and certainly it is practiced even by peoples of the cultures in which we work), . . . [critiques of generalization displace] generalization from its privileged position as the "right" way to do anthropology and [open] the possibility of other modes of anthropological presentation."[20] This quotation characterizes well

---

[18] Dubisch, 5.

[19] Dubisch, 13.

[20] Dubisch, 15.

Dubisch's stance on all the postmodern critiques: they are powerful criticisms which require the methods used by anthropologists to change to some extent. It is clear that although Dubisch takes the postmodern criticisms seriously she does not adopt an overarching pessimistic postmodern epistemology. She seems to be grappling with the more important questions: Can the methods of anthropology yield appropriately accurate descriptions of a culture? If so, how? Her answer to the second question is, "I can only try to be conscious of what and how I write, and strive to make the reader conscious of the hidden agendas of my writing, as well as of its inevitable inadequacies. Ultimately, the authority is mine, and I must take responsibility for it."[21] Jill Dubisch's ethnography illustrates that an anthropologist can have an overarching optimistic epistemology and still, in a very productive way, take the most serious criticisms of the postmodernists seriously. Her criticisms, though couched in postmodernist language, are scientific in nature.

**Conclusion: The Larger Question**

Although I have established that the postmodern view is incompatible with the traditional view and that postmodern critiques can be taken seriously while maintaining the traditional view, I have not addressed the most pressing questions. Can the methods of anthropology yield appropriately accurate descriptions of a culture? If so, how? If anthropologists are to make progress in their field, there are many questions relating to these that need to be dealt with. For instance, anthropologists need to seriously consider what the goals of anthropology are and come to some consensus. They also need to

[21] Dubisch, 19.

consider, specifically, which methods are essential to the field and which are not. If participant observation and ethnographic writing are essential to the field, and I think they are, then anthropologists need to carefully assess what the limits of these methods are and how they can be modified to produce better data. The postmodernist and feminist critiques serve to highlight the fact that traditional ethnographies have been biased in ways that could have been prevented had the anthropologist taken a more careful look at their relationship to the theory they were using and their place among the people they were studying.

Anthropologists like Jill Dubisch have confidence that the methods of anthropology, if modified, can yield appropriately accurate descriptions of a culture. The only way to know if this is true is for anthropologists to examine their own methods and data, and come to some consensus about its truth and accuracy. A careful study by anthropologists of what, if anything, the modifications to the method do to help produce more accurate data needs to be made. Special attention must be given to how and to what extent the methods of anthropology produce data. By having a clear handle on how this is done, the significance of the ethnographer's role in the narrative will become more clear. Having a standard way to include important information about the ethnographer in a narrative will help to address postmodernist concerns and change the way ethnographies are written. This, in the end, should promote a more scientific, rather than a more literary, anthropology. There are many questions that need detailed and agreed upon answers in the social sciences and this "experimental moment" in anthropology opens the door to those answers.

# CHAPTER 2

# IS CULTURAL ANTHROPOLOGY SCIENCE?

## Introduction

Anthropology has been pulled in two different directions in recent years. One direction is toward postmodernism; the other is toward sociobiology and evolutionary psychology. A main motivation for moving anthropology toward sociobiology and evolutionary psychology are the challenges to cultural anthropology's status as a science. Although there are a number of ways to challenge cultural anthropology's status as science, three which originate from the work of E.O. Wilson and other sociobiologists are particularly pressing: (1) Interviews do not produce useable data because it is impossible to get an accurate assessment of a person's beliefs, desires, and intentions; (2) Beliefs, desires, and intentions are not explanatory because they are not causally efficacious; and (3) even if beliefs, desires, and intentions are causally related to behavior, the causal connection is not the kind required for scientific explanation. The third objection is also articulated in "Causal Explanations of Behavior" by Merrilee Salmon, who argues that scientific explanations must make reference to general causal laws. I hope to show that each of these criticisms fails to show that cultural anthropology is not science.

## The Demarcation Problem

Criticisms of cultural anthropology can be best understood in the context of the traditional demarcation question: How should we distinguish science from non-science?[1]

[1] A classic statement of this problem can be found in Karl Popper, *Conjectures and Refutations*, (London: Routledge and Kegan Paul, 1963).

Philosophers of science have attempted to devise a set of conditions that distinguish science[2] from non-science. This is often done in an effort to distinguish science from pseudo-science. However, the same issues arise when trying to understand what, if anything, makes the social sciences science. Some of the features philosophers of science use to distinguish science from non-science are the use of the scientific method, including experimentation and testability; the ability to make predictions and give causal explanations; and the discovery of, and appeal to, laws of nature. These features are connected to one another in important ways. The ability to make predictions depends on the discovery of causal explanations, which often, in the natural sciences, involves an appeal to the laws of nature. Hypotheses for experiments are often about causal connections. The problem with the methods of the social sciences is that they lack many of the features on this list. Anthropologists, for example, cannot experiment on their subjects because of ethical constraints which limit the kinds of predictions they can make. Their limited ability to use all of the methods of the natural sciences is the foundation of their questionable status as true science.

Experimentation and testability are not the only troublesome features of science for social scientists. There is also a serious question about whether or not explanations in the social sciences appeal to natural laws.[3] This is a particularly serious issue because the discovery and use of natural laws is often taken to be a unifying feature of scientific

---

[2] For example, Philip Kitcher's *Abusing Science: The Case Against Creationism*, (Cambridge: The MIT Press, 1998), and Michael Ruse, "Creation Science Is Not Science," *Science, Technology, and Human Values* 7 (1982): 72–78.

[3] Some particular concerns in this debate are discussed in Harold Kincaid's article "Defending Laws in the Social Sciences," *Philosophy of the Social Sciences* 20 (1990): 56–83.

explanations. All of these concerns about the social sciences lead many natural scientists and philosophers of science to be suspicious about the status of the social sciences. How social scientists respond to these concerns will either make it clear that they are indeed sciences or that they are not. We will see that there are plausible ways to preserve the scientific status of the social sciences. We turn now to the specific criticisms that arise out of the worries about demarcation.

**Interviews**

A staple of good science is good data. Without data that accurately represents facts it would be impossible to do any scientific work. Anthropologists, psychologists, and other social scientists who work directly with human subjects have long understood the challenges of getting good data from human beings. Human subjects are crafty. Human beings sometimes lie, are self-deceived, or unreflective about their own culture. Informants may be biased in significant ways. The way a person responds to an outsider (like the anthropologist) may be different than the way they would respond to a close friend. These sorts of considerations lead to the criticism that it is impossible to acquire accurate data from human subjects. Some argue that because human beings give unreliable accounts about their beliefs and desires, data collected by interviewing people about their beliefs and desires is unreliable at best and worthless at worst. If this is true, then anthropologists, despite their best efforts, would not be able to understand cultural traditions through one of their main methods, participant observation. Participant observation requires that the anthropologist get to know people in a group through informal and formal conversation. They record information from their experiences and

use this data as the basis for an ethnography. E.O. Wilson and others think that rather than try to understand a culture through interviews, anthropologists should come to an understanding of human behavior in the same way that zoologists come to understand other animal behavior—by simply observing behavior and recording it.

This objection has some prima facie plausibility because cultural anthropology has had some serious problems with informants and interviews.[4] However, the main claim—that it is impossible to accurately state the beliefs, desires, and intentions of a person—is false. I am not denying that people lie, are self-deceived, unreflective, coy, etc. However, by careful formulation of questions, and by building relationships with the subjects of study, an anthropologist can learn a great deal about the beliefs, desires, and intentions of the people they study. If it were truly impossible or even very, very, difficult to learn the beliefs, desires, and intentions of other people, ordinary people would not be able to function. Our common interactions with other people rely on the ability to both communicate our beliefs and desires and understand other people's beliefs and desires. When we ask for a cup of coffee at a coffee shop and in return we are given a cup of coffee, we have made our beliefs and desires clear and they have been correctly evaluated and understood. Our good friends come to understand a great deal about our motivations and can anticipate our excuses and explanations for our actions. Anthropologists develop

[4] A well known case of this is Margaret Mead's work on the sex lives of teenagers in Samoa. She wrote an ethnography called *Coming of Age in Samoa* (New York: HarperCollins, 2001) which was later discredited by Derek Freeman. The people she had interviewed as teenagers were re-interviewed as adults at which time they claimed to have lied to Mead. Even so, there are those who do not feel that Freemen fairly evaluated the work of Mead. Indeed there is much controversy over this case. For another perspective on this see Paul Shankman, *The Trashing of Margaret Mead: Anatomy of an Anthropological Controversy* (University of Wisconsin Press, 2009).

these everyday skills and employ them in their interactions with people in other cultures. Participant observation is a way for the anthropologist to foster a close relationship with the people she studies. Anthropologists spend months and often years with the groups they study. They live with the people they study with the hope that their prolonged presence will allow their subjects to become comfortable with them. The aim of participant observation is to foster relationships so that accurate data can be collected. This relationship, though perhaps difficult to create, is possible. Although there is no guarantee that the anthropologist will not encounter people who will lie or in some other way sabotage their efforts, there is a great deal of anthropological work to suggest that an anthropologist can know, understand, and accurately represent the beliefs, desires, and intentions of other people a fair amount of the time.

Anthropology has often been singled out for criticism, perhaps because of their own awareness of the difficulties they face in collecting data. However, they are not the only science that faces challenges in collecting good data. Doctors have challenges diagnosing disease because some diseases have similar symptoms. Historically, science has been challenged with acquiring good data. Biologists had to find a way to extract and examine DNA. Astronomers had to build machines for collecting samples from other worlds. None of these things are easy to do. Science would have been halted very quickly if the sheer difficulty of a task was a good reason for abandoning it. The task of collecting data from human subjects is no more difficult than the tasks required to advance other sciences.

The suggestion given by Wilson and other biologists, that anthropologists should not rely on interviews as a source of data, is not a good suggestion. Rather, anthropologists should devise ways of catching and minimizing the problems with participant observation and interviews. With the proper techniques, anthropologists can produce good data from human subjects. For instance, psychologists have been able to develop surveys that can help diagnose mental illness. In order for surveys like this to be reliable, psychologists have had to find ways of asking questions that minimize bias and produce correct results despite a person's tendency to lie or be self-deceived. Anthropologists can develop techniques like these which are sensitive to cultural differences.

**Causal Relevance of Intentional States**

The second line of criticism of anthropology's scientific status is that the beliefs, desires, and intentions that it studies are not causally efficacious and therefore cannot explain cultural phenomena.[5] Wilson suggests this when he writes:

> Consider then the dreams of a magician, a sorcerer, a shaman. They are more than just unique productions of a single mind; they exhibit qualities general to the human species. The art of Pablo Amaringo is worthy of analysis in the manner of the natural sciences. His paintings are a test case of consilience, an arresting fragment of culture that might be explained and thereby given added meaning at the next, biological level down in complexity from artistic inspiration.

[5] This objection is also stated in the work of evolutionary psychologists. See Barkow, J., ed., *The Adapted Mind: Evolutionary Psychology and the Generation of Culture* (New York: Oxford University Press, 1992).

> It is the habit of scientists to look for elements available as entry points for such analysis. [6]

Wilson implies that rather than just try to understand the artist's artwork by appeal to their intentions, we can do more: we can *explain* the artwork "at the next, biological level down." The artwork is explained at the biological level as opposed to being merely understood in relation to the author of the artwork. It is interesting to note that Wilson does not suggest that the artwork be explained at the cultural level but rather at the biological level. What Wilson means by the "biological level" here is that most of a culture's traditions can be explained by evolutionary forces like natural selection, group selection and the like. Both Wilson, and current evolutionary psychologists, think that studying the beliefs, desires, and intentions of individuals in a culture is wrong-headed and that attempting to explain things based on interactions with individuals is a dead end. What underlies this criticism is the belief that the beliefs, desires, and intentions of people are not genuine causes of behavior. They only seem to be. They reason that since it is the evolutionary forces which really cause the behavior, the appropriate scientific explanations should focus on these things.

The view that evolutionary explanations, when available, should supersede anthropological explanations presupposes certain assumptions about the relationship between anthropological and biological explanations. Wilson seems to think that while anthropology helps us to understand culture, biology explains it. An articulation of the assumption that there can only be one genuine explanation can be found in Jaegwon

---

[6] E.O. Wilson, *Consilience: The Unity of Human Knowledge* (New York: Alfred A. Knopf, 1998), 74.

Kim's "Explanatory Realism, Causal Realism, and Explanatory Exclusion." Kim articulates the principle of explanatory exclusion as follows:

> [P]roffered explanations of a single event, with mutually consistent explanantia, can exclude one another in the following sense: there can be no more than a single *complete* and *independent* explanation of any one event, and we may not accept two (or more) explanations of a single event unless we know, or have reason to believe, that they are appropriately related—that is, related in such a way that one of the explanations is either not complete in itself or dependent on the other.[7]

Biologists, and sociobiologists in particular, have implicitly adopted this idea and conjoined it with the belief that the explanations they give exclude explanations that anthropologists give. Or perhaps, more charitably, biologists suppose that anthropological explanations are misguided because they do not recognize the real causal structure of explanations of animal behavior.

However, one can plausibly say that anthropological explanations do not compete with biological explanations. Rather, anthropological explanations are one part of a complex explanation. While sociobiology may give us causal explanations reaching back into our ancestral past, anthropological explanations are more proximate, appealing to causal factors salient in the lives of the current generation. Sociobiologists seem to acknowledge anthropology provides proximate causal explanations, yet they still insist

[7] Jaegwon Kim, "Explanatory Realism, Causal Realism, and Explanatory Exclusion," *Midwest Studies in Philosophy*, 12 (1988), 233.

that the genuine scientific causes of behavior are to be found at the biological level.[8] Evolutionary forces and genetics do play a causal role in what we do. They supply us with the abilities we have and a range of possible behaviors. But they do not *fully* explain our behavior. Evolutionary explanations do not explain how or why a group adopted specific religious beliefs, family structure, or economic patterns. The religious rituals of a group are caused by their beliefs about their god or gods. Understanding these beliefs will explain their particular behavior. In general, anthropologists describe and explain the intentional behavior of people in a culture.[9] Anthropologists study individual and group strategies for coping with the environment and the struggles of survival. If anthropologists describe and explain some of the causes of human behavior, why isn't explaining specific beliefs a scientific endeavor? One reason might be that sociobiologists and evolutionary psychologists do not think that intentional states are causally efficacious so they do not think that explanation in terms of intentional states are scientific.

However, there is a great deal of agreement, among philosophers, that intentional states can cause behavior.[10] Donald Davidson has defended this view in the following way. When we explain an action we cite a person's primary reason for the action. The

---

[8] A clear statement of this sort of position can be found in the introduction to *The Adapted Mind*, edited by Barkow.

[9] By 'intentional' I mean behavior that is done, in that instance, to affect a purpose, including behavior that is routine now, but started for a reason. I do not mean to say that anthropologists do not also study unintentional behavior. An anthropologist might discover that although fathers say that they give food to their children equally, they in fact give more to biological children unintentionally.

[10] There are some philosophers who do not hold this position including Harry G. Frankfurt, Carl Ginet, and G. Wilson, among others.

primary reason consists of a pro attitude. The pro attitude is a person's belief or desire, or both, that rationalizes the action. Davidson thinks that primary reasons are the causes of some actions. One argument he gives in support of this view is that a person can have a reason for performing an action and yet this reason is not why the person acts. In order for a reason to explain an action it must be the reason for which the action is done. It must be done *because* of the reason. If this is the case, we can see that when we are explaining behavior and we cite the reason the person behaves the way he does, the reason is the cause of the behavior. Otherwise it would simply be a reason for the behavior but not *the* reason for the behavior. So, in order for a reason to explain the behavior it must be causally connected to the behavior.[11]

Anthropologists are charged with explaining intentional behavior. They describe general patterns among a group and they describe the behavior of people performing certain actions. In their descriptions they cite the reasons (the beliefs, desires, and intentions) that cause the behavior they observe. In this way, at least, anthropologists do give causal explanations of human behavior.[12]

---

[11] For more see Donald Davidson, *Essays on Actions and Events* (Oxford: Clarendon Press, 1980).

[12] Davidson's description of agency could serve well as a characterization of ethnographic descriptions. Davidson says, ". . .causal explanation takes the form of a fuller description of an action, either in terms of a cause or of an effect. To describe an action as one that had a certain purpose or intended outcome is to describe it as an effect; to describe it as an action that had a certain outcome is to describe it as a cause. Attributions of intention are typically excuses and justifications; attributions of agency are typically accusations or assignments of responsibility. Of course the two kinds of attribution do not rule one another out, since to give the intention with which an act was done is also, and necessarily, to attribute agency." See Donald Davidson, "Agency," in *Essays on Actions and Events* (Oxford: Clarendon Press, 1980), 48.

The claim that beliefs, desires, and intentions are not causally relevant is incorrect. The particular beliefs, desires, and intentions of a person at least partly cause their behavior. What reason, then, could we have for excluding them from an explanation of behavior? If the kinds of explanations that anthropologists give are really causally relevant then they should be used in explanations of behavior, because to ignore the causal role that intentional states play is to ignore part of the cause of the event. To ignore a relevant causal factor is to give an incomplete explanation. Scientists, it seems to me, should be interested in giving the most complete explanations possible. As far as I can tell, no justification has been given for dismissing intentional explanations.

The aim of the criticisms focused on the difficulty of collecting data about intentional states is meant, in my view, to suggest that we do not have access to intentional states in the appropriate way, or at all. But in fact we have a great deal of access to intentional states of human beings. And although good data is hard to get, it is easier to get than many other kinds of data that scientists pursue. For example, getting samples from the surface of the moon, getting DNA from very old material, and getting testimony about sexual abuse from children are all very difficult things to do, and yet they are all pursued aggressively by scientists and others.

Often, E.O. Wilson makes comments to the effect that philosophers want to regard humans as special and different than other animals. He thinks that this sort of thinking motivates attempts to explain human behavior in terms of intentional states. This might be true in some cases. But let me offer another motivation that is in line with the goals and values of science. If scientists value the most complete explanations, then they

should seek to give an explanation that involves all the causal factors that contribute to an event. One of the causes of human behavior is intentionality. Given that we have some access to data about intentional states, scientists should include that data in formulating a complete explanation of human behavior. The reason that we include this sort of data when explaining human behavior is not because human beings are special or different from other animals, but rather because we have access to more data on human beings than we do on other animals in this regard. If one is motivated in this way, one can see that accepting anthropological explanations are not incompatible with biological explanations but complementary to them.

However, one may argue that the fact that intentional states are causally relevant does not prove that explanations that involve intentional states are scientific. Merrillee Salmon[13] contends that in order for an explanation to be scientific it must include general causal laws. We turn now to this difficulty.

**Useful Causal Generalizations**

The third criticism of anthropology's status as a sciences stems from a further worry about intentional states. Although many philosophers think that Davidson's argument successfully shows that intentional states do cause behavior, a question about whether this is enough to show that intentional states can be used in scientific explanations still remains. In "Causal Explanations of Behavior," Merrillee Salmon tries to give examples of causal explanations of human behavior that do not appeal to

[13] E.O. Wilson makes a similar point in a roundabout way, in *Consilience: The Unity of Knowledge*. Wilson says that one of the problems with the social sciences is that there ". . .is little effort to explain phenomena by webs of causation across adjacent levels of organization." (189). I will address Salmon's version because it is a clear presentation of the same objection.

intentional states. She is motivated, in part, to do this because she thinks that Davidson's argument for the causal relevance of intentional states is of limited use. The reason she thinks that Davidson's argument is of limited use is the lack of laws connecting intentional states and behavior. Davidson himself thinks that there are no psychological laws connecting intentional states and behavior. He claims that there are no psychological laws because we make sense of mental states by referring to background beliefs and intentions of individuals. But we make sense of physical events in a wholly physical language by reference to laws that connect physical change. Since the explanations involving mental states are not described in physical terms, they cannot be subject to physical laws. Davidson says:

> Too much happens to affect the mental that is not itself a systematic part of the mental. But if we combine this observation with the conclusion that no psycho-physical statement is, or can be built into, a strict law, we have the principle of Anomalism of the Mental: there are no strict laws at all on the basis of which we can predict and explain mental phenomena.[14]

Salmon thinks that the anomalous nature of the mental is the real problem for the social sciences, because even if intentional states cause behavior, explanations that involve them are not scientific since they are not governed by general causal laws—in this case, general psychological laws. Salmon's objection raises two questions: 1. Are there, or can there be, general causal laws that govern the relationship between intentional states and behavior? And 2. Do the explanations that social scientists give need to involve general

[14] Donald Davidson, "Mental Events" in *Essays on Actions and Events*, (Clarendon Press, Oxford; 1980), 224.

causal laws in order to be scientific? Although these questions are connected in important ways, they are independent questions.

Salmon answers to these questions are the basis for her objection. She answers (1) by agreeing with Davidson that there cannot be general causal laws that govern the relationship between intentional states and behavior. She then assumes that in order for an explanation to be scientific it must involve general causal laws. But Salmon claims that cultural anthropology does not give explanations that involve general causal laws, so, cultural anthropology is not scientific. She does not argue for the claim that scientific explanations require an appeal to general causal laws. However, this claim is controversial.

There is an ongoing debate in the philosophy of science over the nature of scientific laws, and over whether or not the social sciences need laws in order to be scientific. Most philosophers agree that laws of nature can be stated using universal generalizations. But they disagree over what kinds of universal generalizations state genuine laws. Carl Hempel, along with Nelson Goodman, has suggested that laws of nature are universal generalizations that can support counterfactual conditionals and subjunctive conditionals. Hempel also thinks that universal generalizations have the unique feature of being able to be used in explanations.[15] Another theory, held by David Lewis and others, is that laws are part of the simplest and strongest deductive system.[16] A competing view, defended by David Armstrong and others, is that laws are relations

[15] For more, see Carl G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, N. J.: Prentice-Hall, 1966), 47–69.

[16] For more, see David Lewis, *Counterfactuals* (Cambridge, Mass.: Harvard UP, 1973).

among universals.[17] Some people, like Bas van Fraassen and Ronald Giere, think that there are no laws.

Just as there are many views about the nature of laws, there are also a number of competing views about whether or not the social sciences (or sciences in general, for that matter) need laws to be scientific. Several options are available: (a) either the social sciences don't need laws to be scientific or they do, and (b) either they don't have laws (Salmon's view) or they do.

In "There are no Laws of the Social Sciences," John T. Roberts argues that although there are no laws in the social sciences, the social sciences do not need to appeal to laws in order to be scientific. Roberts begins by arguing that there are acceptable theories of scientific explanation that do not require citing laws, such as Wesley Salmon's view that involves causal processes and Philip Kitcher's view which focuses on unifying knowledge. Given that there are such theories, one can plausibly say that scientific explanation does not require an appeal to laws. He then goes on to argue that there are no "social laws." Roberts does not think that there are social laws because social laws would have to be ceteris paribus laws, but laws, he thinks, are universal regularities. Although Roberts denies that there are laws in the social sciences, he emphasizes that the lack of laws in the social sciences does not preclude the social sciences from being true sciences.

In "There are Laws in the Social Sciences,"[18] Harold Kincaid's argues for the title claim. Kincaid generally accepts the traditional demarcation criteria for science. But he

[17] For more, see David Armstrong, *What is a Law of Nature?* (Cambridge, Mass.: Cambridge UP, 1978).

finds arguments like Nancy Cartwright's[19] for ceteris paribus laws persuasive. Kincaid uses "the law of supply and demand" as an example of a genuine law in the social sciences. In order to make her argument persuasive, Merrillee Salmon needs to address both Roberts's and Kincaid's arguments for the scientific status of the social sciences.

Whether or not Salmon can make her objection persuasive, we can at least say that science involves more than explaining things with the help of general causal laws. Giving scientific descriptions of phenomena is an essential task of scientists. For instance, scientists must describe the shape and size of wings and the number of legs or leaves an organism has before they can go on to make larger claims about the organisms. Without the large number of descriptions of particular phenomena, the general explanations and descriptions scientists make would not be possible. In addition, scientists look at individual cases in order to find out how the phenomena are caused. For example, when a doctor wants to understand a disease, she looks at individual cases and tries to find abnormalities in order to find out what problems these abnormalities cause. It is through understanding the mechanisms that cause the cells or chemicals in the body to function improperly that they can look for treatments. The individual cases are, in a sense, governed by general physical laws, as are all physical things. But the

---

[18] Both Harold Kincaid's article and John T. Roberts's article can be found in *Contemporary Debates in Philosophy of Science*, ed. Christopher Hitchcock, (Malden, Mass.: Blackwell Publishing, 2004).

[19] In *How the Laws of Physics Lie* (New York: Oxford University Press, 1983), Nancy Cartwright argues that laws in the natural sciences do not describe universal regularities in our actual experience the way they are often claimed to. Instead, Cartwright claims that all laws are really ceteris paribus laws.

explanations of the individual cases do not appeal to these laws in the explanation. If they do, they only do so indirectly and implicitly.

One could say the same thing about anthropological explanations of intentional states. Intentional states are grounded in physical phenomena that are governed by general causal laws. Mental illness demonstrates this point. When you are depressed, or manic, or suffering from any other mental illness, there is some chemical imbalance in your brain that causes you to exhibit certain behavior or even certain attitudes, such as a feeling of worthlessness. When you are depressed you believe that you are worthless even when you are not. These imbalances, which affect your reasoning by changing your beliefs and desires, can be corrected (although this is a complicated and delicate matter which is not yet well understood). The brain and what it causes are subject to general causal laws in the same way as other organs. The brain seems peculiar because one of the things the brain causes is an ability to think and reason, and our reasoning ability in turn causes some of the actions we perform, but oddly, reasoning does not cause all of the actions we perform. What Davidson notes is that there is no regularity between beliefs and desires and particular outcomes. So, for instance, every time you are hungry for something sweet and head to the vending machine you do not always choose the peanut butter cups. There is no regularity between your particular beliefs and desires and the actions those beliefs and desires affect, in the way that insensitivity to insulin causes you to have higher blood sugar. While this may be the case, there is the possibility that when we know more about how the brain works we will understand why this is the case.

Anthropologists look at the intentional aspects of the causes of culture. Since culture is a complex phenomenon with a number of causes, it cannot be denied that there are other ways to understand the existence of culture and cultural phenomena. Even though cultural anthropologists do not generate "causal generalizations," their descriptions are still scientific because they contribute to the general understanding of human behavior by looking at the generating forces for behavior on a small scale. In addition, intentional states are grounded in our biology,[20] so insofar as biological phenomena and their effects are governed by general causal laws, so too are intentional states.

**Conclusion**

Cultural anthropologists attempt to describe and understand the intentional aspects of culture. They look at individual cases and try to explain how they work. Given that intentional states are grounded in biological phenomena, the objection that what anthropologists describe and explain is not governed by general causal laws is not correct. Intentional states are grounded in biological phenomena in the same way that the products of other biological organs are. The causal nature of intentional states is apparent in the way we distinguish intentional action from unintentional action. Finally, anthropologists can overcome the difficulties of dealing with human subjects provided they can come up with methods for detecting and correcting the problems that human subjects present. The demarcation problem might more easily be resolved if we think of

[20] This idea is similar to Davidson's anomalous monism. Anomalous monism is the view that while all events are physical, mental events cannot be reduced to physical events; rather, mental events are dependent on, or in some other way supervene on, physical events. While I agree with Davidson on some issues, my view is not the same as his.

the social sciences as one human science. Sociology, economics, and other branches of the social sciences are informed by cultural anthropology and make generalizations and predictions about cultures or societies. Just as biology, chemistry, physics and other branches of the natural sciences make up a complete natural science, the branches of the social sciences taken together make up a complete social science.

# CHAPTER 3

# ANTHROPOLOGY, NOT SOCIOBIOLOGY OR EVOLUTIONARY PSYCHOLOGY

## Introduction

The problems with the traditional method of anthropology have motivated both the movement toward postmodernism and the movement toward sociobiology and evolutionary psychology in anthropology. Neither of these movements supports the goal of understanding culture in the way anthropologists conceived it. This division in anthropology is founded in a number of philosophical problems. One of these, discussed in Chapter 2, is whether or not the traditional endeavors of anthropology are scientific. The guiding assumption of critics of the social sciences, and of anthropology in particular, is whether intentions, reasons, beliefs and desires are causally efficacious. If they are not then they should not be part of a scientific explanation. But if they are then they have a place in science. I have argued that intentions, reasons, beliefs and desires are causally efficacious and therefore do play a causal role in human behavior. Given that this is the case, anthropological pursuits that involve interviews of individuals should and need to be part of the data collected in an attempt to understand human culture. However, many anthropologists, rather than fix the problems with the method, have adopted the view that interviews and other data that focuses on what the subjects think and believe is irrelevant to understanding culture. They have adopted a philosophical commitment to the claim that intentions, reasons, beliefs, and desires are not causally efficacious, and so can be ignored. In adopting this belief, and the blessing of the hard sciences that comes

with it, some scientifically minded anthropologists have started doing sociobiology and/or evolutionary psychology instead of traditional anthropology.

I will argue that there are two main reasons for rejecting sociobiology and evolutionary psychology as a replacement for anthropology. First, the goals of anthropology are very different from those of both evolutionary psychology and sociobiology. Second, there are independent reasons for rejecting evolutionary psychology and sociobiology.

**The Goals of Anthropology**

One of the main goals of anthropology is to understand human culture. On the list of things that would lead to an understanding of culture are a definition of culture, an objective description of cultures, and an account of the meaning of the objects, social institutions, etc., of the people in that group from their perspective. The definition of culture provides a rough idea of what it is anthropologists are studying. Although anthropologists may not agree on the definition, there is enough agreement to facilitate work in the field. The objective descriptions of culture are the part of an ethnography which records the social arrangements, religious traditions, family structures, physical artifacts, and general daily life. The interviews and the relationships formed with the people one lives with provide and interpretation of the meaning of the activities one observes. The point of interviewing members of the group is to gather an overall sense of what it is like to be a member of a culture. Often many people are involved in this process: individuals, informants, and translators to name a few. From this, the anthropologist assembles a picture of the meaning of the rituals and everyday activities of

the group. The anthropologist comes to make sense of the group as a whole *by* getting to know the individuals in that group. One could not understand a group in the way that anthropologists do simply by looking at trends in the group. That is why statistical explanations and evolutionary explanations must be supplemented with interviews and data gained from participant observation. These are the things that fill in the details of our understanding of culture. Being able to identify both evolutionary pressures and the intentional aspects of behavior is important in both the description and explanation of cultural phenomena. Neglecting either of these things would result in a failure to adequately explain and describe cultural phenomena.

Given that anthropology adds a more intimate look at cultural phenomena to the social sciences by approaching it on the level of individuals, we can see why both sociobiology and evolutionary psychology are inadequate to the task of achieving anthropological goals. Sociobiology is the study of how evolution has shaped human behavior. Sociobiologists explain human behavior in terms of natural selection, group selection, and inclusive fitness. Evolutionary psychology also tries to explain human behavior in terms of evolution. But evolutionary psychologists believe that human behavior is caused by content-specific evolved psychological mechanisms. The focus of their work is to explain specific behavior by reference to a corresponding psychological mechanism. Both of these disciplines attempt to give evolutionary explanations for human behavior. Evolutionary explanations are not explanations of individual behavior. They are explanations of gene frequencies in a population. They may explain why certain genes that cause certain types of behavior are prevalent in a population, but they do not

explain why the behavior occurs in that context. Rather, they explain why it is advantageous for survival in that context. One can see that evolutionary explanations can help to satisfy one part of the goal of anthropology. They help in the description and explanation of the groups propensities. However, it does not help in gaining the intimate knowledge of the individuals in the group and their relationship to the group as a whole needed to give a full description and explanation of a culture in the way required for anthropology. Because of this both sociobiology and evolutionary psychology fail to be good replacements for cultural anthropology.

Anthropology is a legitimate and separate discipline from both evolutionary psychology and sociobiology because these disciplines have different goals and methods. In addition to this fact, we have good, independent, reasons to reject evolutionary psychology and sociobiology. After a brief overview of some of the essential claims of evolutionary psychology and sociobiology, we will turn to some of these reasons.

**Evolutionary Psychology and Sociobiology**

Although sociobiology and evolutionary psychology are not the same discipline, they are very similar in their goals and methods. Some people, including Philip Kitcher, think that evolutionary psychology is just sociobiology by another name. While I am not claiming that they are the same discipline, where they are similar they are subject to similar criticisms. I also want to make it clear that I am not arguing against more moderate views that take facts about evolution to be relevant to anthropology and use discoveries in evolutionary biology to inform the theories of anthropology. I am concerned with evolutionary psychology and sociobiology because they are suggested as

replacements for traditional anthropology. Although sociobiology and evolutionary biology are similar disciplines, evolutionary psychology is different from sociobiology in that evolutionary psychologists are focused on finding the psychological mechanisms that they think cause human behavior. Sociobiologists, like evolutionary psychologists, aim to find and explain the effects of evolution on human behavior. Unlike evolutionary psychologists, they do not limit their research to finding psychological mechanisms. Rather, their aim is simply to explain human behavior in light of selection pressures. Because 'psychological mechanism' is a newer concept than natural selection, we will look at a characterization of psychological mechanisms before turning to the problems with evolutionary psychology and sociobiology.

In *The Adapted Mind*, Cosmides, Tooby, and Barkow describe evolutionary psychology as a discipline concerned with the psychological mechanisms that cause human behavior. In the introduction to *The Adapted Mind,* Cosmides et al. say that evolutionary psychologists focus on "the evolved information-processing mechanisms that comprise the human mind."[1] By doing this they want to illuminate the "necessary connection between evolutionary biology and the complex, irreducible social and cultural phenomena studied by anthropologists, sociologists, economists and historians."[2] They think that culture is caused by the information-processing mechanisms of the human mind. This is because "information-processing mechanisms . . . allow humans to absorb,

[1] Leda Cosmides, John Tooby, and Jerome H. Barkow, "Introduction: Evolutionary Psychology and Conceptual Integration," in *The Adapted Mind: Evolutionary Psychology and the Generation of Culture,* ed. Jerome Barkow (New York: Oxford University Press, 1992), 3.

[2] Ibid, 3.

generate, modify, and transmit culture—the psychological mechanisms . . . take cultural information as input and generate behavior as output."[3] Evolutionary psychologists think this sort of knowledge will be helpful to social scientists because psychological mechanisms "can be expected to cause variation or uniformity in practices, preferences, or modes of reasoning across cultures."[4] Because evolutionary psychology deals with these fundamental psychological mechanisms, Cosmides et al. take these mechanisms to be the "causal core" of the social sciences. They think that since the psychological mechanisms are at the root of all human behavior, evolutionary psychology will provide the best explanations for human behavior.

According to Cosmides et al., the psychological mechanisms we have are collectively a response to the adaptive problems of the Pleistocene environment because humans spent two million years as hunter-gatherers in that environment. Figuring out what the adaptive problems of the Pleistocene were, they think, will help us explain our responses to our current environment. Now that we have some idea of what a psychological mechanism is supposed to do, we can look at some of the problems with explanations that depend on psychological mechanisms, as well as some problems with sociobiology.

**Problems with Evolutionary Psychology and Sociobiology**

*Philip Kitcher's Objections*

[3] Ibid, 6.

[4] Ibid, 6.

In *Vaulting Ambition: Sociobiology and the Quest for Human Nature* (Cambridge, Mass.; MIT Press, 1985), Philip Kitcher provides many good arguments against sociobiology, and to a lesser extent, against evolutionary psychology. One of Kitcher's main complaints against evolutionary psychology and sociobiology in *Vaulting Ambition* is that evolutionary psychologists and sociobiologists do not follow the standards of good science that other evolutionary scientists do. In particular, their explanations of human behavior do not rule out other plausible explanations. In a more recent article entitled "Giving Darwin his Due," Kitcher focuses his criticisms on evolutionary psychology. The main criticisms are as follows. First, the evolutionary psychologist's commitment to the idea that there is a psychological human nature "is at odds with the modest Darwinian theme of anti-essentialism—indeed, evolutionary psychology is dominated by a tendency to write as if frequency-dependent selection and polymorphism didn't exist."[5] Cosmides, Tooby, and Barkow explicitly state that one of their goals is to uncover human nature. The anti-essentialism that Kitcher refers to is the fact that there are no static characteristics of an organism, and so there can be no unchanging human nature. Second, evolutionary psychologists do not carefully formulate mathematical models and they ignore the possible role of cultural transmission. Kitcher makes this same criticism against sociobiology in *Vaulting Ambition*. Third, often their conclusions are based on assumptions about the "savannah environment" that are not very well supported and "about the ways in which phenotypic traits are linked together."[6] The problem that

[5] Philip Kitcher, "Giving Darwin His Due," *Columbia University*, http://www.columbia.edu/~psk16/darwin.htm.

[6] Ibid.

Kitcher is addressing here is the fact that the way phenotypic traits work together to increase fitness is a complex process, and yet evolutionary psychologists often talk as if we can clearly identify specific advantages of a particular trait.

I would like to add that sometimes evolutionary psychologists talk as if understanding the adaptive problems of the Pleistocene will give us all the information we need to interpret behavior in the present. But most evolutionary biologists agree that evolution can take place over a long period of time, a geologically short period, or very rapidly. If this is the case, simply looking at the Pleistocene for information about adaptive problems could be misleading.

In addition to these main objections, Kitcher provides a sustained argument against sociobiology in *Vaulting Ambition*. We turn now to some of my own objections.

*What exactly are psychological mechanisms?*

The picture we get of psychological mechanisms, from the introduction to *The Adapted Mind*, is that of a specific structure in the brain which causes behavior, or which makes certain types of behavior possible. As Cosmides and Tooby say in "The psychological Foundations of Culture,"

> Thus human design resolves itself into two primary tiers: First, an encompassing functional superstructure of virtually universal, complexly articulated, adaptively organized developmental, physiological, and psychological mechanisms, resting on a universally shared genetic basis; and, second, low level biochemical

> variation creating usually slight individuating perturbations in this universal design due to the existence of a reservoir of genetic variability in the species.[7]

My understanding of the causal chain represented by this description is that we have genes that are responsible for the general creation of the brain as well as the creation of specific formations in the brain, the psychological mechanisms. I take it that these specific formations are physical structures made up of an arrangement of brain matter. These structures, according to Tooby and Cosmides, dispose people to exhibit certain behavior in the right circumstances. Psychological mechanisms, then, when put in the right environment, will cause a particular behavioral response. Without those particular structures the behavior would not occur, and without particular environmental cues the behavior would not occur. The mechanism, I take it, "lies dormant" when not confronted with the environmental factors that would "turn it on."

Tooby and Cosmides often describe psychological mechanisms as dispositions to behave in certain ways or to detect certain things. But they also insist that the psychological mechanisms are the main cause of particular behavior. These things are incompatible because dispositions do not cause behavior. Rather, the thing that causes the behavior is what causes the dispositions. So in this case the physical structure, which is caused by genes, is the cause of the behavior. Frank Jackson has clearly articulated the idea that dispositions are not causes. In *From Metaphysics to Ethics,* he says:

> Dispositions are not causes, and in particular, are not causes of their manifestations. Their categorical bases do all the causing, where by the

[7] Leda Cosmides and John Tooby, "The Psychological Foundations of Culture," in *The Adapted Mind: Evolutionary Psychology and the Generation of Culture,* ed. Jerome Barkow, (New York and Oxford: Oxford University Press, 1992), 80.

> categorical basis of a disposition in some object, I mean the property of the object responsible for its having the disposition; that is, the property that is responsible for the object's being disposed to behave in the way definitive of the disposition in question. Consider, to illustrate the point, a fragile glass that shatters on being dropped because it is fragile, and not (say) because of some peculiarity in the way it is dropped. Suppose that it is a certain kind of bonding *B* between the glass molecules which is responsible for the glass being such that if dropped , it breaks. Then the dispositional property of being fragile is the second-order property of having some first-order property or other, bonding *B*, is the categorical basis of the fragility. But then it is bonding *B*, together with the dropping, that causes the breaking; there is nothing left for the second-order property. . .the disposition itself, to do.[8]

Tooby and Cosmides, however, seem to think that understanding the psychological mechanisms, which they take to be dispositions, will make it possible to causally explain human behavior. They refer to evolutionary psychology as "the causal core of the social sciences." But if dispositions are not causes, then either psychological mechanisms are not dispositions or they are not causally relevant. Because Tooby and Cosmides put so much emphasis on the causal nature of psychological mechanisms, the most charitable way to state this objection is that evolutionary psychologists are not focused on the right

[8] Frank Jackson, *From Metaphysics to Ethics: A Defense of Conceptual Analysis*, (New York: Oxford University Press, 1998) 91–92.

things. Rather than focus on explaining *behavior* in terms of psychological mechanisms, they should first focus on explaining the psychological mechanisms themselves.[9]

The reason we need to know how the psychological mechanisms themselves evolved is because it is only when we know what causes the psychological mechanism, and how and what effect the environment plays in its development, that we will be able to say what causal role the psychological mechanism plays in behavior. We need to know the range of a psychological mechanism's flexibility. Genes limit the range of possibilities that can occur but they do not, in a straightforward way, determine the structures that are produced. So without an understanding of the range of a psychological mechanism, evolutionary psychologists will not be able to come up with explanations that rule out other plausible theories. This is because you need to be able to say that it was the psychological mechanisms, and not another factor or another combination of factors, that produced the behavior.

Until we have a well-articulated explanation of the psychological mechanism and the range of behaviors they support, as well as the role of the environment, we will not be able to give causal explanations of human behavior based on psychological mechanisms. If evolutionary psychologists focused on how genes and the environment cause psychological mechanisms, they would be one step closer to correctly articulating the causal role of psychological mechanisms in human behavior.

*Rationality*

[9] Kitcher briefly makes a similar point in *Vaulting Ambition* (307) when he says, "Their [the evolutionary psychologists] discussions are askew because they shed no light on what really needs evolutionary explanation— to wit, the proximate mechanisms."

Cosmides and Tooby do not think that many of the concepts that anthropologists use to explain certain aspects of culture are explanatory. The reasoning Cosmides and Tooby give is evidenced in the claim, "Of course, as most cognitive scientists know (and all should know) 'learning'— like 'culture,' 'rationality,' and 'intelligence'— is not an explanation for anything, but is rather a phenomenon that itself requires explanation."[10] The argument in this passage, put simply, is that things like "culture," "rationality," and "intelligence," which might be used to explain behavior, cannot be used to explain behavior *because* those things are themselves in need of explanation. I wish to focus on rationality here because I think that many of the things that are the result of rationality, like reasoning to a conclusion, can be explanatory.

While Cosmides and Tooby are not addressing rationality in this paragraph, they do mention it in the list of things that do not have explanatory power. The reason they give for the claim that the things on this list cannot explain anything is simply that they themselves need to be explained. However, the mere fact that something needs to be explained does not show that it is not explanatory. For example, if you are trying to explain why an accident involving break failure occurred, you can explain the accident by noting that the brakes failed, even though why the brakes failed is in need of an explanation. The mere fact that something needs to be explained itself does not rule out the possibility that it is explanatory. The same sort of argument would be true of the other concepts on the list. Before such concepts can be dismissed as lacking all explanatory

[10] Ibid, 122.

power, Cosmides and Tooby would need to supply a better argument for this conclusion in each case.

*My Biological Clock is Ticking But I'm Not Listening*

Evolutionary psychologists claim that psychological mechanisms are content-specific instincts. They also claim that they are the most important causes of much human behavior. This includes things such as mate choice, play, and many other things. One clear instance of an instinct is that of our strong desire to reproduce. Many women have experienced the urgent desire to have children as they reach their late twenties and early thirties. This feeling is sometimes referred to with the expression "my biological clock is ticking." The urgent desire to have children can be very strong, and yet if one is not in a good position to have children, and one is thinking clearly about one's situation, one can decide not to have children. One can thwart the efforts of one's biological instincts by using birth control. If reasoning, given relevant information, can prevent instincts from doing what they are supposed to do, then this would be some reason to think that they are causally relevant and thus, heavily explanatory in some situations. If reasoning can interfere with one of the strongest survival instincts we have, then it seems plausible that reasoning can make a difference in human behavior even if there are content-specific instincts that pull at us to behave in a particular way. If this is the case, understanding the role of reasoning in human behavior is an important part of explaining that behavior.

*What do evolutionary explanations explain?*

Anthropologists, and social scientists in general, try to gain a certain kind of understanding of human behavior. Anthropologists want to understand the beliefs of a

group. They want to understand why people do the things they do and what those things mean to those people. Similarly, psychologists also want to understand their subjects' beliefs, desires, intentions, and feelings so that they can help individuals deal with particular problems. To say that we can understand what people are doing in terms of their beliefs suggests that we can explain what they do in terms of their beliefs.[11] Although this is not the only kind of explanation and understanding anthropologists seek, it is a very important part of what they do.

The problem with replacing, discounting, or de-emphasizing these sorts of explanations in favor of evolutionary explanations is that evolutionary explanations do not explain, and do not attempt to explain, the same things. Explanations that involve the concept of inclusive fitness do not explain why the behavior occurs. Rather, they articulate a consequence of that behavior. Unless there is a gene that directly causes a particular behavioral result, inclusive fitness is not an explanation of that behavior. Natural selection and inclusive fitness explanations do not and are not intended to explain individual behavior. As Andrew Ariew argues in "Ernst Mayr's 'Ultimate/Proximate' Distinction Reconsidered and Reconstructed":

> Proximate causes of individual life histories vary between individuals in a population. No two individuals live and die in exactly the same way or necessarily give birth to the same number of individuals. So, after tracing out proximate causes for every individual in a population, there is something left over to explain, namely what some of these disparate life histories have in

[11] In the next chapter I will defend the claim that explanations in terms of beliefs, desires, and intentions are real explanations that should be a legitimate part of the social sciences.

> *common* that set them apart from their conspecifics. Evolutionary explanations identify these commonalities in terms of statistical properties of an evolving *population*. Hence, evolutionary explanations differ *in kind* from proximate explanations. Evolutionary explanations are statistical; they range over the ensemble of individuals, taken as a class. Proximate explanations are individual level causal explanations ranging over individual life histories. [12]

Anthropologists study both individual behavior and group behavior. The goal of understanding culture involves understanding how and why certain traditions came to be and what choices were made by individuals that changed the group or established traditions. To adopt methods that are not designed to produce this sort of data would thwart the main goal of anthropology. Just as statistical data on the prevalence of disease in certain populations does not explain why the individuals have the disease, or what causes the disease, it takes studying both the disease itself and the people that have it to understand why it is prevalent in the population; so too is it necessary for anthropologists to go beyond the statistical and evolutionary explanations to form a good understanding of culture.

**An Example of Evolutionary Psychological work: "The Evolution of Sexual Attraction: Evaluative Mechanisms in Women"**

To get an idea of how these criticisms apply to examples of work in evolutionary psychology, I want to take an example from *The Adapted Mind*. Tooby, Cosmides, and

[12] Andre Ariew, "Ernst Mayr's 'Ultimate/Proximate' Distinction Reconsidered and Reconstructed," *Biology and Philosophy* 18 (2003): 561.

Barkow take the essays in *The Adapted Mind* to be good, textbook examples[13] of the products of their discipline. I have chosen to discuss one article from the collection by Bruce J. Ellis entitled, "The Evolution of Sexual Attraction: Evaluative Mechanisms in Women."

Ellis tries to provide evidence that women have a psychological mechanism that *causes* them to be sexually attracted to "males of high mate value." Since this is an evolved mechanism, it causes women to choose men with characteristics consistent with high mate value in a hunter-gatherer of the Pleistocene. Ellis begins providing some possible answers to the question, "What traits would have been correlated with high male mate value in our natural environment?"[14] Ellis narrows the answer to this question down to three possibilities: 1. A man's ability and willingness to provide food or economic support to his wife and children. 2. A man's ability and willingness to protect his wife and children. 3. A man's ability and willingness to care for and socialize his children. Although he claims to narrow down the answer to these three possibilities, he acts as if all three together are important. Ellis claims that, "To the extent that males in the Pleistocene differed in their propensity and ability to provision their mates and children, and to the extent that this variation was signaled by observable cues, selection would have shaped female choice in favor of males who displayed such cues."[15] He does not go

---

[13] In fact *The Adapted Mind* is often used as a textbook.

[14] Bruce J. Ellis, "The Evolution of Sexual Attraction: Evaluative Mechanisms in Women," in *The Adapted Mind: Evolutionary Psychology and the Generation of Culture*, ed. Jerome Barkow, (New York: Oxford University Press, 1992), 268.

[15] Ibid, 268.

on to give evidence that each of these things can be and is signaled by observable cues. Rather, he assumes that they are signaled by observable cues and begins to discuss these cues. He thinks that social and economic status, ornamentation, character traits and a variety of personality traits are observable cues of mate value. Ellis provides evidence from a number of studies to try to show that women are sexually attracted to men that have high social and economic status, that dress well, that are of better than average height, that are physically dominant, that have character traits like ambitiousness and expressiveness, and that are older but not too old.

There are a number of problems, consistent with the general problems with evolutionary psychology, with this article. First, as the title suggests, Ellis is supposed to be discussing psychological mechanisms for sexual attraction in women. However, Ellis uses the terms 'attracted' and 'sexually attracted,' throughout the article, and the problem is that he does not mean the same thing by the terms in every case. Sexual attraction is a physical response. Attraction may or may not be physical. In some cases, Ellis assumes that a woman's willingness to have sex with a man is an indication of sexual attraction. While this is not a completely unwarranted assumption, it is not always the case that a willingness to have sex is accompanied by a physical sexual attraction. What Ellis needs to show was that women feel *sexual* desire for men with high status, money, and the relevant personality traits, not just that they were willing to have sex with men of this sort.

Second, in the section on ornamentation, Ellis says that the psychological mechanism would be, "Prefer ornamentation that signals high status in my culture."[16] But why would this be the content of the psychological mechanism? Psychological mechanisms are supposed to be content-specific, and although this does have a specific object (high status ornamentation) it is not specific enough. Evolutionary psychologists think that the evolved mechanisms that we now have evolved in response to the problems of the Pleistocene. They think this because we spent a very long time as hunter-gatherers (two-million years). This fact, in conjunction with the evolutionary psychologist's belief that we do not have a general logic but rather content-specific mechanisms, should lead to conclusion that what we have is a psychological mechanism to prefer the ornamentation that signaled high status in our hunter-gatherer ancestors.

Third, in his discussion of physical dominance, Ellis says that women could have a mechanism to detect a male's ability to win in a physical fight either by sheer force, or by coercion and intimidation by their physical presence. It is plausible that in the Pleistocene environment, sheer physical strength and ability to win fights would be the clear observable cue of a man's ability to provide protection, and also perhaps of his status. But this is in conflict with other evidence Ellis cites in his article. He often points out that women are not as attracted to a man's physical appearance as they are to men with the other features he discusses. Clearly in our evolutionary environment men that were strong and able to intimidate others with their physical stature would make better

[16] Ibid, 271.

protectors. So physical characteristics that indicate physical strength should be very important to women, but according to Ellis's own research they are not.

Fourth, in both his discussion of physical dominance and his discussion of body language, Ellis sets up false dilemmas. In the section on physical dominance, he uses Daly and Wilson's categories for men. They claim that men are either the kind that "don't take any shit" or the kind that "can be pushed around."[17] However, it seems to me that there are many categories in between these that many men would fit better in. In the section on body language, he says that women put men into categories of either high dominance body language or low dominance body language. But, again it seems possible that there are other categories. Ellis needs to provide evidence to rule out the other possible categories, which he does not. Until he does, categories like these, and these in particular, are false dichotomies.

Fifth, although there are a few instances when Ellis tries to rule out other theories, there are at least two major cases where he does this unsuccessfully. The first theory he addresses is an alternative explanation of why women seek men with high status, money, etc. This theory, called the structural powerlessness theory, says that women want men with those characteristics because they can provide the women with access to things women cannot get for themselves due to social inequality. Proponents of the structural powerlessness theory think that the differences between men and women in mate choice preference are socially conditioned. Ellis thinks that this theory leads to the testable hypothesis, "Women with access to power and wealth will act and feel sexually more like

[17] Ibid, 275.

men (much the same as men, in fact) than women who are comparatively powerless and poor."[18] Ellis takes this hypothesis to be refuted by a study done on female medical students and leaders of the feminist movement. The women in this study preferred men that were well-off and powerful. In addition, the medical students wanted to pursue only relationships that could lead to marriage. From this, Ellis concludes that the social powerlessness theory has been refuted. However, Ellis assumes that a woman who has money and a good job makes her equivalent in power to a man in the same situation. But that is only the case if women in general are equal to men in society, which they are not, no matter their wealth and position.

The second theory Ellis addresses is in the section of the paper on height preference. Ellis argues that women prefer tall men. The alternative theory, called the central tendency theory, claims that women prefer men that fall near the midpoint in the population distribution. Most of the data Ellis provides supports the central tendency hypothesis. Ellis himself, at important points, claims that the evidence better supports the central tendency hypothesis. If the data supports his theory at all, it is that height has something to do with attractiveness, but it is not the central cue Ellis claimed it is. And yet, at the end of the section, he concludes that the evidence is too sparse, and perhaps contradictory, to favor one hypothesis over the other.

Finally, almost all of the features Ellis discusses are features that would demonstrate a man's *ability* to provide for, and protect a woman and her children. Only one of the characteristics he discusses addresses a man's willingness to do these things

[18] Ibid, 273.

for a woman, and in this case only indirectly. However, it seems equally important, if not more important, that a women be able to detect a man's willingness to support her as it is for her to detect his ability to support. If a man is not willing to support a woman, then his skills, strength, and personality do not benefit her chances of survival at all. It seems that half of the psychological mechanisms are missing from Ellis's research.

What Ellis's article demonstrates is that evolutionary psychologists, even in textbook cases, fail to avoid major pitfalls to doing good scientific research. Ellis makes unwarranted assumptions, and fails to adequately rule out other theories.

**Conclusion**

If there are such things as psychological mechanisms which are evolved features of the human mind, this information would be helpful to anthropologists for understanding similarities among cultures. Knowing what all human beings have in common can help anthropologists figure out how and why cultures have certain social, economic, and familial structures in common. However, it could never replace what anthropologists do. Evolutionary psychologists and sociobiologists do not do what anthropologists do. The complex causal story that explains human behavior is not simply told by evolutionary psychologists or sociobiologists. Anthropologists tell one part of the causal story, while biologists tell another part– neither is dispensable.

Anthropologists pursue a unique set of features of the human experience that cannot be explained or understood simply in terms of statistical trends or the standard features of evolutionary explanations. This fact presents special challenges both to anthropologists and philosophers of science because it challenges some standard beliefs

about what counts as a scientific explanation. In the next chapter I will consider these issues in detail.

# CHAPTER 4

# REDUCTIONISM, ORGANIZATION, AND THE SOCIAL SCIENCES

Reductionism threatens the goals, methods, autonomy, and explanations of the social sciences in at least two ways. First, some reductionists, such as E.O. Wilson, argue that the social sciences should adopt the methods of the natural sciences and focus on explaining only those aspects of human behavior that can be investigated using the methods of the natural sciences. Doing this would wholly, or partially, eliminate the goals, methods and unique subject matter of the social sciences. The second threat results from attempting to organize the social sciences using a reductionist hierarchy. If the social sciences are structured using a reductionist hierarchy, their explanations will inevitably compete with each other for explanatory primacy, resulting in a disunity which is counterproductive. Both concerns threaten the autonomy of the social sciences. If the social sciences explain only what can be explained by the methods of the natural sciences, they will need to change their goals which will change the subject matter of the social sciences altogether. If there is a reductionist hierarchy among the social sciences, some think, that would mean that some social sciences are dependent on others in a way which undermines their autonomy. What I hope to show is that neither of the reductionist concerns is a genuine threat to the goals, methods, explanations, or autonomy of the social sciences.

## Reductionism: Friend and Foe

Reductionism is the notion that if a higher-level science can be deduced from a lower-level science, then the higher-level science can be reduced to the lower-level science. Reductionism can have as its object the theory and methods of a science or it can describe an ontological relationship. Reductionism in the natural sciences is perceived as helpful because it connects the natural sciences to each other and it creates a hierarchy among them. Whether the higher-level sciences can in fact be completely reduced to physics does not affect the perception that there is this relationship among the natural sciences. This perception, among others, unites the goals and methods of the natural sciences. In the social sciences however, reductionism more often divides the social sciences and pits them against each other.[1]

Reductionism presents at least two threats to the social sciences. The social sciences attempt to describe and explain human behavior at the individual and group level while taking into account the cultural context. The first threat reductionism presents is to the goals and methods of the social sciences. Sometimes reductionists claim that the social sciences would be better at predicting and explaining behavior if their methods and goals were strongly shaped by biology and neuroscience. Reductionists think that while the social sciences can explain things at a higher level of generality those explanations will not be substantial enough to produce theory that is based on facts that would provide a framework for deeper explanations in the social sciences. Rather they think social scientists should look to biology and other natural sciences to find the building blocks for

[1] This characterization of the role of reductionism in the social sciences is based on information in "Functionalism and Macrosocial Science," Chapter 5 of Alexander Rosenberg, *Philosophy of Social Science* (Boulder, Colo.: Westview Press, 2008).

their theories and explanations. Reductionists think that the higher levels of generality can be reduced to their parts and, sometimes, their relational properties at the lower levels. The reductionist view leads some to criticize the social sciences for attempting to explain human behavior without using the lower levels as the basis for their theoretical claims. Research programs such as sociobiology and evolutionary psychology represent attempts to use the reductionist framework to redirect the goals and methods of the social sciences.

The second threat from reductionism comes from within the social sciences. Since reductionism does not unify the goals and methods of the social sciences, each social science has a set of goals and methods that might be incompatible with the goals and methods of the other sciences. Although all the social sciences very generally aim at explaining human behavior, they focus on different kinds of behavior. Sociology, for example takes societies and institutions as its subject, while psychology is more concerned with the normal and abnormal characteristics of individuals, and cultural anthropology looks at both groups and the individuals in a cultural context[2]. Each science wants to establish its autonomy which leads to struggles over which science is foundational. There is no agreement about whether there is a reductionist hierarchy among the social sciences, and if there is a hierarchy, reductionist or not, which social science is most basic. What is at stake for each social science is causal and explanatory primacy. It is not in the interest of a social science like sociology to be reduced to

[2] The characterizations of sociology, psychology and anthropology are not meant to be definitions. I am just making the point that although they all study human behavior, they focus on different aspects of behavior and on groups and individuals to greater and lesser degrees.

psychology because what sociologists explain is group behavior which is a unique area of study, some argue[3], only if a group is not just a sum of its parts. Theoretical disputes, such as the one over methodological individualism[4], reflect this internal tension among the social sciences.

In what follows I will argue that reductionism does not provide a reason for preferring the goals and strategies of sociobiology and evolutionary psychology over the more traditional strategies of the social sciences. In other words, reductionist considerations are not a reason to change the subject. The internal threats that arise from reductionism are more pressing and complicated. I will argue that a reductionist hierarchy is not needed to achieve coherence in the social sciences, but that they do need a coherent organizational structure. The explanations in the social sciences are causally integrated[5] with each other in a way that makes trying to order the explanations of the social sciences in a reductionist hierarchy inappropriate. However, understanding how the explanations

---

[3] Emile Durkheim, for example.

[4] Alexander Rosenberg, in *The Philosophy of Social Science*, defines methodological individualism as follows: "[A]ny example given of a social fact is best described or fully explained in terms of the behavior of individuals or is not a fact of any kind, but a figment of the holist's imagination or research program" (142). Daniel Little, in *Varieties of Social Explanation* (Boulder, Colo.: Westview Press, 1991), defines methodological individualism as "the view that social explanations and descriptions must be grounded in facts about individuals. This doctrine consists of several related but distinct claims: a thesis about social entities, a thesis about the meaning of social concepts, and a thesis about explanation" (183).

[5] What I mean by this is that there are compositional relationships between the social sciences which make some social sciences causally integrated with others. For example, one might say that individual psychology is compositionally related to the psychology of groups, and the social conditions in which individual and group behavior occur are background conditions for that behavior. This is only a suggestion of how explanations in the social sciences might be causally integrated. How they actually are is, I think, a major project for theorists in the social sciences.

in the social sciences are related to each other is necessary for progress in the social sciences.

**Changing the Subject**

Naturalistic anthropologists, as well as some biologists, want to establish a connection between the social sciences and the natural sciences by appealing to reductionism, or at least the possibility of reductionism. The appeal to reductionism is motivated by the desire to ensure that explanations in the social sciences are causal. E.O. Wilson puts it this way:

> The social sciences possess the same general traits as the natural sciences in the early, natural history or mostly descriptive period of their historical development. . . . But if natural history by whatever name is the foundation of all the sciences, why is it not yet theory? The main reason is that it includes little effort to explain phenomena by webs of causation across adjacent levels of organization.[6]

Wilson is claiming that in their current state the social sciences are not yet sciences because they do not explain human behavior in a causal way, they merely describe it. In order to give explanations "across adjacent levels of organization," Wilson suggests that the social sciences increase their connection to biology. He says about economics:

> The full understanding of utility will come from biology and psychology by reduction to the elements of human behavior followed by bottom up synthesis, not from the social sciences by top-down inference and guesswork based on

[6] E.O. Wilson, *Consilience: The Unity of Knowledge* (New York: Alfred A. Knopf, 1998), 188–189

> intuitive knowledge. It is in biology and psychology that economics and other social sciences will find the premises needed to fashion more predictive models, just as it was in physics and chemistry that researchers found premises that upgraded biology.[7]

Wilson suggests at many points in his book *Concilience* that the social sciences need to deepen their connection to biology and more actively use biology in their explanations of human behavior and that a failure to do so will keep the social sciences from developing any further. Wilson claims that reduction from the social sciences to biology will improve the explanations in the social sciences because it will establish a causal connection across levels of organization. Certainly, there is a robust connection between biology and the social sciences, and discoveries in evolutionary biology, neuroscience and other branches of the natural sciences will provide important information that should shape the explanations of social scientists. In this way biology can provide "premises that upgrade" the social sciences. However, there is a significant difference between using discoveries in biology and other natural sciences to inform theory creation and explanation in the social sciences and the idea that a full understanding of concepts, theories and explanations will *come from* biology and other natural sciences. While the natural sciences can inform the theories and explanations of social scientists, they will not be able to provide a full understanding of the theories and concepts of the social sciences because the domain of biology does not cover all of the phenomena that social scientists study. For example, biology does not address the social behavior of human being in a

[7] Ibid, 206.

cultural context. Reductionism cannot play, in any simple way, the role that Wilson suggests it should and will.

There is no unproblematic reduction between the social sciences and biology or some other branch of natural science. Biological considerations and principles will only help to explain certain kinds of claims about human behavior; the domain of the social sciences extends beyond these claims. One way to see this is to compare the subject matter of explanations in the natural sciences to those in the social sciences. In *Forms of Explanation*, Alan Garfinkel puts the requirements for reduction nicely:

> So in order to assess a claim of reduction, we need a notion of when two or more explanations are explaining the same thing. . . . In particular we can say that if the reduction is to be successful, the two explanations must have the same *object*. This means that they must be about the same phenomena and also that they must construe the problematic in the same way. Not only must they be talking about the same thing but they must have contrast spaces that line up in the right way. Otherwise the reduction will fail.[8]

I agree with Garfinkel that if a reduction does not meet these criteria it will fail. The point of reducing one explanation to another is to replace the first explanation with a more thorough explanation. If the object of the explanation is not the same then the alternate explanation will not give a more thorough explanation; it will give an explanation of something else entirely. Generally speaking, the explanations given by biologists and sociobiologists do not have the same object as explanations given by anthropologists, for

[8] Alan Garfinkel, "Reductionism" in *The Philosophy of Science* ed. by Richard Boyd, Philip Gasper, and J.D. Trout (Cambridge, Mass.: The MIT Press, 1991), 444.

example. Although anthropology, biology and sociobiology aim at understanding human behavior, the contrast spaces do not line up in the right way. Each group asks questions about different things. Biology and sociobiology aim at understanding human behavior in the context of evolutionary history. Questions about our evolutionary history include large stretches of time and are about human behavior as a *species*. Biologists do not focus on the behavior of particular humans or even small groups of humans. Anthropology, on the other hand, looks at both the behavior of specific human beings and small groups of human beings. The foci of an anthropologist's questions are much narrower than a biologist's. Anthropologists focus on stretches of time that are microscopic compared to the stretches of time studied by biologists. Anthropologists ask questions like, "Why do girls in this cultural tradition get married by the age of 14?" Sociobiologists ask questions like, "What evolutionary pressures make marriage advantageous?" Biologists don't ask questions about marriage. This demonstrates that biology, sociobiology, and anthropology do not have the same object.

In addition to having a different object, biological explanations are not more complete than anthropological explanations. Granted, they contain information that anthropological explanations do not contain. However, if all we had were biological and/or sociobiological explanations, we would have a seriously incomplete picture of the human organism. A complete explanation of individual human behavior would cite many factors, including biological processes and chemical reactions in the brain, a person's interaction with the resources in the environment, a person's interactions with other people, the influence of a person's cultural context, and the influence of social forces on

the person, as well as the relations between each of these factors and many others. The biological factors which contribute to individual and group behavior are important, relevant, and in many ways informative, but they will not on their own provide a more complete explanation than those provided by the social sciences. Wilson seems to believe that reductionism will clarify murky explanations in the social sciences, but this is not the case. Biological explanations are, like social explanations, one part of a large set of explanations that make up a complete explanation of human behavior. The explanations provided by the natural and social sciences are connected to each other. Our evolutionary history and our neurological makeup are background conditions for our ability to have culture. And there is research showing that culture influences how our brains work.[9] So there is much to be gained by a thorough explication of the relationship between the biological causes of human action and the intentional action that many social scientists study. Clarifying that relationship will be very helpful, but it is not a panacea for all the theoretical and methodological problems that the social sciences face.

The social sciences cannot simply adopt the methods of the natural sciences. Since the subject matter of the social sciences includes intentional action that takes place in a relatively short time span, the social sciences must have their own methods for collecting and evaluating this sort of data. Evolutionary explanations will not do the job. The social sciences can adopt some of the methods of the natural sciences and use them successfully, but where their data is different in kind from the natural sciences, different

[9] For example, see John Gabrieli et al., "Cultural Influences on Neural Substrates of Attentional Control," *Psychological Science* 19 (2008): 12–17. In this article, Gabrieli et al. claim that Easterners and Westerners use different parts of their brain when solving problems. The explanation for the difference, they claim, lies in the general cultural values of the participants.

methods will have to be developed and employed. There is no doubt that biology and neuroscience will provide part of the explanation of human behavior, but they will not completely explain human behavior. The social sciences provide explanations at a different level of generality than the natural sciences. Therefore, they will inevitably explain things that are untouched by biology and neuroscience; indeed, such things may have only a tangential connection to these subjects. For all the above reasons, the social sciences should not be threatened by the idea that they could and should use what the natural sciences have to offer to support their work.

What the natural sciences have to offer cannot explain the wide range of social phenomena that the social sciences encompass. Social scientific explanations cannot be reduced to biological explanations in any straightforward way, that is if they can at all, so whatever benefit reductionism confers on the social sciences is not in providing better explanations. Rather the benefit of the connection with the natural sciences is in finding out how explanations in the social sciences fit with those in the natural sciences and using what the natural sciences have to offer to inform and shape theory in the social sciences.

**Folk Psychology**

Another problem for social scientists that is related to reductionist concerns is the use of folk psychology in theoretical endeavors and in the collection of data. Critics like Wilson think that the use of folk psychology by social scientists leads to a lack of scientific rigor which is detrimental to progress in the social sciences. The connection this criticism bears to reductionist concerns is that some reductionists suggest that the solution to the problems with folk psychology is to abandon it in favor of the methods of biology

and scientific psychology. In addition to this criticism, some philosophers, such as Churchland, Stich, and others, criticize the use of folk psychology on the grounds that that the intentional states that folk psychology posits do not actually exist, and therefore folk psychology cannot be reduced to theories in the natural sciences. They believe that these concerns, as well as other philosophical problems, make folk psychology useless to explanation in the social sciences. I argue that neither Wilson's criticisms nor Churchland's criticisms provide sufficient reasons to abandon folk psychology altogether, but they do illuminate an area for theoretical development in the social sciences.

Wilson rejects the use of folk psychology in the social sciences by focusing on the practical problems that using folk psychology creates. As Wilson says:

> Familiarity bestows comfort, and comfort breeds carelessness and error. Most people believe they know how they themselves think, how others think too, and even how institutions evolve. But they are wrong. Their understanding is based on folk psychology, the grasp of human nature by common sense . . . shot through with misconception, and only slightly advanced over ideas employed by the Greek philosophers. Advanced social theorists, including those who spin out sophisticated mathematical models, are equally happy with folk psychology. As a rule they ignore the findings of scientific psychology and biology.[10]

Wilson makes a number of claims which need to be examined. The first criticism is that social scientists should not use folk psychology because it leads to serious errors. Second, Wilson claims that all folk psychology amounts to is a common sense understanding of

[10] Wilson, 183.

human nature. Third, Wilson claims that social scientists "as a rule" do not pay attention to, or use discoveries in, biology or scientific psychology to inform their work.

Let's take a closer look at Wilson's first claim: social scientists should not use folk psychology in their research because it makes them prone to making mistakes. Wilson is not specific about what kind of mistakes he is worried about, but he does seem to imply that our common sense understanding of "how others think, and . . . how institutions evolve" are false because social scientists use folk psychology to understand these things. What he might mean is that because folk psychology is "shot through with misconception," the explanations given using folk psychological terms will be false because they do not involve precise concepts and true causal claims. While Wilson does not elaborate on his concerns, they seem similar to objections raised by Churchland. Churchland does not think that the intentional categories of folk psychology such as beliefs, desires, etc. can be lined up with the concepts of neuroscience in a way that would facilitate the reduction of folk psychology to neuroscience. He says, "A successful reduction cannot be ruled out, in my view, but FP's explanatory impotence and long stagnation inspire little faith that its categories will find themselves reflected in the framework of neuroscience."[11] The main theme of Wilson's book *Concilience* is to point out that the success of the natural sciences comes from the interconnectedness that reductionism provides, and thus he recommends that the social sciences would be more fruitful if they made their connection to the natural sciences through reduction explicit

[11] Paul M. Churchland, "Eliminative Materialism and Propositional Attitudes," in *Folk Psychology and the Philosophy of Mind*, ed. Scott M. Christensen and Dale R. Turner (Hillsdale, NJ: Lawrence Erlbaum Associates, 1993), 49.

and clear. Churchland's concerns are similar in that he suggests that folk psychology is fruitless and, more strongly, that it is false because of our inability to reduce it to neuroscience.

The standard that is being used to dismiss folk psychology is that a science can be fruitful only if it can be reduced to a lower-level science. If this is the standard one wishes to apply, opponents of folk psychology will get more than they bargained for when this same standard is applied to biology. Wilson and others seem to think that all the natural sciences are reducible to physics, but it is not clear that they are. Biology in particular uses concepts, like natural selection, that do not line up with the concepts at lower levels in a way that facilitates reduction.[12] If the fact that some biological concepts are not reducible to lower levels is not a reason to mark them as radically false, then it should not be a reason to think that folk psychological concepts are radically false. Just because some concepts are not reducible does not show that they are not in some more general way connected to lower levels of organization. Although evolutionary biology is not reducible to molecular biology, the discoveries of molecular biology are influential. This suggests that even if the higher and lower levels of biology are not connected by reduction, they are still in an appropriate hierarchical relationship. The same thing can be said about higher levels of organization such as those of the social sciences and those of biology. The inability to reduce evolutionary biology to molecular biology tells us something important about the nature of the concepts of evolutionary biology, and likewise for the social sciences. The concepts of evolutionary biology apply to complex

[12] For detailed examples, see Alex Rosenberg and Daniel W. McShea, *Philosophy of Biology* (New York: Routledge, 2008), 96–126.

relationships between populations and the environment, and the same is true of some concepts in the social sciences. One significant difference between concepts in lower levels of organization and those at higher levels is that the things to which many concepts in both evolutionary biology and the social sciences refer are multiply realizable. If a thing is multiply realizable, then there is more than one way it can be realized. For example, an ecosystem can be realized by all the animals, insects, and environmental conditions in a wetland or by all the animals, insects, and environmental conditions in a desert. Multiple realizability creates complications that are not present at levels where it does not appear.

In "Folk Psychology is Here to Stay," Terence Horgan and James Woodward provide an objection to Churchland's argument. Their objection is that Churchland does not argue against or rule out viable non-reductive materialist theories such as anomalous monism[13]. They say, "The availability of anomalous monism as an alternative to reductive materialism makes it clear that even if FP is not reducible to neuroscience, nevertheless the token mental events posited by FP might well exist, and might well bear all the causal relations to each other, to sensation and to behavior which FP says they do." [14] The availability of viable alternatives to eliminativism makes it clear that, at the very least, it is possible for some folk psychological claims to be true. In addition, because

[13] Anomalous monism is the view that mental states are dependent on/supervene on physical states but are not reducible to physical states because there are no strict laws connecting mental states to physical states.

[14] Terence Horgan and James Woodward, "Folk Psychology is Here to Stay," in *Folk Psychology and the Philosophy of Mind*, ed. Scott M. Christensen and Dale R. Turner (Hillsdale, NJ: Lawrence Erlbaum Associates, 1993), 149.

there are viable non-reductive options, Horgan and Woodard think that Churchland is mistaken to assume that folk psychology must be reducible to neuroscience in order to be compatible with it. A more general point I would like to draw from the considerations about reductionism and Horgan and Woodward's objection is that reductionism is not the source of fruitfulness in the social sciences. Certainly Wilson would agree that biology has been fruitful despite our inability to reduce many of its concepts to lower levels. So the argument that the source of the fruitlessness of folk psychology is our inability to reduce it to lower level natural science is erroneous.

The social sciences do seem to lack the kind of coherence and connectedness that the natural sciences have. Since I don't think the lack of reducibility is the source of the problem, perhaps Wilson's more explicit objection about concepts is. Wilson thinks that using folk psychology in the context of data collection breeds error because folk psychology is used regularly outside the scientific context. I agree with Wilson that familiarity does in some cases breed error. However, Wilson sets up a false dilemma by making it seem as if the only options we have are either to use folk psychology in its most common form or abandon it altogether and use the methods of natural science. There is another option and that is to use the information we have about the weaknesses of folk psychology to more carefully categorize explanations in the social sciences.

I agree with Wilson, the use of folk psychology does tend to extend the reach of causal attributions of intentional states too far. We often use beliefs, desires, etc. to explain our own behavior and that of others; however, there are many cases in which we do not correctly attribute intentional states or correctly identify the causes of behavior.

We sometimes overestimate how much of what we do is intentional. However, there are at least some cases in which we clearly are both aware of our intentional states and correctly attribute intentional states as causes to ourselves and others. Furthermore, social scientists should be and *are* aware of the discrepancies between what people do and what they say. That is why Wilson's second criticism is also false. It is incorrect to say that social scientists use a simple common-sense version of folk psychology. Social scientists are, to varying degrees, aware that data collected from interviews, informal discussions, and surveys many not accurately represent the causes of behavior or even the interviewee's actual beliefs, desires, or feelings. It is common in anthropology, for example, to record as many objective facts about food distribution, family structures, etc. as possible, all of which can stand in contrast to the self-reports from individual interviews. Social scientists have attempted to standardize fieldwork methods and to work within explicit theoretical frameworks. These attempts at standardization and uniformity have not been very successful in the sense that many social sciences don't have one set of methods and theory everyone uses in the same way, and this is a worthy target of criticism. Perhaps if the social sciences had an explicit, uniform set of methods and there was general agreement about background theoretical claims, the social sciences would be more fruitful.

Wilson's third criticism is that social scientists "as a rule" do not pay attention to, or use discoveries in, biology or scientific psychology to inform their work. This is simply false. As was noted in the previous criticism, there is a lack of unity with regard to appropriate methods. A central disagreement in anthropology is over to what extent the

theories, methods, and discoveries of biology should be incorporated into anthropological research. Some anthropologists, such as Lawerence Kuznar, strongly advocate using standard principles of the scientific method in anthropological research. Wade Davis's ethnography *Passage of Darkness: The Ethnobiology of the Haitian Zombie* attempts to understand Haitian zombies anthropologically, while giving an explanation of the zombie phenomena based on the scientific research about relevant plants and animals. In addition, anthropologists like Helen Fisher have made the discoveries of biology and scientific psychology a main feature of her research program. Although I would not claim that anthropologists or social scientists in general, "as a rule" use the discoveries of biology in their work, many social scientists are open and actively seeking a more concrete connection to the natural sciences.

What Wilson might mean by his criticism is that he does not think social scientists give enough weight to evolutionary considerations or causes other than intentional ones, and that is their mistake. If this is his criticism it very well may be true. However, it is by no means obvious in what capacity and to what degree the explanations given by social scientists can be informed by biology without changing the subject.

**Reductionism, Competition, and Organization**

The second threat reductionism presents to the social sciences is in the form of competition between social sciences for explanatory primacy. Since there is no clear explanatory hierarchy in the social sciences, some theoretical approaches, like methodological individualism for example, are in competition with other approaches. The debate is over which explanations are more fundamental. Are explanations involving

individuals the best explanation of human behavior or are explanations involving social facts more explanatory? Since there is no general agreement about the hierarchy or about how these explanations are connected to one another, each discipline is threatened by the others' explanations and thus competition arises, which only creates more divisions. The need for a coherent structure in the social sciences is a long standing problem;[15] reductionism simply highlights existing tensions among the social sciences. By examining the structural problems in the social sciences, I hope to show that explanations in the social sciences should not be in competition with each other or with explanations given by natural scientists. If social scientific explanations are no longer in competition with each other, a more productive organizational structure can emerge.

*Reductionism as an Organizing Principle*

Even if some natural sciences cannot in fact be reduced to physics, reductionist notions do create a reductionist hierarchy among the natural sciences. The challenges for explanation and reduction in biology are mirrored in the social sciences, and yet biology[16] maintains the hierarchical organization of the natural sciences while the social sciences do not. One reason for the dissimilarity is the subject matter of the social sciences.

---

[15] For example, in a 1955 paper entitled "Societal Facts," Maurice Mandelbaum puts his concerns this way: "In the first place, the concepts and methods utilized in the natural sciences have been more sharply defined than have been those which social scientists employ. In the second place, there is less disagreement among natural scientists than among social scientists as to the purposes which actually do underlie, or which should underlie, their studies. In the third place, the relations among the various branches of natural science seem to be more easily definable and less subject to dispute than is the case among the social sciences."

[16] For example, ecology would be a higher level of explanation than cell biology.

Both biology and the social sciences deal with higher levels of organization that exhibit a great deal of complexity. The more complexity there is, the harder it is to isolate the variables relevant to explaining an event. Although biology deals with complex systems, biologists have a wider range of experimental techniques available for eliminating and isolating variables. This makes it easier to demarcate the domains of each branch of biology and to preserve a reductionist hierarchy. The ability to isolate or eliminate variables allows scientists to more accurately determine causes and the percentage of causal contribution when multiple causes are in play. When we can isolate causal contribution it is easier to sort out which domain an explanation belongs to, which keeps explanations from competing with each other. Since it is clear which branch of biology explains certain phenomena and the domains of explanation of each branch of biology are clear, the branches of biology, while in a hierarchical relationship, are not competing over explanatory primacy because everyone knows where they fit in the structure of the natural sciences.

The social sciences are limited in their ability to control variables because of the ethics that govern human experimentation. The limits on experimentation coupled with the complexity of human behavior lead to an ambiguous structure in the social sciences. As a result it may be unclear what questions should be answered by which discipline, and often more than one discipline will answer the same question from a different perspective. The inability to isolate and eliminate variables to the same degree as the natural sciences is a serious disadvantage for the social sciences. This disadvantage is partly to blame for the lack of a clear organizational structure in the social sciences. Since

the social sciences cannot demarcate their disciplines in the same way biologists do, they must use some other strategy for organizing the disciplines.

*Organization in the Social Sciences*

What social scientists do know is that the causes of human behavior are many and complex. A complete explanation of human behavior will be a multi-level explanation. Human behavior is caused and affected by many influences. In addition some human behavior is multiply realizable, for example communicating can be realized by verbal utterances, signs, whistles etc. When explaining complex events one would expect there to be more than one explanation which, taken together with other explanations, will form a complete explanation. Sober puts this point nicely:

> Perhaps an ideally complete scientific explanation of a singular occurrence in which an individual (or set of individuals) exhibits a multiply realizable property (or relation) would include the macro-story, the micro-story and an account of how these are connected. If this is right, then reductionists and antireductionists alike are mistaken if they think that only part of this multilevel account deserves mention.[17]

I agree with Sober that a complete explanation is going to involve explanations on more than one level as well as an account of how these explanations are related to each other. The social sciences and some of the natural sciences all contribute explanations to the macro-story and the micro-story of human behavior.

---

[17] Elliot Sober, "The Multiple Realizability Argument Against Reductionism," *Philosophy of Science* 66 (December 1999), 550.

The disputes in the social sciences over which explanation is best, or primary, or "real," are out of place given the nature of the subject matter of the social sciences. The explanations of social scientists are not in competition with each other for explanatory primacy or causal relevance. The problem in the social sciences is not really about which view is more fundamental but rather about the relationship between the explanations. Debates about social facts and methodological individualism are not about the relationship between explanations that cite social facts and explanations that cite individuals; rather they are about which sort of explanation is the "real" explanation. If those on the side of social facts are right, then explanations in terms of individual motives, etc. are false, because they are not the real causes of behavior, and do not explain. If methodological individualists are correct, then it is facts about individuals which are explanatory and explanations involving social facts are not explanatory. But neither of these views is unproblematic and many philosophers have come to the conclusion that some kind of middle position is correct. Mandelbaum, for example, suggests that there are some social facts that cannot be reduced to individual behavior and that studying individual behavior is explanatory as well. He writes:

> [I]t is important to insist that even though societal concepts cannot be translated into psychological concepts without leaving this societal remainder, it is not only possible but is indeed necessary to make the *partial* translation. . . . Ideologies and banks and marriage systems do not exist unless there are aggregates of individuals who think and act in specific ways, and it is only by means of establishing the forms of their thoughts and their actions that we can apprehend

> the nature of the societal organization in which they live, or that we can corroborate or disallow statements concerning this organization.[18]

Mandelbaum and others[19] make it clear that explanations that involve social facts and those that involve individual intentional states are important in generating a complete explanation of human behavior. If the best explanation of human behavior involves both social facts and individual intentions, these explanations are not in competition, but rather are in need of alignment with each other. This is why a reductionist hierarchy as an organizing principle doesn't work well in the social sciences.

In rejecting the multiple realizability argument against reductionism, Sober captures well the problem with viewing explanations in the social sciences as competing explanations. Sober says:

> [S]urely an ideally complete physics would not make it reasonable to reject all statements in higher-level sciences. This means that those statements must be needed to explain something that statements in an ideal physics could not explain. The multiple realizability argument presents itself as a diagnosis of why this is so.
>
> This line of argument rests on a misunderstanding of inference to the best explanation. If you think that $A_1$ is one of the micro-realizations that *P* has, then you should not view "*P* causes *Q*" and "$A_1$ causes *Q*" as competing hypotheses (Sober 1999). The evidence you have may justify accepting both. Inference to the

---

[18] Maurice Mandelbaum, "Societal Facts," in *The Philosophy of Social Explanation*, ed. Alan Ryan (New York: Oxford University Press, 1973), 113

[19] For example, Kincaid in "Reduction, Explanation, and Individualism" argues against methodological individualism but maintains the importance of explanations involving individuals.

> best explanation is a procedure that belongs to the context of justification. Once you have used that technique to accept a variety of different hypotheses, it is perfectly possible that your set of beliefs will furnish several explanations of a given phenomenon, each perfectly compatible with the others. Some of those explanations will provide more details while others will provide fewer. Some may cite proximal causes while others will cite causes that are more distal. The mistake comes when one applies the principle of inference to the best explanation a *second* time—to the set of hypotheses one *already* believes, and rejects hypotheses that one does not "need" for purposes of explanation.[20]

The social sciences have accepted, to some degree, that human behavior should be explained in terms of groups and individuals. The disciplines of the social sciences are clearly meant to give explanations some with more detail and others with less. Sociology, for example, studies societies and institutions on a large scale. Economics gives more specific information about how people use resources like land and labor on both a small scale and a large scale. Anthropology studies small groups and individuals within cultural a cultural context. Psychology gives more detail about normal and abnormal behavior in individuals but also makes generalizations about larger groups. Each explanation contributes something to our understanding of human behavior, but none of the explanations is alone a sufficient explanation of human behavior. Since the explanations each branch of social science provides are not complete explanations and they address different aspects of human behavior, they are not in competition with each other for causal or explanatory primacy. Human behavior is complex and requires study from

[20] Sober, 556.

many directions. Once we see that all social sciences are working toward that same general goal the internal threat posed by reductionism is no longer a concern.

**Conclusion**

The social sciences, if they are to make progress, need to be more unified with respect to their goals. However, neither changing the subject matter of the social sciences nor fighting over explanatory hierarchy will create the unity the social sciences need. I have argued that although the social sciences can and should integrate the findings of the natural sciences into their theory and method, they should not abandon the goal of explaining human behavior in a social context which requires unique methods. I have also argued that the explanations from each social science should not be viewed as in competition with each other. Explaining complex events like human social behavior will require multiple explanations which, taken together, will be a complete explanation.

# CHAPTER 5

# CAUSAL EXPLANATIONS IN THE SOCIAL SCIENCES

Up to this point I have described the theoretical conflicts in cultural anthropology. Now I would like to address what is at the root of these disagreements. The root of the difference between interpretive anthropologists and scientifically oriented anthropologists is a disagreement over the scientific status of intentional states. There is no clear philosophical theory about how to treat explanations that involve intentional states. We do not have a clear picture of how these sorts of explanations fit into scientific explanations or even if they fit at all. There is something of a consensus among philosophers that intentional states can be causal. The idea that human intentions are a real causal part of moral action is widely accepted. However, there is also controversy over our ability to find psychological laws, an issue that affects how we view the scientific status of intentional states. I will attempt to clarify the problem in philosophy and argue that the lack of philosophical consensus creates a problem for anthropologists and other social scientists. Then we will consider how these problems can be solved. In doing so, I will address challenges to both the causal relevance of token mental states and the role of mental states in explanatory generalizations.

## The Root of the Problem

Theories about human action in the social sciences rely on an intuitive understanding of two issues: the causal relevance of intentional states and the role of laws in explanation. The way these issues are resolved in the theories of social scientists

makes a significant difference to how they approach their subject matter. Recently anthropology has split in two directions. Some anthropologists have taken a naturalistic approach while others have moved in an interpretive direction. Naturalistic anthropologists[1], generally speaking, want to align the methods and goals of anthropology with those of the natural sciences as much as possible. Interpretive anthropologists, again generally speaking, do not think that mimicking the natural sciences is possible or desirable. In previous chapters I have discussed many of the theoretical and methodological debates this division has created and now I would like to address what I think is at the root of the split.

At the root of the split is a disagreement about how generalizations based on data about intentional states should be treated. Let us look at how naturalists might reason. They might assume that either intentional states are not causally relevant, or that the generalizations based on data that cite intentional states cannot be subsumed under laws and therefore fail to meet the standards of scientific explanation. If one holds either of these views, one would have a reason not to use traditional methods of data collection. Methods such as participant observation and interviews primarily collect information about the subject's beliefs, desires, motives, etc. Since this data contains descriptions of intentional states and generalizations about intentional states, it would not be considered usable or scientific data. If one was working with the assumption that human behavior cannot be explained scientifically by intentional states, one might be inclined to think that

[1] Some examples of naturalistic anthropologists are Lawrance Kuznar, John Tooby and Leda Cosmides, and Helen Fisher. Some examples of interpretive anthropologists are Clifford Geertz, and Jill Dubisch, who I discussed at length in an earlier chapter.

better explanations of human behavior could be found either at the biological level, or through other methods, such as those of natural scientists who observe and explain the behavior of non-human animals. Some anthropologists take either of these approaches to the data.

Now let us assume instead, as the interpretivist might, that intentional states are causally relevant. If intentional states are causally relevant then it would make sense to try to understand and explain human behavior in a cultural context. If intentional states are causally relevant then interviews of people would contribute to at least partial explanations of their behavior. If intentional states are explanatory it would make sense to have the goal of understanding people in their cultural context and to use traditional methods such as interviews and participant observation to attain that goal. The interpretive anthropologists who take individual intentions to be causally relevant often concede that the data is not as reliable[2] as other kinds of scientific data. Although interpretative anthropologists might endorse the idea that intentional states are causally relevant they often do not accept the claim that the explanations of behavior that they collect are scientific explanations. The reason for this is that many believe that a scientific explanation requires the explanation to be subsumed under laws, or that in order for a generalization to be scientific it must be exceptionless and thus the interpretive anthropologist would not think of the explanations they provide as scientific. This is not to say that interpretive anthropologists would not like their explanations to be scientific, but rather by making the assumptions that they do about what is required for a scientific

[2] Additional reasons for thinking the data is not as reliable as data in the natural sciences has been discussed in other chapters.

explanation, they do not have the argumentative resources to defend their explanations against those who accuse them of being unscientific.

The products of the naturalist and interpretivist assumptions are, on the one hand, explanations that do not explain human behavior in a cultural context, and on the other hand, explanations that do not attempt to meet the requirements for scientific explanation. Neither of these is desirable and both are based on assumptions about the causal relevance of intentional states.

I think it is clear that a lot rides on having agreement about the role of intentional states and laws in explanations in anthropology. Without some consensus on these issues it will be difficult for anthropologists to have a unified set of goals and methods. I think we can take what is good about each approach to achieve a middle ground that integrates the need for scientific explanations, while preserving the relevance and value of the traditional goal of understanding human behavior in a cultural context. Explanations in the social sciences need to meet the standards of scientific explanation, but we ought not to expect them to be exactly like the explanations found in physics. Since the main difference between anthropological explanations and natural science explanations revolves around the use of intentional states, social scientists need basic standards by which they can evaluate the data they produce. The difficulties in having a standard approach to this kind of data are many and difficult but not insurmountable.

One of the main features of scientific explanation is the elucidation of causal relationships and the laws that govern those relationships in the current explanatory scheme. Since issues about scientific explanation are at the root of the split between

naturalistic and interpretivist anthropology, we need to explore these issues in more detail. In order to have a theory of explanation, anthropologists and other social scientists need some way to make sense of data that contains intentional states. Anthropology, and the social sciences in general, need a unified theory of explanation for at least one reason. Without one there can be no clear connection between explanations in the social sciences and those in the natural sciences. And if the social sciences are to maintain their status as true sciences they will need a way of connecting their explanations with those of the natural sciences. We turn now to what philosophers have to offer on the connection between explanation, laws, and intentional states.

**Invariance vs. Laws**

To see how the philosophical problems that underlie the different theoretical approaches to data in anthropology arise, we need to examine the claims that create the problems. I will address the two main problems that arise for the social sciences: the problem of exclusion and the problem of nomic subsumption. Both of these problems have a bearing on the causal relevance of intentional states and so resolving these issues will set the groundwork for a theory of explanation in the social sciences.

Before I address the main issue I first want to explain in more detail why the issue of causal relevance is important to the social sciences. The social sciences attempt to do at least two things. First, they describe and catalogue human behavior. Second, they try to explain human behavior in terms of various influences such as individual intentions or social facts. In order to give true descriptions of human behavior one must correctly identify at least some of the causes of behavior. To give true explanations of behavior in

terms of cultural influences or intentions, these things must actually be casually relevant. If human beings do not do what they do, at least in part, because of their intentional states or cultural influences, then the social sciences would not provide explanations and would provide only false descriptions of what people do. It might be argued that all we need in order to understand other people is to know what they think they are doing. In this case it would not matter if social scientists gave false descriptions or false explanations because we would still have all the information we need to understand people. While understanding people is certainly part of the goal of the social sciences, to just understand what people think they are doing does not fulfill the mission of science. Science attempts to actually explain its subjects. If the social sciences are going to be true sciences, they must do more than just report what people think they are doing: they must also explain what causes people to do what they are doing. Insofar as the social sciences set out to give true descriptions and explanations of human behavior, the issue of the relationship between the causal relevance of intentional states and laws needs to be addressed.

There are many philosophical positions on the relationship between mental states and physical states, but only some of them support the idea that mental states are causally relevant. I will start by discussing the view that mental states are not causally relevant and then I will focus on the view that mental states are causally relevant. Many philosophers take the relationship between the mental and the physical to be a supervenience relationship. Perhaps the most popular form of this view is functionalism. Functionalists define mental states in terms of their functional properties. If a mental state is defined in terms of its functional properties, then mental states can be realized in many

different ways. Functionalism seems like a promising theory about the relationship between the mental and the physical, however Jaegwon Kim has argued that functionalism leads to epiphenomenalism of the mental.

In "Mechanism, Purpose, and Explanatory Exclusion," Kim articulates well the problem of exclusion and how it leads to epiphenomenalism. Views, like functionalism, that propose a supervenience relationship between the mental and the physical need to explain how mental states supervene on the physical base. The main problem with explicating the relationship between the physical base and the supervening mental state is that it seems as if all the causal work is done by the physical base which leaves no causal work for the supervening mental state to do; this is the problem of downward causation. If the mental cannot cause changes in the physical base, then the physical base is really where all the causal work gets done. The problem of exclusion is just that if the physical base is where all the causal work is done, then it seems that the supervening mental states are excluded from doing any real causal work which would make them epiphenomenal.[3]

When one is trying to explain intentional behavior as anthropologists are, one needs to be able to identify what is doing the causal work so that the explanation correctly identifies the cause of the behavior. Psychology and anthropology both collect data that assumes, to some degree, that mental states are causally relevant. If mental

[3] Jaegwon Kim, "Mechanism, Purpose, and Explanatory Exclusion," in *Supervenience and Mind: Selected Philosophical Essays* (Cambridge UK: Cambridge University Press, 1993), 237–264. Kim also discusses the problem of exclusion in *Physicalism, or Something Near Enough* (Princeton: Princeton University Press, 2005). For a different statement of this problem see Timothy O'Connor and John Ross Churchill, "Is Non-reductive Physicalism Viable within a Causal Powers Metaphysic?" in *Emergence in Mind*, ed. Cynthia Macdonald and Graham Macdonald (Oxford: Oxford University Press, 2010), 43–60.

states are not genuine causes then social scientists would give false descriptions and explanations of a lot of human behavior. So if functionalism really does lead to epiphenomenalism of the mental, then it cannot provide a satisfactory basis for explanation in the social sciences.[4]

Let us consider a different approach to the relationship between the mental and the physical. Donald Davidson's anomalous monism is the view that mental states are identical to physical states, but there are no laws connecting mental states to other mental states, or mental states to physical states. Davidson's view is a supervenience view in the sense that a change in mental states means a change in physical states. Davidson explicates the supervenience relationship in a very simple way: the mental state supervenes on the physical state because the mental state is identical to the physical state. Critics of Davidson, such as Kim,[5] complain that in order for mental states to be causally relevant they must be able to cause events under their mental descriptions. But Davidson denies that mental states cause events under their mental descriptions. Davidson responds to Kim's criticism by claiming that no matter how we describe an event, its causal powers stay the same, and hence it does not matter that mental states cannot cause events under their mental descriptions. However, Davidson's reply to Kim does not solve the problem because of the principle of nomic subsumption which says that if events are related to

[4] If it turns out that some version of functionalism correctly represents the world, then the social sciences in their present form would have to make extensive fundamental changes. While this is not a reason for rejecting functionalism, it would be a reason to overhaul the social sciences.

[5] Jagewon Kim, "Epiphenomenal and Supervenient Causation," *Midwest Studies in Philosophy* 9 (1984): 257–270.

each other as cause and effect they must fall under strict laws. If mental states are to have any causal relevance then they would need to fall under strict laws, but Davidson does not think that mental states fall under any strict laws when described as mental states. As a result, some like Kim and McLaughlin[6] argue that Davidson's position implies that mental states are epiphenomenal. Epiphenomenalism about the mental is incompatible with explaining human behavior with reference to mental states as some social sciences do.

The positions of Kim and Davison set up the problems that lead to epiphenomenalism very well. By addressing the problems with Kim and Davidson we can clear the hurdle that epiphenomenalism presents to the social sciences. Considering these views leaves us with two problems. Can mental states be causally relevant if they supervene on a physical base? And, can mental states be causally relevant if they are not subsumed under strict laws? I think the answer to both of these questions is yes. These questions can be answered in the affirmative by considering arguments by Stephen Yablo and James Woodward.

**Yablo**

In "Mental Causation,"[7] Stephen Yablo provides a plausible solution to the problem of exclusion. Yablo argues that mental events[8] are causes and causally relevant in most of the cases in which we take them to be. Yablo has two goals: the first is to show

---

[6] Brian P. McLaughlin, "On Davidson's Response to the Charge of Epiphenomenalism," in *Mental Causation*, ed. John Heil and Alfred Mele (Oxford: Clarendon Press, 2003), 27–40.

[7] Stephen Yablo, "Mental Causation," *The Philosophical Review* 101 (1992): 245–280.

[8] Yablo also argues that mental properties are causally relevant.

that mental events can be causally relevant, and the second is to show that they are in fact causally relevant causes. Yablo's general strategy is to show that the mental is related to the physical as determinable is related to determinate and then argue that because they are related in this way the mental is causally relevant.

Before showing that the mental is related to the physical as determinable is related to determinate, Yablo formulates the determination relation in purely metaphysical terms to avoid standard conceptual and metaphysical objections. The example most often used to illuminate the determinable/determinate relationship is color. Scarlet is a determinate of the determinable red, and red is a determinate of the determinable color. Similarly, in the case of the relationship between the mental and the physical, the physical event is a determinate of the mental event, which is the determinable. Yablo argues that the combination of multiple realizability (M) and supervenience (S) add up to the determination relation, metaphysically conceived. Yablo puts it this way:

> Together, (M) and (S) make it a matter of necessity that something has a mental property iff it has a physical property by which that mental property is asymmetrically necessitated. But this is extremely suggestive, for with 'determines' substituted for 'asymmetrically necessitates', it becomes
>
> (D) Necessarily, something has a mental property iff it has also a physical determination of that mental property;
>
> And (D) is an instance of the standard equation for determinables and determinates generally, namely, that something has a determinable property iff it has some determinate falling thereunder. This calls out for explanation, and the

> one that comes first to mind is that mental/physical relations are a species of determinable/determinate relations.[9]

To clarify the first part of the quotation, it is clear that the physical base could be a determinate of the mental only if the mental could exist without that particular physical base, which is just what multiple realizability says. Yablo argues that even if mental properties are multiply realizable, the mental properties must have some physical base but they needn't have any physical base in particular. As indicated above, the mental and physical are related as determinables and determinate. It is a characteristic of the determination relation that determinables are not in competition for causal relevance with their determinates. Since the mental and physical are related as determinable to determinate then contrary to exclusion there is room for the mental to be causally relevant.

Perhaps the biggest challenge the problem of exclusion presents is that if the physical is causally sufficient for an effect then there can be no other independent cause. One way to deal with this problem is to show that even if the physical is causally sufficient for an effect, there can be other causally relevant factors. This is precisely Yablo's strategy. He provides examples which demonstrate that even if *x* is causally sufficient for *y* there can still be other factors which are causally relevant to *y*. He writes:

> Let *x* be causally sufficient for *y*. Then taken at its word, the exclusion principle predicts that *y* owes nothing to the causal intermediaries by which *x* brings *y* about. When R is causation's converse, the prediction is different but still absurd: events causally antecedent to *x* can claim no role in *y*'s production. Of course, the

[9] Yablo, 256.

> case that interests us is *R* = the determination relation. Remember Archimedes' excited outburst on discovering the principle of displacement in his bath. Assuming that his shouting "Eureka!!" was causally sufficient for his cat's startled flight, nobody would think that this disqualified his (simply) shouting from being causally relevant as well.[10]

What examples like the one Yablo mentions show is that it is a mistake to think of causal influence as a competition between antecedents which only one of them can win. Instead, at least with respect to things related by the determination relation, the causal antecedents act more like partners rather than competitors.

After Yablo establishes that mental events can be causally relevant despite the fact that their physical bases are causally sufficient for them, he goes on to argue that mental states are in fact casually relevant in the majority of common sense cases. Causes ought to be commensurate with their effects. They should contain causally important details and not much that is causally irrelevant. The idea behind this commensuration constraint is that causes should make a difference to their effects. Yablo fleshes out this constraint with four formal requirements for causes. First, effects must be contingent on their causes:

> (C) If *x* had not occurred, then *y* would not have occurred either.[11]

Second, causes must be adequate for their effects:

> (A) If *x* had not occurred, then *if it had, y* would have occurred as well.[12]

---

[10] Ibid, 272.

[11] Ibid, 274.

[12] Ibid, 274.

Third, we have the requirement condition where *x* is required for *y* when:

(R) For all $x^- < x$, if $x^-$ had occurred without *x*, then *y* would not have occurred.[13]

Fourth, *x* is enough for *y* when:

(E) For all $x^+ > x$, $x^+$ was not required for *y*.[14]

When all four conditions are met, *x* is proportional to *y*. When deciding between two causes the one that is most proportional to the effect is to be preferred. Yablo illustrates how mental events can be causes using an example that involves a decision. Yablo imagines a scientist keeping track of his brain state *e* as he decides whether to knock on your door or ring the doorbell. What we want to know is whether it is the decision *m* or its physical determination *p* that causes the scientist's readings. Yablo clearly explains why *m* is better suited to be the cause of *e* than *p* is. He writes:

(where *r* = the doorbell's ringing):

(i) *m* is a counterexample to *r*'s requiring *p* (for *r* would still have occurred, if *m* had occurred without *p*).

(ii) *p* is not proportional to *r* (since *r* does not require it);

(iii) *p* does not cause *r* (since it is not proportional to *r*);

(iv) *p* is not a counterexample to *m*'s enoughness for *r* (it could be a counterexample only if *r* required it);

---

[13] Ibid, 276. In (R) and (E) $x^-$ and $x^+$ stand for events. (R) Should be read: For all events such that one is greater and one is lesser, if the lesser event had occurred without the greater event, then y would not have occurred. Therefore, according to Yablo, if Socrates cannot drink the hemlock without guzzling it, then if the drinking (the lesser event) had occurred without the guzzling (the greater event), then Socrates would not have died (y). So the guzzling is required for the dying.

[14] Ibid, 277.

(v) *p* is not a counterexample to *m*'s proportionality with *r* (by inspection of the remaining conditions);

(vi) *p* poses no evident threat to the hypothesis that *m* caused *r*.

Here are the beginnings, at least, of a story wherein a mental event emerges as better qualified than its physical basis for the role of cause. I believe this *kind* of story is enacted virtually wherever common sense finds mental causation.[15]

Yablo's argument shows how mental events can be causally relevant, and how in fact they are, thus the problem of exclusion is avoided. Most importantly for the social sciences, Yablo shows how the mental is causally relevant in most common sense cases. This is important for the social sciences because much of the ethnographic data is the result of interviews, so it is important that in most cases where a mental state is cited as a cause of someone's behavior, it can be the cause of the behavior. Although Yablo's argument does not guarantee that social scientists will always correctly identify the cause of someone's behavior, this view does make it reasonable for social scientists to describe events that involve beliefs, desires, and other mental states as they commonly do.

**Woodward**

Although Yablo's argument provides a solution to the problem of exclusion, we are still faced with the concerns that arise from Davidson's view. Yablo explicitly states[16] that his argument does not address the same problem Davidson addresses. Yablo shows us how mental events can be causally relevant in individual cases but he does not address generalizations that involve mental events. The main difficulty for Davidson arises from

[15] Ibid, 278–279.

[16] Yablo, notes 11 and 13, 249.

the requirements for causal relevance for generalizations. Kim's objection to Davison is that in order for an event to be causally relevant it must be subsumed under strict laws, but since mental events are not governed by strict laws mental events are not causally relevant. The nomic subsumption requirement for causal relevance is problematic both because of the threat it poses to the causal relevance of mental events and because of the threat it poses to the causal relevance of any generalization not subsumed under strict laws, like those found in the social sciences. One way to address Kim's objection is to deny that subsumption under strict laws is required for causal relevance; this is just the strategy James Woodard takes. Woodward offers an alternative account of causal relevance for generalizations that is helpful in two ways. First, his view in conjunction with Yablo's provides us with a way of answering Kim's objections. Second, Woodward supplies us with a method for distinguishing explanatory generalizations from non-explanatory generalizations in the social sciences.

In "Explanation and Invariance in the Special Sciences," Woodward provides an account of causal explanation that does not require generalizations to be subsumed under strict laws in order to be explanatory. Woodward argues that it is possible for a generalization to be explanatory even when it holds over only a specific domain. Woodward's idea is that in order for a generalization to be explanatory it must be invariant in certain circumstances rather than lawful. A generalization is invariant when it is change-relating (defined below) and remains stable even when tested by experimental manipulations. Woodward's account of causal relevance is helpful in resolving Davidson's problem because Davidson's trouble arises from his claims that mental states

are not connected to each other or to physical states by strict laws. This claim is problematic because of the principle of nomic subsumption which says that if events are related as cause and effect, they must be subsumed under strict laws. Given that Davidson does not think that mental states under their mental descriptions are governed by strict laws, it would follow that they are not causally relevant. In order to preserve their causal relevance, one would need to show that there are criteria that separate explanatory generalizations from non-explanatory generalizations, but do not require nomic subsumption. Woodward's notion of invariance under interventions provides us with just this sort of criteria.

Woodward rejects the traditional view of explanation which requires nomic subsumption, among other things, for explanation. Woodward thinks that laws are neither necessary nor sufficient for explanation. The reason he thinks that laws do not distinguish explanatory generalizations from non-explanatory generalizations is that in some cases laws are not "change-relating," that is, they do not tell us what changes would occur if we manipulated one of the variables of a generalization. Instead he proposes that generalizations that are invariant under a range of interventions are explanatory. Not all invariant generalizations will be explanatory; only those that support active counterfactuals will be. An intervention is an experiment meant to test the variables of a generalization for a causal relationship. In order to rule out spurious relationships, the intervention must meet certain criteria. First, the intervention should not be correlated with any other cause, but a cause may be correlated with an intermediate cause. Second, one must rule out causes that would affect the outcome independently of any causal chain

that leads to the effect. Third, the intervention should not cause the outcome, unless it causes it through the experimental cause in question.[17]

One advantage of using invariance to distinguish explanatory from non-explanatory generalizations is that invariance admits degrees. A generalization can be invariant under some conditions but not others. By specifying the domain under which a generalization is explanatory, one can provide explanations using generalizations that hold under only that domain. This more accurately reflects the way some natural scientists use generalizations to explain phenomena and provides the social sciences with a way to account for the intuitively plausible notion that they, at least sometimes, provide explanations. A second advantage is that since invariance can be used to distinguish explanatory from non-explanatory generalization in a way that does not depend on nomic subsumption, Davidson's problem can be resolved. Kim's objection to Davidson is that mental states under their mental descriptions cannot be subsumed under any strict laws and are therefore not explanatory. However, if we use the standard of invariance instead of nomic subsumption then we only need ask if mental states under their mental descriptions figure in invariant generalizations. It seems much more likely that there are invariant generalizations that connect mental states to other mental states and mental states to physical states.

Although Woodward provides us with a helpful account of explanation that does not require nomic subsumption, he does not address individual cases where mental states are cited as causes. Since the social sciences cite mental states as causes in both

[17] Woodward provides a formal characterization of an intervention on page 201 of "Explanation and Invariance in the Special Sciences."

generalizations and in individual cases, we need to adopt both Woodward's approach to generalizations and Yablo's approach to individual cases. Both views taken together answer Kim's criticisms and set the stage for a systematic approach to data that involves mental states in the social sciences.

**Evaluating Generalizations in the Social Sciences**

In addition to resolving some of the issues raised by Davidson's view, Woodward is helpful in another way. Earlier I claimed that what is at the root of the division in anthropology is differing views on the scientific status of intentional states. The differing assumptions about the nature of intentional states are based on notions about what is required for an explanation to be a scientific explanation. Both naturalist anthropologists and interpretivist anthropologists seem to accept the traditional criteria for scientific explanation, at least implicitly. What divides them is the extent to which they think the traditional methods of cultural anthropology can satisfy these criteria. Neither side thinks the generalizations anthropologists make can serve as the basis for scientific explanations. In response to this alleged fact about generalizations in anthropology, anthropologists have, roughly speaking, split into two camps. Those who think that the appropriate response to learning that generalizations in anthropology do not meet the usual scientific criteria is to try to make the methods of anthropology as much like those of the natural sciences as possible, in the hope that by having a more scientific method they will produce scientific data and scientific explanation. In the other camp, interpretivists take it to be a futile goal to try to produce scientific explanations and instead see the goal of anthropology as understanding what people are doing without

making any claims to explain behavior scientifically. Some interpretivists may see their goals as mainly descriptive, while others take their goal to be literary. I think that both types of anthropologists have conceded too much. What philosophy offers is a compromise that can reunite anthropology if anthropologists are willing to overturn their basic assumptions. Woodward and Yablo provide a bridge back to a united anthropology by explaining how both individual events can have genuine mental causes and how generalization over a limited domain can be causal and scientifically explanatory.

Woodward is especially helpful because he not only explains how generalizations about limited domains can be explanatory and causal, he also provides a standard by which generalizations can be evaluated. This tool provides anthropologists and social scientists in general a way to evaluate the quality of generalizations common in their fields. Part of the motivation for the split in anthropology was the lack of a clear procedure for evaluating the strength of generalizations and explanations produced by anthropologists. But if we accept Woodward's idea that an explanation is explanatory when it is invariant in the right way, we can unite the goals of both the interpretivist and the naturalist. Both interpretivists and naturalists broadly speaking want to understand human behavior in a cultural context. What divides them is how to go about achieving this goal and to what extent this goal is scientific. Using Woodward's notion of invariance would unite both the goals and methods of the interpretivist and naturalist by legitimizing both aims and both methods to some degree. Most importantly, Woodward's idea of invariance supplies a unique way of looking at causal explanation that allows explanations of the sort found in anthropology to be both causal and scientific without

having to meet the criteria that have hitherto prevented explanations in the social sciences from being considered truly scientific. The wider range of generalizations that count as scientific explanations on Woodward's view make it possible to see anthropology as aimed at giving scientific explanations while at the same time recognizing the limited domain of most explanations in anthropology and the social sciences in general.

Woodward's notion of invariance under an intervention[18] is clear enough that it can be used by anthropologists in the field to state and evaluate generalizations. If anthropologists could agree that some generalizations about limited domains[19] are scientific and explanatory, then they would have a foundation on which to build theory that integrates their goals and methods. Invariance integrates their methods by satisfying both the naturalist's desire to produce explanations that are causal and scientific and the interpretivist's recognition of the limited domain of generalizations in anthropology. By accepting Woodward's approach to explanation, the interpretivist and the naturalist can come back together to pursue what has always been the aim of cultural anthropology: understanding and explaining human behavior in a cultural context. Having a clear test for generalizations also makes it easier to distinguish between explanations of behavior and descriptive tasks undertaken by anthropologists.

If Woodward and Yablo provide a clear strategy for using mental states in explanations, then disagreements that stem from differing views about the scientific status and usefulness of data that involve mental states can be resolved. If these issues are

[18] Woodward allows that interventions can be natural or experimental, so in anthropology some of the interventions will be naturally occurring changes that are observed and recorded by anthropologists.

[19] The discipline will determine the domain to which the explanation is relative.

resolved, anthropology can be a united field that clearly pursues a scientific goal while carefully dealing with the unique challenges of the subjects of study.

**Objections to Woodward**

Woodward and Yablo provide a solution to one major source of theoretical disagreement in the social sciences. However, invariance does not help to clarify the metaphysical structure of the sciences in the way that laws and reductionist hierarchy seem to. The level of generality of a law helps to establish the domain of a science and the place of the science in the hierarchy of sciences. Using Woodward's procedure for determining the importance of generalizations would yield a similar organizational structure to laws and reductionist hierarchy in the natural sciences. But in the social sciences his procedure would not work as well. The relationships among disciples in the social sciences are unclear at best. The social sciences are not unified by theory or method but only by a very general goal. There are many problems that arise out of this lack of unity in theory and method. Namely, different disciplines provide explanations that may compete with or even be incompatible with explanations from other disciplines. Without a unified theory and method it is difficult to assess the progress of the social sciences because progress in one discipline is often independent of progress in other areas. Since the social sciences have not been able to find laws that could help to organize them and they disagree about the order of the reductionist hierarchy, they are in desperate need of some organizing principle that will unify their theories and methods. I will argue that disagreement about compositional metaphysical relationships in the social

sciences which lead to theoretical differences make it difficult to apply Woodward's method completely.

I want to begin by explaining Woodward's procedure for ranking the importance of generalizations. Woodward uses two strategies for ranking the importance of a generalization: the proper subset relation and the idea of "privileged" interventions. These two standards together, Woodward thinks, will provide a way to distinguish more explanatory generalizations from less explanatory generalizations.[20] Both components are important because while the proper set subset relation provides an ordinal arrangement of generalizations, it does not provide an objective ordering and some generalizations will not be related in this way. Since the first way of ordering generalizations will not cover all cases, we need the second component, ranking generalizations by their ability to undergo "privileged" interventions, to finish the job. Each discipline will determine what counts as a privileged intervention. Woodward puts it this way:

> Thus expectations about the sorts of changes over which fundamental relationships will be invariant help to set the explanatory agenda for different scientific disciplines. These expectations will in turn be grounded in very general

[20] James Woodward, "Explanation and Invariance in the Special Sciences," *British Journal for the Philosophy of Science* 51 (2000), 221–222. Woodward writes:

> My suggestion, then, is that both of the considerations described in this section—comparisons of invariance based on the proper subset relation and judgments about the significance or importance of the intervention over which a generalization is invariant—play an important role in the construction and assessment of explanatory generalizations. Together they provide a basis for distinguishing among invariant generalizations with respect to degree and kind of invariance and for judging that although a generalization is invariant under some interventions, it is nonetheless relatively fragile or unrobust in the sense that it is stable only under a unimportant set of interventions or under a set of changes that is relatively small in comparison with some rival generalization.

> empirical discoveries about the sorts of relationships in the domains of these disciplines have been found to be invariant in the past and under what sort of changes.[21]

I think the idea is something like this: scientists will expect/require generalizations to be invariant under certain changes and a generalization's ability to undergo these changes will demonstrate the fundamental explanatory role of those generalizations. The changes which determine whether a generalization has fundamental explanatory power will be the 'privileged' changes. Once scientists have a general idea of what kinds of relationships have been invariant in the past, they can take note of what sorts of changes these relationships were invariant under and use those as the basis for determining what changes future generalizations will have to undergo in order to be 'privileged.'

The problem is that there are already structuring principles at work in the way scientists collect data, so relationships and generalizations will have been selected and ordered by the theoretical commitments of scientists. Woodward's suggestion that scientists look at past data to determine which changes are privileged would reflect the theoretical commitments of the scientists. This is not necessarily a bad thing if there is theoretical agreement about the fundamental metaphysics, but when there is no agreement, looking at empirical discoveries will just reflect that disagreement. If privileged changes are supposed to help to order and organize generalizations and help scientists to distinguish between important and unimportant generalizations, one can't

[21] Ibid, 219.

rely on data that is already organized by theoretical principles because then Woodward's criteria would not be the organizing principle; the theoretical principles would be.

As I said earlier, in the natural sciences this is less of a problem because of the level of agreement about very fundamental metaphysical commitments, but in the social sciences where there are no agreements about even the most basic metaphysical issues, using Woodward's method becomes more difficult. There is no agreement in the social sciences about which data are best, which generalizations have undergone important changes, which relationships are important, which methods produce data, etc., so the social sciences in a sense do not have a set of data that they can review and from which important relationships and changes may be selected. I will illustrate my point by using an example from economics that Woodward discusses.

Woodward uses the example to illustrate how individual disciplines can determine which changes are most important. In setting up this example, Woodward lays out the assumptions economists make about the rational choice theory, expected utility, and the self-interestedness of economic agents. Woodward points out that there are situations in which these principles do not hold, such as when an agent's brain has been manipulated surgically or through medication, but that economists do not tend to take these sorts of failures seriously. He goes on to say,

> By contrast, failures of invariance under other sorts of changes are regarded as much more important. For example, microeconomists often require that fundamental explanatory generalizations such as the principles of RCT be invariant under changes in information available to economic agents or under changes in the incentives or relative prices they face. Indeed, a standard

> assumption among many microeconomists—one might take it to be constitutive of a certain sort of methodological individualism—is that the generalizations that will be invariant under such changes in information and prices all describe the behavior of individual economic agents rather than the relations between macroeconomic or aggregate-level variables like 'inflation', 'unemployment', and 'gross domestic product'. That is, the idea is that there are no purely macroeconomic relationships that are invariant under changes in information and incentives and hence that there are no fundamental explanatory relationships between macroeconomic variables.[22]

The problem is that in the social sciences there is no agreement that the rational choice theory correctly represents relationships between groups and individuals. There is also disagreement about whether there are explanatory relationships among "aggregate-level variables." At least some economists and sociologists would take issue with these assumptions and not agree that the relationships and changes preferred by microeconomists are the right ones. Woodward is trying to show that each discipline would be able to decide which changes and relationships it finds most important and so may respond to my claim by saying that so long as microeconomists agree then it doesn't matter that sociologists, anthropologists or psychologists would disagree. However, theories like rational choice theory include an assumption about the metaphysical composition of individual and group relationships. Since the individual/group relationship is fundamental to all the social sciences, to adopt a theory which will be contradicted by other social sciences creates a very serious disunity and will make it

[22] Woodward, 220.

difficult to organize the social sciences as a whole. If Woodward's method is to work, assumptions or theories that play a role in marking a generalization or relationship important must not be an assumption or theory that will be contradicted by other disciplines in the social sciences. Given the daunting range of theories and the lack of agreement about the most fundamental relationships, finding theories or assumptions that could serve as a starting point would be very difficult. The reason for this difficulty lies with another metaphysical problem, the problem of composition.

In the natural sciences each science explains different things. Since each natural science has its own subject matter, the main metaphysical concern of the natural sciences is how each science fits with the others[23], where can one dovetail into the next. Using hierarchy to structure these relationships makes sense. However, the social sciences all explain the same thing[24], human behavior. Each discipline in the social sciences focuses on different causal factors that contribute to human behavior but the general subject matter is the same. This creates two structural issues in the social sciences: how do all the explanations of the social science come together to explain human behavior, and how do the social sciences fit into the order in the natural sciences. Let me address the first question. I am proposing that the structure and relationships among the disciplines in the

[23] It is not clear how every science fits with the others. In the social sciences, this uncertainty leads to disputes over the domains of each science and over the place of each science in the hierarchy, but such is not the case in the other sciences.

[24] I think the way the social sciences are divided is the result of making the social sciences mimic the natural sciences. But there are significant differences between the subject matter of the natural sciences and the subject matter of the social sciences. The social sciences can be seen as a single science with each branch taking a different approach to explaining an aspect of human behavior.

social sciences are significantly different than those found in the natural sciences. This is not a view generally accepted by social scientists in practice. Instead, social scientists have assumed that the social sciences will have a hierarchical structure like the natural sciences, and as a result, arguments over which discipline is most explanatory or most primary have arisen. More specifically, this is evidenced in debates over which theoretical approach is more explanatory, those that appeal to individuals or those that appeal to groups and institutions. It would matter which theoretical approach is more explanatory only if the explanations went from more primary to less primary in a hierarchical fashion. There does not seem to be a clear winner in these sorts of debates. Both explanations that involve individuals and those that involve groups and institutions provide us with interesting and relevant explanations of human behavior. What I think this tells us is that the explanation of behavior is most likely a single complex explanation that involves many causal factors. Each branch of the social sciences focuses on a factor or set of factors that contribute to explaining human behavior. If we see the social sciences as a whole providing an explanation of human behavior rather than each discipline providing multiple competing explanations of human behavior, then rather than competing for a place in the hierarchy of explanation, social scientists would need to focus on how their explanations together make up **an** explanation of human behavior. Only once we have an idea of what the whole explanation consists of can we answer the second question, how do the social sciences fit into the order in the natural sciences.

All of the theoretical splintering and competition for explanatory primacy in the social sciences keeps the social sciences from pursuing concepts and theories that would

unify their goals and methods. A lack of fundamental theoretical agreement slows, and sometimes halts, progress in the social sciences. What I have suggested is a way to understand the social sciences which preserves the important aims of each discipline while removing the obstacles to forming a unified set of goals and methods. What remains to be sorted out are the compositional relationships among the social sciences.

**The Structure of Explanation in the Social Sciences**

In this final section I want to return to an idea I mentioned earlier about the social sciences. The social sciences are different than the natural sciences because all the social sciences aim at the same general goal: to explain human behavior. Since they are unified by this goal, in a sense they are all aimed at explaining one thing. That is why it makes more sense to think of the explanations from each discipline as partial explanations of human behavior. In order to get the full explanation of human behavior, one would need to stitch together the explanations from each discipline. Since the explanations from each discipline work together to explain human behavior, it is imperative that the metaphysical structure of the things to which those explanations refer be elucidated.

One reason there is competition for explanatory primacy among some social sciences is that understanding the compositional relations between individuals, groups, institutions, cultures and nations is no easy task. It is unclear how the causal powers of psychological properties contribute to the actions of individuals, how the decisions of individuals determine the behavior of groups, and how groups make up institutions. Since there is no agreement about these compositional relationships, the social sciences use theories about these relationships that are in conflict with each other, which is an

impediment to using Woodward's method. This also creates a situation where explanations in one discipline compete with explanations in other disciplines to be crowned the "real" or most primary explanation. This would not be the case if the metaphysical structure of these relationships were clarified. I cannot sort out the entire structure of explanations in each social science and the structure of the explanations of the social sciences as a whole because each of these issues would require extensive treatment on their own. What I will do instead is provide a sketch of how one might address these issues. I will argue that, at least[25], an analogy can be drawn between mechanistic explanations in other special sciences and explanations in the socials sciences. Viewing the overall explanations in the social sciences in this way could structure the explanations in each of the disciplines of the social sciences.

**A Sketch of a Solution**

When you consider the kinds of explanations given by social scientists, it is easy to see that no single social science provides a complete explanation of human behavior. A full explanation of human behavior will be the result of combining a number of explanations. But in order to produce one, we need to know how those explanations fit together. It is this more general explanation that is analogous to a mechanistic explanation. In "Hyper-extending the mind?: Setting Boundaries in the Special Sciences," Carl Gillett provides a set of criteria that elucidates the metaphysical structure of mechanistic explanation in the special sciences that, when applied to the explanations in

[25] I say "at least" because explanations in the social sciences might not strictly speaking meet all of Gillett's criteria below; they meet most of them and the one they don't meet is adaptable, I think, to the social sciences.

the social sciences, can clarify the relationships between the explanations in the social sciences. Part of what is helpful about Gillett's criteria is that the causal powers and properties found in disciplines that focus on the individual should tell us something about the causal powers and properties of groups discussed in other disciplines.

In the literature there has been debate over whether the explanations that involve groups can be reduced to explanations about individuals. The debate has not been resolved because it seems clear on the one hand that individuals do make up groups, and so one might be inclined to think the group can be reduced to individuals, but on the other hand it seems that the group has some properties and powers that do not correspond with properties of the individuals that make up the group. This impasse is due to the view that there is a reductionist hierarchy between groups and individuals. If instead one considers individuals to be the parts and the group to be the whole, then the issue of reduction goes away and in its place we can ask how the properties and powers of individuals make up the powers and properties of the group. When we see the relationship as a part/whole relationship and we apply Gillett's criteria to this relationship, we will notice that both the individual, and the group it is a part of, have their own powers and properties that are connected to each other but that are unique. Thus, there is no competition between explanations at the individual level and explanations at the group level; rather there is a special connection between these explanations that will provide a mechanistic explanation of human behavior that unifies many of the branches of the social sciences.

I want to turn now to the details of Gillett's paper and then explain how these details can be applied to the explanations in the social sciences. Gillett's aim is to provide

a metaphysical structure for explanations in the special sciences that is true to the way scientists actually work. Gillett lays out eight conditions that a causally integrated mechanistic explanation needs to meet.

1. Compositional relationships are not a species of causal determination. Instead they are "synchronous, do not occur between wholly distinct entities and do not involve the mediation of force and/or the transfer of energy."[26] Based on these characteristics he calls such relationships non-causally determined.
2. The entities that bear compositional relationships to each other are usually qualitatively distinct. The idea is that the sub-units have powers that are not the same as the powers of the composed entity which have their own powers. In addition, "the entities taken to be involved in the relevant compositional relations are usually of *qualitatively different* kinds."[27]
3. Gillett says that "various 'packages' of powers, properties, individuals and mechanisms studied by lower level sciences *together* compose qualitatively different powers, property instances individuals and mechanisms studied by higher level sciences. Thus, although the components are qualitatively distinct from the entity they compose, none

[26] Carl Gillett, "Hyper-Extending The Mind?: Setting Boundaries in the Special Sciences," *Philosophical Topics* 35, no. 1/2 (2007): 166.

[27] Ibid, 166.

the less . . . these many components *together* non-causally result in this qualitatively different composed entity."[28]

4. The entities that are components are or have powers which comprise the powers of the composed entity.
5. The components are associated with or are individuals that bear powerful relations to each other.
6. The components are also spatially contained within the individual that is the composed entity.
7. The components are "*parts of* the individual associated with (or which is) the composed entity."[29]
8. This sort of compositional relationship takes place only under particular background conditions.

Let me now explain how I think these eight criteria apply to the explanations in the social sciences.

In order for this to work, we first need to see the individuals involved in the explanations in social sciences that deal with individuals and small groups as parts of the groups that other social scientists appeal to in their explanations. So, for example, the individuals involved in the explanations in psychology are parts of the groups featured in explanations of sociology. The causal powers and properties discovered about individuals will, in certain background conditions, produce the causal powers and properties of the

[28] Ibid, 166.

[29] Ibid, 166.

group. I think this is roughly true of the individual/group relationship.[30] With this in mind let us go through each of the criteria. The individual/group relationship[31] meets the first criterion because when one describes or explains group behavior, the individual behavior that composes the group is synchronous with it. Further, so long as we see the individual/group relationship as a part/whole relationship, we can see that the individual and group are not wholly distinct. The second criterion is especially important for quashing competition between the disciples of the social sciences. This criterion allows that the parts and the whole are qualitatively distinct. I think this is also true of individuals and groups. The third criterion makes clear that the properties and powers found at the lower levels taken together compose the properties and powers found at the higher level, and this is where social scientists should try to look for the mechanisms that create the situation in which the higher level entities have their powers and properties. This, I think, would provide an interesting area of study for social scientists that could advance the explanations of the social sciences as a whole.

Again, if we are thinking of individuals and groups in a part/whole relationship, it seems clear that they meet the fourth criterion: the powers of individuals comprise the powers of groups. The fifth criterion is also met because the relations between individuals do bear powerful relations to each other. The sixth criterion, that the components are spatially contained within the individual that is the composed entity, is true of aggregated

[30] There may be other factors I'm not considering that would not make this true but I am unaware of them.

[31] I would also include small group/larger group, larger group/institution, institution/culture and any other combinations of this kind found in the social sciences.

groups, however the sixth criterion may not be strictly speaking true of a looser sense of "group" but something similar to this criterion could be adapted for those cases. The seventh criterion is also met: individuals are parts of the composed entity—the group. And the last criterion is met because these individual/group relationships only take place under certain background conditions.

Since the entities explained and described by the social sciences fit Gillett's criterion we now have the beginnings of a structure for inter-level explanation in the social sciences. By understanding that each science provides a partial explanation that can be integrated into some of the other explanations to produce a more general explanation that is also a causal explanation, the social sciences will have a structure that does not put the explanations from each discipline in competition with each other. This should also provide some incentive to agree on basic metaphysical commitments about the relationships between individuals and groups. The picture Gillett paints is one in which the powers and properties at lower levels and at a higher levels are important, influential, and essential for explanation. This I think captures the relationships between disciplines in the social sciences better than the hierarchy that marks the structure of the natural sciences.

While I have not completely worked out all the details of inter-level explanation in the social sciences, what I have hoped to show is that there is a way to flesh out the special relationship among the disciplines in the social sciences. This strategy will eliminate some of the theoretical and structural issues that would keep the social sciences from being able to use Woodard's method.

## Conclusion

The social sciences are unique and complex, but they can use some methods from the natural sciences along with their own methods to piece together the puzzle that is human behavior. At the root of the disagreements is the issue of the causal relevance of intentional states. I have argued that intentional states are causal and that there are methods, such as Woodward's and Yablo's, that can produce scientific causal explanations. Once we understand that social scientists can preserve their traditional goals and methods (explaining behavior using participant observation, interviews, etc.) while at the same time taking a scientific approach to data collection, social scientists can move toward a more unified theoretical commitments which will ultimately result in more progress.

# WORKS CITED


Ariew, Andre. “Ernst Mayr’s ‘Ultimate/Proximate’ Distinction Reconsidered and Reconstructed.” *Biology and Philosophy* 18 (2003): 553–565.

Armstrong, David. *What is a Law of Nature?* Cambridge, Mass.: Cambridge UP, 1978.

Atkinson, Paul, and Martyn Hammersley. *Ethnography: Principles in Practice*. London: Tavistock Publications, 1983.

Barkow, Jerome H., Leda Cosmides, and John Tooby. “Introduction: Evolutionary Psychology and Conceptual Integration.” In *The adapted mind: Evolutionary psychology and the generation of culture*, edited by J. Barkow, 3–15. New York: Oxford University Press, 1992.

Cartwright, Nancy. *How the Laws of Physics Lie*. New York: Oxford University Press, 1983.

Cerroni-Long, E.L. "Anthropology at Century's End." In *Anthropological Theory in North America*. Edited by E.L. Cerroni-Long. Westport, CT: Bergin & Garvey, 1999.

Churchland, Paul M. “Eliminative Materialism and Propositional Attitudes.” In *Folk Psychology and the Philosophy of Mind*, edited by Scott M. Christensen and Dale R. Turner, 42–62. Hillsdale, N.J.: Lawrence Erlbaum Associates, 1993.

Cosmides, Leda and John Tooby. “The Psychological Foundations of Culture.” In *The adapted mind: Evolutionary psychology and the generation of culture*, edited by J. Barkow, 19–136. New York: Oxford University Press, 1992.

Davidson, Donald. *Essays on Actions and Events*. Oxford: Clarendon Press, 1980.

Dubisch, Jill. *In A Different Place: Pilgrimage, Gender and Politics at a Greek Island Shrine*. Princeton, N.J.: Princeton UP, 1995.

Ellis, Bruce J. "The evolution of sexual attraction: Evaluative mechanisms in women." In *The adapted mind: Evolutionary psychology and the generation of culture*, edited by J. Barkow, 267–288. New York: Oxford University Press, 1992.

Fisher, Michael M. J., and George E. Marcus. *Anthropology as Cultural Critique: An Experimental Moment in the Human Sciences*. Chicago: The University of Chicago Press, 1986.

Gabrieli, John, et al. “Cultural Influences on Neural Substrates of Attentional Control.” *Psychological Science* 19 (2008): 12–17.

Garfinkel, Alan. “Reductionism.” In *The Philosophy of Science*, edited by Richard Boyd, Philip Gasper, and J.D. Trout, 443–459. Cambridge, Mass.: The MIT Press, 1991.

Gillett, Carl. “Hyper-Extending the Mind? Setting Boundaries in the Special Sciences.” *Philosophical Topics* 35 (2007): 161–188.

Harris, Marvin. “Science, Objectivity, Morality.” In *Anthropological Theory in North America*, edited by E.L. Cerroni-Long, 77–84. Westport, CT.: Bergin & Garvey, 1999.

Hempel, Carl G. *Philosophy of Natural Science*. Englewood Cliffs, N. J.: Prentice-Hall, 1966.

Horgan, Terence, and James Woodward. “Folk Psychology is Here to Stay.” In *Folk Psychology and the Philosophy of Mind*, edited by Scott M. Christensen and Dale R. Turner, 144–166. Hillsdale, NJ: Lawrence Erlbaum Associates, 1993.

Jackson, Frank. *From Metaphysics to Ethics: A Defense of Conceptual Analysis*. New York: Oxford University Press, 1998.

Kim, Jaegwon. “Epiphenomenal and Supervenient Causation.” *Midwest Studies in Philosophy* 9 (1984): 257–270.

Kim, Jaegwon. “Explanatory Realism, Causal Realism, and Explanatory Exclusion.” *Midwest Studies in Philosophy* 12 (1988): 225–239.

Kim, Jaegwon. “Mechanism, Purpose, and Explanatory Exclusion.” In *Supervenience and Mind: Selected Philosophical Essays*, 237–264. Cambridge, U.K.: Cambridge University Press, 1993.

Kim, Jaegwon. *Physicalism, or Something Near Enough*. Princeton, N.J.: Princeton University Press, 2005.

Kincaid, Harold. "Reduction, explanation, and individualism." *Philosophy of Science* (1986): 492-513.

Kincaid, Harold. “Defending Laws in the Social Sciences.” *Philosophy of the Social Sciences* 20 (1990): 56–83.

Kincaid, Harold. “There are Laws in the Social Sciences.” In *Contemporary Debates in Philosophy of Science*. Edited by Christopher Hitchcock, 168–186. Malden, Mass.: Blackwell Publishing, 2004.

Kitcher, Philip. *Vaulting Ambition: Sociobiology and the Quest for Human Nature*. Cambridge, Mass.: The MIT Press, 1985.

Kitcher, Philip. *Abusing Science: The Case against Creationism*. Cambridge, Mass.: The MIT Press, 1998.

Kitcher, Philip. "Giving Darwin His Due." *Columbia University*. http://www.columbia.edu/~psk16/darwin.htm.

Layton, Robert. *An Introduction to Theory in Anthropology.* Cambridge, Mass.: Cambridge University Press, 1997.

Lett, James. *Science, Reason, and Anthropology: A Guide to Critical Thinking*. Lanham, MD.: Rowman & Littlefield, 1997.

Lewis, David. *Counterfactuals*. Cambridge, Mass.: Harvard UP, 1973.

Little, Daniel. *Varieties of Social Explanation.* Boulder, Colo.: Westview Press, 1991.

Mead, Margaret. *Coming of Age in Samoa.* New York: HarperCollins, 2001.

Mandelbaum, Maurice. "Societal facts." In *The Philosophy of Social Explanation*, edited by Alan Ryan, 105–118. New York: Oxford University Press, 1973.

McLaughlin, Brian P. "On Davidson's Response to the Charge of Epiphenomenalism." In *Mental Causation*, edited by John Heil and Alfred Mele, 27–40. Oxford: Clarendon Press, 2003.

O'Connor, Timothy, and John Ross Churchill. "Is Non-reductive Physicalism Viable within a Causal Powers Metaphysic?" In *Emergence in Mind,* edited by Cynthia Macdonald and Graham Macdonald, 43–60. New York: Oxford University Press, 2010.

Pinxten, Rik. *When the Day Breaks: Essays in Anthropology and Philosophy*. Frankfurt: Peter Lang Verlag, 1997.

Popper, Karl. *Conjectures and Refutations*. London: Routledge and Kegan Paul, 1963.

Roberts, John T. "There are no Laws of the Social Sciences." In *Contemporary Debates in Philosophy of Science*. Edited by Christopher Hitchcock, 151–167. Malden, Mass.: Blackwell Publishing, 2004.

Rosenberg, Alexander. *Philosophy of Social Science*. Boulder, Colo.: Westview Press, 2008.

Rosenberg, Alex, and Daniel W. McShea. *Philosophy of Biology.* New York: Routledge, 2008.

Ruse, Michael. "Creation Science Is Not Science." *Science, Technology, and Human Values* 7 (1982): 72–78.

Sober, Elliot. "The Multiple Realizability Argument against Reductionism." *Philosophy of Science* 66 (1999): 542–564.

Strinati, Dominic. *An Introduction to Theories of Popular Culture.* London: Routledge, 1995.

Wilson, E.O. *Consilience: The Unit of Human Knowledge*. New York: Alfred A. Knopf, 1998.

Woodward, James. "Explanation and Invariance in the Special Sciences." *British Journal for the Philosophy of Science* 51 (2000): 197–254.

Yablo, Stephen. "Mental Causation." *The Philosophical Review* 101 (1992): 245–280.