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**An Inquiry into the
Generic Social Processes of Science**

by

Robert Arthur Campbell

A thesis
presented to the University of Waterloo
in fulfillment of the
thesis requirement for the degree of
Doctor of Philosophy
in
Sociology

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An Inquiry into the Generic Social Processes of Science

Abstract

This project has three specific objectives: (1) to establish a rationale for a symbolic interactionist approach to the study of science, as opposed to other approaches within social studies of science, (2) to demonstrate the viability of this approach through an initial and partial application in a fieldwork setting, and (3) to draw attention to some particular aspects of the everyday practical accomplishment of the scientific enterprise that have been unduly neglected. The first objective is accomplished initially through a systematic critical examination of previous efforts to understand science from a philosophical, historical, and sociological perspective, and finally through a description of what a symbolic interactionist approach to the study of science would look like. The second objective is accomplished through the carrying out of empirical research (open-ended interviews) among practicing academic research scientists (biologists, chemists, geologists, and physicists), and organizing, analyzing, and presenting the data gathered in terms of a "generic social process" scheme, that is consistent with symbolic interactionism. The third objective is accomplished through the presentation of data on various aspects of the scientific enterprise, with a particular emphasis on activities related to pursuing funding and managing equipment. The data provide an interesting glimpse into the mundane aspects of the daily lives of scientists that has generally not been forthcoming in other social studies of science, and the study ends with the statement of six positions that reflect both the implications and benefits of adopting a symbolic interactionist approach to science. They are as follows: (1) there is no one science, (2) there is no one way to study science, (3) science is demarcated from non-science on the basis of a boundary negotiated among scientists, members of other pursuits, and the public, (4) the most promising unit of analysis for the study of science is the community, (5) symbolic interactionism allows for a science of science, and (6) science is what scientists do.

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I also want to thank all of the scientists who gave freely of their time to discuss various aspects of their lives and work with me.

Finally, I want to thank my wife Carole for agreeing to let me go back to school in my forties to complete a task started many years ago.

My only regret is that I was unable to finish the whole process in time for my father to see it done.

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Chapter One

INTRODUCTION

Approaching the Study of Science

The scenery in the play was beautiful, but the actors got in front of it.

Alexander Woolcott

I was raised to revere science second only to God. Not only were these two things sacred, but the formal study of either was meant for those individuals who were destined to become integral members of the communities of faith that constituted their institutional presence in society. The idea that someone would actually stand outside of science to examine what science was about or what scientists did seemed ludicrous to me. Yet, like many others before me, I find myself plunging into one of the most controversial and contentious areas in modern scholarship (the other, of course, being religion).

(personal reflection)

The present project has three specific objectives: (1) to establish a rationale for a symbolic interactionist approach to the study of science, as opposed to other approaches within social studies of science, (2) to demonstrate the viability of this approach through an initial and partial application in a fieldwork setting, and (3) to draw attention to some particular aspects of the everyday practical accomplishment of the scientific enterprise that have been unduly neglected. The intention is to make a positive contribution to our understanding of science, while also advocating an alternative approach to the study of science. The advantage of the symbolic interactionist perspective lies primarily in its emphasis on the exploration of human group life through direct interaction with the actors or agents that participate in the subculture or social world being studied. However, in order to understand what might be new about this approach and its benefits for the study of science, we must first sketch out some aspects of what is already known from other studies of science. That is the initial task of this introduction.

As a starting point, I would like to draw attention to what are perhaps the two greatest difficulties of studying science, namely, (a) defining exactly what one means by the word "science" and (b) determining what methods are appropriate for the examination of such a phenomenon. Historians, philosophers, sociologists, and scientists themselves have all provided different answers to the questions of what science is and how it should be studied, with an astonishing variety of results. Even within these disciplinary areas, the diversity and complexity

of perspectives represented form the basis for seemingly endless controversy and debate. Yet, in spite of all of the scholarly contention surrounding it, science “as an activity” carries on and continues to offer clear indications of its success by way of new discoveries and breakthroughs. This success largely accounts for the perception of many people that science represents the ultimate human achievement. Just how science is accomplished, however, remains a mystery.

While there is little scholarly or lay consensus on a precise definition of “science,” there are a number of characteristics that emerge as people try to identify the “essence” of science. As Harriet Zuckerman (1988:513) indicates, some of the elements that have been put forward include the following:

a body of certified knowledge, ...a set of procedures for finding things out,
...a social enterprise, a culture or tradition, and a set of social arrangements
for developing, certifying, and communicating knowledge.

Ambiguity arises in our efforts to determine whether any or all of these aspects are necessary and/or sufficient to form a definitive view of science, or whether one aspect is more important than another, and if so under what conditions. As a basis for the discussions that follow, I will consider as scientific those endeavors that attempt to gain knowledge of the world through systematic empirical investigation.

For my purposes, science, like art or religion, will be approached as an area of human endeavor not “essentially” different from any other set of cultural practices or products. Likewise, the people who practice science – scientists – are not seen to be “essentially” different from those engaged in any other realm of human activity. The distinction between science and these other areas of practice is primarily one of situation and context. Thus, while an observer may be able to distinguish substantive knowledge and activities that seem unique to scientific inquiry, or some specific area of science, somewhat similar or parallel processes may be found among artists, auto mechanics, and apiculturists, as they engage their worlds in meaningful manners.

As Thomas Gieryn (1995:44) argues,

exposing the contingent, flexible, pragmatic, and (to a degree) arbitrary
borders-and-territories staked out for science is the start of a critical
evaluation of the consequences of such cultural classifications – not just
for intellectual but real-world lives.

Simply put, then, for the purposes of this project, science is what scientists say they do (see Gieryn, 1995; Barnes et al., 1996:140-168; Dolby, 1996:159-189; Taylor, 1996).¹

Now, to adopt such an approach, I need to be able to defend the argument that science is not “essentially” different from any other aspect of human group life. This means, in turn, that – as a fundamental starting position – I want to claim that science does not possess some a *priori* or inherent attribute that forms the basis for such a distinction. This is, in fact, a claim basic to this project.

Not surprisingly, since science is one of our most successful and securely established institutions, its boundaries have become deeply entrenched. Deeply entrenched conventions are easy to mistake for natural divisions and there are strong incentives which encourage such mistakes. Mistakes, however, they remain, and potentially expensive ones at that. (Barnes, et al., 1996:168)

With respect to understanding science, the costs of such mistakes are the artificial closure of debates and the maintenance of unexplored assumptions. It is my view that any distinct qualities that may exist to differentiate science from non-science are “constructed” by the people engaged in “doing” science. In other words, the meaning of science emerges from the social interactions of scientists in their day-to-day work, as they engage in a variety of activities that they have determined make up their world. This stance is consistent with a symbolic interactionist approach to science that would focus on the active dynamic nature of “science as practiced.” It is the establishment and application of just such an approach to science that constitutes the bulk of this work.

If there is a common thread among perspectives on science, it appears to be that the members of the scientific community are engaged in the production of “viable” knowledge.² Whether one subscribes to an “internalist” view that this knowledge is arrived at through the systematic application of the “scientific method,” or to an “externalist” view that outside social and political considerations contribute to the “construction” of scientific knowledge, our perceptions of knowledge and science seem extensively interwoven. One basis for this linkage

¹ Charles Taylor (1996) provides a thorough exploration of the “rhetoric of demarcation” that scientists employ to differentiate themselves from members of other groups.

² I use the word “viable” here to express the notion that science produces knowledge that is capable of living, of growing and developing.

is the association of scientific knowledge with the pursuit of truth (see Fuchs, 1992; Shapin, 1994; Solomon, 1995). It is perhaps because of this conceptual association that philosophers were the first group of scholars to devote a great deal of attention to analyzing the scientific enterprise. As will become clear throughout the following chapters, one consequence of the linkage between scientific knowledge and truth is that certain philosophical debates have tended to dominate the research agenda of most groups interested in studying science. What follows is a brief survey of the philosophy, history, and sociology of science, all of which will be explored in more detail in later chapters.

The *philosophy of science* is concerned primarily with arguments about the ontological and epistemological foundations of science (see Feigl and Brodbeck, 1953; Suppe, 1977; Hacking, 1983; Kitcher, 1993; Losee, 1993; Rouse, 1996; Couvalis, 1997). That is, philosophers want to know "how the world is," and what forms the basis for determining truth about the world. One major issue that arises from these concerns is the so-called "demarcation problem," associated first with the Vienna Circle (see Ayer, 1959) and then with Karl Popper (1959), which involves trying to establish a basis for demarcating science from non-science. One common component of the answers offered by philosophers is that scientific knowledge is knowledge about a real world "out there." In other words, the truth or accuracy of scientific theories is determined by how well they "represent" nature, which is often considered to be fixed or given (see Hacking, 1983; Leplin, 1984; Giere, 1988). Similarly, scientific investigations are seen to employ strictly defined notions of argument and evidence, based on systematic logical forms of reasoning (see Newton-Smith, 1981; Brown, 1988). This "rationality" assumption also leads to the notion that science progresses, and that scientific knowledge accumulates as more research is carried out. Exactly how science progresses and by what means scientists determine when they have acquired new knowledge, however, are open questions. As will become clear, science is far messier than most philosophical descriptions allow, and solutions to the kinds of problems just mentioned have failed to emerge through the application of logical reasoning alone.

In contrast to philosophical approaches, the *history of science* consists largely of people describing the circumstances and events surrounding the discovery or invention of particular

scientific facts, laws, or theories (see Williams and Steffens, 1978; Ronan, 1982; Hall, 1983; Brush, 1988; Lindberg, 1992). The portraits that are provided of scientists and their work help us to see how the modern corpus of scientific knowledge was constructed piece by piece.

Historians also look for details of social, political, economic, or religious attitudes or events that may have facilitated or inhibited a particular series of occurrences in science. In this way, they provide not only a chronology of scientific progress, they also draw attention to the conditions of community life (and elements of error, myth, and superstition) that may surround science. The more details they uncover, the more complex their task becomes, and the more complex our view of science becomes. The history of science, then, provides us not just with an accounting of the "accomplishments" of science, but with pictures of the temporal and cultural contexts within which science is carried out. As with philosophical studies of science, historical studies are limited in what they can tell us about the scientific enterprise, particularly with respect to the details of daily scientific practice. The reason for this is simply that philosophical and historical studies of science, for the most part, have not been based on the direct observation of scientific practice in the here and now. Instead, they have been based largely on scientists' *ex post facto* accounts of particular experiments and discoveries.

The *sociology of science* is one of the more recent approaches to the study of science. The first research in this field was carried out by Robert K. Merton [1938](1970), who established a link between the Puritan ethic and the rise of modern science in the West. Merton's approach to science was strongly influenced by the ideas of the sociologists Talcott Parsons and Max Weber, and by the dominant view of science in his day, namely, that associated with positivism and what is often called exceptionalism (see Feigl and Brodbeck, 1953; Hacking, 1983). Exceptionalism is the name given (see Mannheim, 1952) to the view that the inner workings of science, consisting of the methodology and the substantive findings of science, are outside the purview of sociology (see Merton, 1973:268). This meant that for Merton the sociology of science was largely confined to studying science as an institution, regulated by certain norms and values. The norms of science, like those of any cultural institution, exist because they function to advance the goals of that institution. Mertonian-inspired research tended to focus on

how the reward structure of science functioned to promote the creation of certified knowledge, the institutional goal of science. Similarly, this research showed how the ethos of science was maintained through systems of positive and negative sanctions. While drawing attention to incidences of deviance and conflict in science, Mertonian sociology of science dealt more with how science *ought* to be done, rather than how science *is* done. Part of the argument I present here is that scientific knowledge is not exceptional and that what is needed is a sociological approach that advocates the open exploration of the scientific enterprise to see what is going on in science on a day-to-day basis.

A major change occurred in all three of these approaches to science when Thomas Kuhn [1962](1970) demonstrated the inadequacy of philosophical arguments to account for the events that took place in the history of science. If Kuhn is correct, as George Couvalis (1997:89) says, then we might conclude that "science is not an enterprise which has led to a growth of knowledge on the basis of objective standards that can be plausibly articulated as general rules." Building on the work of Ludwik Fleck [1935](1979), Kuhn emphasized the importance of taking a sociological approach to the study of science, one that highlighted the activities engaged in by members of the scientific community.

What followed from Kuhn's work was the development of approaches to the study of science that challenged many of the foundational concepts that had been taken for granted by those who had until then undertaken research within the philosophy, history, and sociology of science. Many, though certainly not all, philosophers, historians, and sociologists began to view science more explicitly as a "social construction." A new "sociology of scientific knowledge" (see Bloor, 1976) emerged that encouraged researchers to re-conceptualize distinctions between both the internal and external workings of the scientific enterprise and its cognitive and social elements. Although these new research initiatives have been regarded by many as controversial,³ the transition initiated by Kuhn and others provided a rationale for sociologists to enter the laboratories to study scientists *in situ*. The aim of these studies was to gather empirical evidence from practicing scientists that could shed light on issues that both logical analysis and

³ Steve Shapin (1995:292) observes that science studies are not for the "cardiovascularly challenged."

historical research had failed to resolve in a convincing manner. These so-called “laboratory studies” went a long way toward demonstrating the inadequacy of existing philosophical and historical accounts of the scientific enterprise, as well as showing that the more restricted Mertonian sociological approach to science provided only a partial, if not ideological, glimpse at scientific practice. However, in spite of the initial empirical success of these studies, many researchers have chosen to abandon what, from a neo-positivist perspective, might be considered the “epistemologically suspect” activity of carrying out ethnographic fieldwork. Instead, as Michael Lynch (1993:105) indicates, they appear to prefer engaging in what are perceived to be “more respectable academic pursuits” like historical and textual analysis.

A broader *social study of science*, or cultural study of science (see Labinger, 1995; Rouse, 1996), has emerged from the sociology of scientific knowledge as a new variant on the sociology of science. More explicit emphasis is given to the cultural and practical aspects of accomplishing the scientific enterprise, while not eliminating the more traditional concerns of philosophical, historical, and sociological studies of science.⁴ Surprisingly, however, in advocating a more practice-oriented appreciation of the scientific enterprise (see, e.g., Pickering, 1995), research in this area has not been grounded, to the extent that one might imagine, in the empirical investigation of the actual practice of contemporary scientists. This is puzzling given the manner in which the rationale for these studies is often framed. For example, Joseph Rouse (1996:238) conceives of cultural studies of science as including

various investigations of the practices through which scientific understanding is articulated and maintained in specific cultural contexts and translated and extended into new contexts.

⁴ For many scholars (see e.g., Bloor, 1976; Knorr-Cetina, 1981; Haraway, 1988; Woolgar, 1988b; Fuchs, 1992; Gibbons, 1994; Shapin, 1995; Barnes, Bloor, and Henry, 1996; Roth, 1996), the sociology of science is considered to be a branch or subfield of the sociology of knowledge. There are two bases for this position. First, there is the perception that some relationship exists between all forms of social activity and the production of knowledge. This idea, in fact, might be considered the central notion of the sociology of knowledge (see Mannheim, 1952; Berger and Luckmann, 1966; Curtis and Petras, 1970; Merton, 1973; Longino, 1990; Gillespie, 1991; Shapin, 1994). Second, science is considered to be the area of human activity that is especially suited to providing us with our knowledge of the world. The break with this position that is important for the social study of science is to see science as not *simply* being about the production of knowledge. The close examination of the daily life of scientists may reveal a variety of other goals towards which the activities of scientists are directed.

Taking the case of a specific laboratory procedure like electrophoresis, for example, we might be interested in understanding some of the following issues. How did scientists establish the protocol for carrying out this procedure and interpreting its results? How did scientists in other laboratories come to learn about and apply this procedure to the exploration of similar or different phenomena? Does it matter whether this procedure is being carried out in Canada or Japan, or whether by a graduate student or a technician? Will scientists working on plant proteins have a different understanding of this procedure from those working on animal proteins? Surely, the investigation of questions like these should include the empirical exploration of science as it is being practiced.

While Rouse is still primarily concerned with developing a philosophical understanding of science, he (1996:259) makes the important point that, in part at least, science studies should "participate in constructing reliable and authoritative knowledge of the world by critically engaging with the scientists' practices of making meanings." Where the present study differs from Rouse's approach is with respect to what constitutes the appropriate means for securing reliable and authoritative knowledge of science. Consistent with a symbolic interactionist perspective, I contend that this knowledge should stem from our understanding of the processes of interaction and interpretation engaged in by the members of the scientific community.

The Present Project

This project fits more or less directly into the approach that I have called the social study of science. More specifically, it adopts a symbolic interactionist (see Mead, 1934; Blumer, 1969; Prus, 1996) theoretical approach to the study of science as a form of day-to-day "practical accomplishment." While a more detailed rationale and description of this approach will be given below, the fundamental basis of symbolic interactionism is the notion that people attribute meanings to the objects they encounter in their daily lives through processes of social interaction and interpretation.

As stated at the outset, this project has three specific objectives: (1) to establish a rationale for a symbolic interactionist approach to the study of science, as opposed to other

approaches within social studies of science, (2) to demonstrate the viability of this approach through an initial and partial application in a fieldwork setting, and (3) to draw attention to some particular aspects of the everyday practical accomplishment of the scientific enterprise that have been unduly neglected. The first of these objectives is accomplished initially through a systematic critical examination of previous efforts to understand science from a philosophical, historical, and sociological perspective. The "upshot" of this review is that, while valuable insights into the nature of the scientific enterprise have been forthcoming, there are problems and shortcomings that might best be addressed through alternative approaches to the study of science. For example, social studies of science have tended to reflect a philosophical agenda, case studies have tended to be historical rather than contemporary, and much of this research has viewed science as being carried out within a normative institutional framework. As has already been implied, these emphases have provided a partial, limited, and in many ways distorted view of the scientific enterprise that still leaves us with many unanswered questions about the actual day-to-day practice of science (see Knorr-Cetina, 1995:141). Much of the work that has been done has served to restrict artificially the scope of science studies and leave scholars, particularly sociologists, wondering why they should be interested in studying science and how they might go about studying it. Thus, it is my contention that the most promising course of action is to see science as a form of human activity that is, in many ways, like any other. Symbolic interactionism recognizes those elements that are common to every aspect of human group life, while at the same time not obscuring those things that are unique to particular settings. The first objective is completed through a description of what a symbolic interactionist approach to science would look like. My basic argument here is that science emerges out of the daily processes of social interaction and interpretation that scientists engage in as they encounter the objects that constitute their life-worlds.

Integral to making such a claim is the fact that it can be substantiated in the "real world" in which scientists carry out their activities. Therefore, the second objective involves carrying out empirical research among practicing academic research scientists. The data gathered here are then organized in terms of a "generic social process" scheme, a conceptual framework that is

consistent with symbolic interactionism. This scheme not only facilitates the comparison of activities across multiple scientific settings, it also provides the basis for comparisons with the activities engaged in by people in other settings. In Chapter 5, I delineate the variety of generic social processes to be found in scientific activity.

The third objective is accomplished through the presentation of data on various aspects of the scientific enterprise; specifically I will focus on the generic social processes associated with *pursuing funding* and *managing equipment*. These important daily activities in which scientists engage have not received adequate attention in the literature to date, and yet it is difficult to conceive how the empirical investigation of the world could proceed without adequate material resources and the finances required to purchase, maintain, and apply those resources. Choosing to focus on these two areas allows me to explore aspects of science that are part of the daily experience of all scientists as well as that of members of other human groups. The remainder of this section provides a more detailed examination of all three objectives.

As a theoretical perspective, symbolic interactionism is most centrally associated with George Herbert Mead (1934) and Herbert Blumer (1969). The ideas of symbolic interactionism, however, are rooted in the American pragmatism (see Shalin, 1986) of John Dewey (among others) and the ethnographic urban research tradition that was developed at the University of Chicago in the early part of this century (see Kurtz, 1984). The principal focus of this perspective is the intersubjective construction of meaning that develops as people, in the process of ongoing group life, try to make sense of the world in which they live. These interaction processes are predicated on symbolic communication, of which the primary example is language. However, symbols (and language) are not considered to be static entities. Rather, symbolic interactionism stresses the emergent and changing nature of meaning that arises as people engage in the day-to-day activities that make up their lives. The interactionist emphasis on human action is paramount, and research carried out in this tradition details the ways in which people construct their life-worlds through processes of interaction and interpretation.

A symbolic interactionist approach to science, then, would focus on the emergent and processual nature of the everyday life-worlds of scientists as they interact with other members of

the scientific community and engage in the activities that constitute their work. In this view, science neither represents a pre-existing objective reality nor a subjective construct of individual minds. Rather, science is the intersubjective accomplishment of a group of people, who systematically study phenomena of various sorts. The research task for the sociologist is neither to question why scientists do the things they do nor to determine whether the activities they engage in are or are not "scientific," according to some "essentialist" or supposedly objective criteria. The goal of interactionist science studies is to describe how the members of a particular human group accomplish the phenomenon they call science. So, to take the position that science is what scientists do is to argue that science is constituted of the meanings that emerge as scientists interpret, reinterpret, and communicate about, the activities they engage in.

The method that this project employs to gather, analyze, and present data is derived from the ethnographic emphasis of symbolic interactionism. In particular, it makes use of the "generic social process" (GSP) scheme developed by Robert Prus (1987, 1996). Ethnographers (see Hammersley and Atkinson, 1995) emphasize the task of achieving an "intimate familiarity" with the group being studied and, while research of this kind has provided great insight into many aspects of human group life, what has been missing is a mechanism for transferring these insights across a variety of contexts and situations. In other words, the specificity of these studies means that they have remained largely isolated from one another. Prus (1996:142) defines generic social processes as "the transsituational elements of interaction – the abstracted, transcontextual formulations of social behavior," and he argues that the GSP scheme provides a way to maximize conceptual development in qualitative sociological research by providing "transsituational reference points that enable scholars to compare and contrast ethnographic studies in many contexts" (1996:25). Whether a researcher is studying athletes, accountants, or members of any other human group, the insights gained in one context can help to illuminate what goes on some place else. It is this ability to move beyond a single setting that makes the GSP scheme so important for the researcher, and it is the opportunity to take the lessons learned through the study of science into other contexts, and vice versa, that makes the present project particularly pivotal. This goal can be best accomplished when fieldwork is carried out in a variety

of settings using the GSP scheme as a framework for all aspects of the research process. While the material presented later in this work offers a starting point for such comparisons, researchers have yet to make use of this scheme in a sufficient number and variety of contexts to permit the kind of conceptual development and increased understanding of human group life envisioned by Prus.⁵

The GSP scheme, as developed by Prus, is organized around what he refers to as basic elements of human endeavor, namely, acquiring perspectives, achieving identity, getting involved, doing activities, developing relationships, and forming and coordinating associations. By focusing on these processual elements, the details of the collective human acts of interaction and interpretation can be obtained and presented in ways that retain their natural qualities. At the same time, these specific processes do not represent a rigid typology designed to constrain our view of what goes on in science, or in any other subcultural realm. Rather, they constitute a heuristic device through which the intersubjective accomplishment of all areas of human endeavor can be examined and assessed, in a self-referencing way. That is, the research process itself critically mirrors the activities being examined. Data, and the concepts used to describe that data, emerge through ongoing processes of social interaction and interpretation.

The data for the present project were gathered through a series of open-ended interviews with a number of practicing academic research scientists (see Chapter 5 for details of the sample) in the so-called basic or natural sciences (biology, chemistry, geology, physics). In order to provide more comprehensive support for the contention that science is what scientists do it would be desirable to undertake extended periods of participant observation in at least a couple of distinct settings within all four of these disciplines. How such a research agenda could be carried out by any individual is difficult to imagine, and ideally the most benefit would come from eight or more researchers carrying out their fieldwork simultaneously. They could then

⁵ In a volume edited by M. L. Dietz, R. Prus, and W. Shaffir (1994), a number of ethnographers present findings from their fieldwork in terms of generic social processes. While this collection goes a long way in demonstrating the potential of the GSP scheme, there are two major weaknesses. First, the majority of these studies were not carried out with the GSP scheme in mind and therefore the chapters have a certain artificial quality. Second, while the individual chapters are grouped together around particular GSPs, there is no real effort to do the kind of comparative synthesis advocated by Prus.

meet regularly to compare notes and further develop an understanding of what they were observing through the very processes of interpretation and interaction that they were observing in the field. The generic social process scheme is ideally suited to facilitating this kind of large-scale multiple site research initiative, but surely a first step towards such a concerted effort is to establish an appropriate framework for carrying it out and provide at least some demonstration of the expected outcome of such an effort. This is the limited objective of this initial foray into the symbolic interactionist study of science within a GSP framework.

The second objective of the present project clearly states that this is an initial and partial exploration of the scientific enterprise. I thought that it was more important at this point to gather *some* information from a variety of informants in different contexts, than to limit myself to an examination of a single location in the hope of establishing a more comprehensive view of that particular setting. As will be discussed in the next couple of chapters, previous efforts to study single sites of scientific activity have not produced the yield that we might expect when we take into account the amount of time that researchers have spent in those settings. As has already been mentioned, part of the reason for this is that investigations of these settings have been directed by efforts to follow an agenda established by philosophers of science. Just as important, however, is the fact that during the extended periods of participant observation that these researchers engaged in, insufficient effort was expended in talking to scientists in an open and exploratory fashion to see what the scientists think is going in their life-worlds, and how they interpret the objects they encounter on a daily basis. Therefore, a critical aspect of the present study is the very act of "talking" to scientists and reporting on what it is they have to say about science.

It is difficult to conceive of how any particular research project could hope to encompass the whole of science, whatever that may be, and so I devote special attention to two little-studied but consequential aspects of science, namely, *pursuing funding* and *managing equipment*. I have done so for four reasons. First, funding and equipment related activities have been unduly neglected. Second, a focus on epistemological concerns has diverted effort away from fieldwork. Third, the activities engaged in with respect to funding and equipment are common to

all scientific settings. Fourth, similar activities are carried out by members of other human groups. Much of the material introduced in the following discussion will be developed at greater length throughout the next few chapters and in the chapters devoted specifically to funding (Chapter 7) and equipment (Chapter 8).

To begin with, the activities around funding and equipment have been selected because they have not received adequate attention in the literature to date. Scientists spend a great deal of time engaging in a variety of activities centered on funding and equipment. As part of his exploration of the relationship between commercial and academic interests in the emerging field of biotechnology, Martin Kenney (1986:18) states that as much as 30 percent of a scientist's time can be devoted to the grant application process. Similarly, Stevenson and Byerly (1995:133) observe that with increased competition and the rising cost of science, "many scientists now find themselves spending a lot of their time preparing detailed grant applications, many of which will be unsuccessful." Just exactly how much time is spent on various funding-related activities in different disciplines remains unclear, however, because no one has done any concentrated research into "how" scientists deal with the funding process. With respect to equipment, in a discussion of the relationship between material resources and scientific productivity, Bruce Hevly (1992:358) states that "the scientists' material world is primarily a technological one, and the technological needs and opportunities have to be counted as strong influences on scientific practice." As with funding-related activities, it is precisely at the level of practice, where our understanding of how scientists think about, talk about, and make use of equipment is inadequate. It is not enough to state that these issues are important and time-consuming, and leave it at that.

To argue that issues related to funding and equipment have been unduly neglected requires some initial clarification. To the extent that research on these issues has been carried out, it has tended to take the form of more macrosociological and functional analyses, that emphasize how financial support and material resources are related to what many would consider to be the institutional goal of science, namely, the growth of knowledge. Government officials, politicians and agency bureaucrats alike, members of the public, and scientists

themselves, argue that financial and material resources are integral and pivotal to the successful operation of the scientific enterprise on a large scale. Big science is big news, and high levels of funding and massive equipment or extensive facilities are often identified as essential elements of big science (see Galison and Hevly, 1992). Members of the public are well aware of the controversies and accomplishments associated with the Hubble telescope (see Chaisson, 1994), for example, and a great deal of public attention surrounded the cancellation of construction on the Superconducting Supercollider (see Ritson, 1993).⁶ Other high profile issues in science include the global effort to fight both cancer (see Fujimura, 1996) and AIDS (see Shiits, 1987), as well as the Human Genome Project (see Cook-Deegan, 1994), which promises to produce a formula or code for human life. Particularly with respect to these large-scale enterprises, science appears to be intimately tied into issues of social welfare, and members of various groups are anxious to understand the relationship between investment in scientific research and the common good.

Where the neglect of funding and equipment related issues is most surprising is in the more microsociological, particularly laboratory-based, studies of science that have been carried out as part of the newer post-Kuhnian and post-Mertonian research in the sociology of science. The three major (book-length) ethnographic studies of science are those of Bruno Latour and Steve Woolgar [1979](1986), Karin Knorr-Cetina (1981), and Sharon Traweek (1988). While these authors draw attention to the importance of funding and equipment in the accomplishment of scientific goals, the detailed exploration of these activities is generally sidestepped on the way to addressing questions of epistemological interest. For example, Latour and Woolgar (1986:88) portray the activity that takes place in the laboratory as "the organisation of persuasion through literary inscription." The various instruments and pieces of equipment (inscription devices) found in the laboratory produce readable output (e.g., squiggly lines on chart paper) that the scientists "transform" into scientific papers, journal articles. The details of how facts (the contributions to

⁶ On a personal note, I find it curious that the resources were found to build an instrument designed to give us a glimpse at the origins of the universe, but sufficient support could not be garnered to construct a device that would allow us to recreate in part, on our own planet, the physical conditions that existed at the time when the universe came into existence.

knowledge found in these papers) are constructed are conveniently “forgotten” when it comes to writing up results. That is, in print, scientists emphasize the product of their activities, not the processes of production. Ironically, this is precisely what happens with Latour and Woolgar. They become so focused on the products of the scientific enterprise that they “forget” to explore, in sufficient detail, how those products are arrived at through the daily activities of scientists. Knorr-Cetina, on the other hand, while (1981:94) conceding that published articles may be the primary “end-product” of scientific research, is more concerned with demonstrating the indeterminacy and contextual contingency of the processes of scientific reasoning that give rise to scientific knowledge. As part of this clearly epistemological project, she (1981:83-87) draws attention to the “resource-relationships” within which scientists carry out their work. For example, the scientists she studied indicated to her the importance of “the art of writing a grant proposal,” and yet, while we learn something about why the scientists placed so much emphasis on this process, we learn almost nothing about the activities that constitute this “art.” As with Latour and Woolgar, Knorr-Cetina does not pause to explore in a sustained manner what goes on, with respect to funding and equipment, as science is carried out on a day-to-day basis.

Sharon Traweek (1988) focuses more directly on how issues related to funding and equipment are integrally tied into notions of the individual scientist’s identity and involvement, as well as how these matters are taken into account by members of the broader high energy physics community. High energy physics is big science, and at the heart of this discipline is a set of complex and expensive devices known as detectors, which are used to explore the fundamental building blocks of nature, by literally getting in the way of a beam of high energy particles. Part of Traweek’s objective is to delineate cultural differences between the way in which physics is carried out in Japan and the United States, but much of her concern is with how the “careers” of individual scientists are shaped. In this respect, Traweek’s study resembles the approach advocated in the present project, because it looks at the meaning that objects have for those that encounter them on a daily basis. However, she becomes preoccupied with what might be called a “phenomenology of detectors,” in much the same way that Latour and Woolgar develop a “phenomenology of inscriptions.” In both of these cases, the phenomena under study are being

examined primarily in order to determine how they contribute the production of knowledge and the resolution of epistemological debates born of the interests of the philosophers of science. At the same time, the neglect of funding and equipment related issues in laboratory studies is at least to some extent a function of the fact that so few ethnographic studies of science are being performed.

In his analysis of laboratory studies and his assessment of the crisis that occurred in relativist and constructivist studies in the 1980s, Michael Lynch (1993:90-105) indicates what he thinks went wrong with the laboratory studies initiative, and why other researchers are reluctant to carry out ethnographic studies of science. With respect to what went wrong, Lynch observes that many of those that performed fieldwork early in their careers have shifted their approach towards science. Steve Woolgar (see 1988a, 1988b), for instance, became more concerned with issues of discourse and reflexivity, while Bruno Latour (see 1987, 1988) turned to more historical studies of science in an effort to understand how "science in action" plays itself out through series of human and material resource networks. So, rather than forming the basis for sustained fieldwork, it appears to be the case that "more often the themes and research strategies from constructivist studies are used for interpreting archival materials and constructing historical modes of demonstration" (Lynch, 1993:103). Lynch offers the following explanation. First, the constructivist claim to have demonstrated the socially constructed character of the "content" of science, what philosophers of science refer to as belonging to the "context of justification," meant in large part that their goal had been accomplished, and they could move on to other pursuits. It is as if, having proven, at least to their own satisfaction, that the philosophical view of science was incorrect, sociologists of science could not see what benefits would accrue from a closer examination of the scientists' life-worlds. Second, two problems with single-site laboratory explorations seemed to persist. In the first place, these studies did not appear to hold up to reflexive scrutiny. That is, by studying individual "extraordinary" examples of science, typical of previous studies that they had criticized for this limited view, they failed to provide a basis for a broader, more intersubjective perspective on science that could only emerge through the exploration of multiple sites, representing a variety of circumstances. The second problem is

that laboratory studies ignored broader elements of sociological interest. Much of this criticism was based in the perception that these studies were too microsociological, and that there was no way to extend these studies to incorporate elements of community and institutions. In the absence of satisfactory resolution of these issues, however, "ethnographic studies remain exceedingly difficult to undertake, as they are not supported by the professional archives, established literatures, and communities that facilitate familiar modes of historical and sociological scholarship" (Lynch, 1993:104).

Lynch suggests that there are three pragmatic difficulties facing any would-be ethnographer of science. First, "cutting-edge" research is extremely difficult to carry out. Part of this has to do with gaining entry into an appropriate research site, and part has to do with the technical demands placed on the researcher, particularly with respect to learning a highly complex (scientific) language. Second, Lynch suggests that the audience for such studies needs to be tutored in the technicalities of the science being observed in order to be able to read the findings, and even then they are likely to view the results as "tedious reportage." Third, the "career demands" faced by academics mean that extended periods of fieldwork are "more suited to dissertation research," but even this has been stymied because there have been "very few graduate students in the field." In Chapter 5, I offer a challenge to all of these claims, by demonstrating that they reflect a rather limited view of the scope and methods of the social study of science. As Lynch (1993:105) observes, however, the low status accorded to laboratory studies and the difficulties associated with fieldwork have led to the common refrain that "there is little left to do inside the lab." It may be the case that no further insight into epistemological problems will be forthcoming from ethnographic studies of science, but that does not mean that much of sociological interest might not be waiting to be discovered. For example, the exploration of scientific practice may help us to understand how the identities of individual scientists, and the communities of which they are a part, are constructed, maintained, and changed. It may also help us to discover the similarities and differences between the ways in which scientists and members of other occupational groups achieve their goals. The material presented in chapters 7 and 8 shows how the funding and equipment related activities of

scientists have parallels with those carried by, among others, outlaw biker gangs, and it also supports a modified conceptualization of what it means to say that science is socially constructed.

In these last several paragraphs, I have been building on the work of Michael Lynch, in an effort to demonstrate that part of the rationale for studying issues related to funding and equipment in science is to move beyond a research agenda set by philosophers, in order to explore matters of broader sociological interest. If we move beyond a view that some of the activities that scientists engage in are clearly scientific, in that they contribute directly to the growth of (scientific) knowledge, while other activities are “merely” part of the mundane daily existence that scientists share with others, we can begin to demonstrate that science is of sociological interest precisely because it is a human group activity. The symbolic interactionist perspective is based on the notion that people attribute meanings to objects as they encounter them on a day-to-day basis. Therefore, our examination of science should be predicated on an effort to observe how meanings are arrived at and shared among practitioners, as they attempt to accomplish their goals. As has been stated, within this perspective, there is no *a priori* basis for determining what activities do or do not constitute science. Rather, if this kind of judgment is to be made, it should be reached through the empirical investigation of science as practiced. As a start, evidence from previous studies would appear to support the contention that pursuing funding and managing equipment are important aspects of the scientific enterprise.

The third and fourth aspects of the rationale for studying funding and equipment related issues in science are tied in to the search for a way to expand the sociological interest in science studies. Previous efforts to explore science have not only been bounded by adherence to a philosophical agenda they have been carried out for the most part on cases of “extraordinary” science. In other words, they have involved remarkable or exceptional individual scientists and their accomplishments – Nobel prize-winning scientists, and/or major discoveries such as pulsars, quarks, thyrotropin releasing factor, and so on. While some of these studies (see, e.g., Latour and Woolgar, 1979; Knorr-Cetina, 1981) have noted the ambiguous, tentative, messy, and circumstantial aspects of scientific practice, they have tended to concentrate on the details

of laboratory procedures and experiments, to the exclusion of more sustained examinations of the more ordinary, non-experimental, mundane activities that constitute the scientific enterprise. This is important because, just as scholars can find no clear demarcation between science and non-science in terms of its method or procedures (see Chapter 2), in general terms there is no consensus on what constitutes scientific versus non-scientific practice. As I will argue later, these disputes are largely irresolvable, and they certainly will never be resolved without more direct insight into what takes place in the day-to-day practice of ordinary science, as carried out by ordinary scientists. Part of the solution is to begin with an examination of those activities that are common to scientific research being carried out in multiple settings, by a broad array of scientists. As has been mentioned, issues related to funding and equipment are routinely raised by scientists and by those that study science, yet neither has received the kind of broad-spectrum investigation that would shed light on how scientists perceive and deal with these issues. Part of my argument here is that much of this neglect is due to the absence of an appropriate method for getting at these issues, and this leads to the fourth element of the rationale.

Finally, then, I am starting out this project with the assumption that the kinds of activities engaged in by scientists are similar to those carried out by members of other human groups, in ways that most sociologists of science, while perhaps recognizing, have failed to examine or develop systematically. Previous approaches to the study of science have been based on the assumption that there is something different about the scientific enterprise, but they have not been able to demonstrate precisely *how* science is different from other human endeavors. A symbolic interactionist approach to science would look for this difference to emerge out of the daily processes of interaction and interpretation that scientists engage in. Similarly, by employing the generic social process (GSP) scheme, we will be able to discern those ways in which science resembles other areas of group activity. In this way, we gain a broader sociological appreciation of the scientific enterprise, and of group life more generally.

In one sense, then, the present project offers a qualitative empirical study of selected aspects of the scientific enterprise, but it also offers an opportunity to examine and assess the

applicability of the generic social process scheme to exploring the life-worlds in which scientists work. So, for example, if the scientific enterprise is indeed “exceptional” in some sense, we may discover that social processes which take place in a variety of other settings do not take place within science. On the other hand, we may discover that the lack of attention that has been paid to the more mundane aspects of science can, in large part, account for the perception that science is exceptional. It may in fact be the case that, on a day-to-day basis, science looks very much like any other realm of social activity.

Despite being one of the most interesting, complex, and challenging areas of sociological research, the sociology of science has remained a marginal area, of little or no interest to many scholars. Part of the reason for this is the lack of a collectively sanctioned theoretical and methodological framework within which to carry out research activities on the ways that science is achieved in practice. Another significant element is the restriction imposed by treating the study of science rather exclusively as a subset of the sociology of knowledge (in a Mertonian sense). In other words, not only is the sociologist restricted from examining the inner workings of science, but any study of the “institution” of science must begin from the perspective that the institutional goal of science is the establishment of certified knowledge. It must accept science’s own normative definitions of itself while trying, through study, to see if such is indeed the case. It may also be the case that social researchers take for granted the activities of scientists, considering them too preset or boring for close analysis. More likely, there is a certain “fear” of studying science, as it is produced in the field, not only because of the difficulties in comprehending what is going on in science (see Lynch, 1985, 1993), but because there is a “threatening” aspect to this kind of research. “It is one thing to study prostitutes or addicts at some remove from the university, or to study a cult with few adherents. It is another to study the practice of what is, in fact, the dominant religion of one’s own place of work” (Star, 1989:2). The implied threat is that sociologists in studying science may undermine, or at least be perceived by some colleagues as challenging, the foundations of their own discipline. The present project offers at least a partial remedy for this situation.

The Remaining Chapters

Chapter 2, *The Establishment of Science Studies*, reviews in some detail the literature in the philosophy, history, and sociology of science. Both this chapter and the next flesh out the argument I made above about the broader area of science studies, outlining the contributions and shortcomings of a variety of theoretical and methodological approaches that have, to date, been brought to bear on this aspect of human group life. The sheer volume and diversity of these approaches makes a completely systematic or comprehensive coverage of this material more or less impossible. Thus, I have focussed my attention on certain kinds of literature. The following criteria for selection have been applied. First, an effort is made to introduce some of the key figures and debates in the study of science (e.g., Popper, Merton, Kuhn, and issues like the unity of science and social constructionism), so that the reader gains an appreciation of these areas as a whole. Second, specific initiatives, particularly those that contribute to an understanding of a symbolic interactionist approach to science, and thus help to underscore the rationale for the present study, are covered in more detail. The central figure on both of these counts is Thomas Kuhn. The initiative that marks the transition between the works covered in this chapter and the next is the so-called "strong programme" in the sociology of scientific knowledge, which offers a post-Kuhnian programmatic statement of how science should be studied by sociologists.

Chapter 3, *Studying Science by Studying Scientists*, examines the findings of a group of researchers who, in the late 1970s, went into the laboratories of practicing scientists to observe what was going on. The major work examined here is *Laboratory Life* (Latour and Woolgar, 1979, 1986), which is distinguished not only by being the first book-length work of this kind, but by illustrating how the evidence of scientific practice can lead to the conclusion that scientific facts are socially constructed in the laboratory setting. The three other major figures examined here are Karin Knorr-Cetina, Sharon Traweek, and Harry Collins. The first of these scholars has provided the most systematic statement of the social constructivist approach to science studies yet developed (see Knorr-Cetina, 1981, 1982, 1983a, 1983b, 1995) and has most clearly demonstrated what benefits accrue from studying scientists in the laboratory setting. Traweek

(1988) draws particular attention to the community aspect of scientific practice, and offers a comprehensive description of a "career" in science. Collins (1992) demonstrates the value of going into the laboratory to study normal/ordinary scientific activity.

Chapter 4, *Moving Science Studies Forward*, reviews four fairly recent attempts to establish alternate approaches to the study of science, that move beyond some of the internal conflicts that the authors identify as inhibiting research in this area. Susan Leigh Star's *Regions of the Mind* (1989) is based on an interactionist approach to science studies that is applied to an historical case study regarding the dispute over localisationist and diffusionist understandings of brain function. In his *The Professional Quest for Truth* (1992), Stephan Fuchs suggests taking an organizational approach to the study of science that would give a more prominent role to struggles for resources and jurisdiction among interested groups. Andrew Pickering's *The Mangle of Practice* (1995) emphasizes the dialectic nature of scientific practice and the compromises that emerge from the atmosphere of uncertainty in which scientists work. The final work examined here is an article by Stephen Barley and Beth Bechky (1994) from the journal *Work and Occupations*, entitled "In the Backrooms of Science: The Work of Technicians in Science Labs." Of the four works covered in this chapter, this one comes closest to the present project, in that it presents results of fieldwork carried out in a number of contemporary settings.

Chapter 5, *An Interactionist Approach to Science*, presents the theoretical and methodological foundations for the present study, along with specific details of how this study was carried out. It comprises three sections. The first section outlines the symbolic interactionist perspective within sociology, and indicates how this perspective is intimately linked to the ethnographic research tradition. The second section examines the details of the "generic social process" (GSP) scheme developed by Robert Prus (1987, 1996). This scheme provides the framework for the collection, analysis, and presentation of data from the fieldwork portion of this project. The third section of this chapter describes the setting and the sample in detail, along with the mechanics of how all phases of the project were accomplished.

Chapter 6, *An Emergent View of Scientific Practice*, provides data gleaned from interviews with academic research scientists that address a wide range of activities from within

the scientific enterprise. These include: getting ideas, assessing feasibility, getting prepared, implementing plans, obtaining results, and disseminating results. The emphasis throughout is on “doing” science. This chapter portrays the potential of adopting the symbolic interactionist approach to the study of “science as intersubjective accomplishment.” The next two chapters, which represent the primary empirical core of this project, draw particular attention to some specific aspects of the scientific enterprise.

Chapter 7, *Pursuing Funding*, provides data on how scientists go about obtaining the financial resources necessary for carrying out their work. Scientific research is generally costly, and government, industry, the public, and scientists are all concerned with how much money is being spent, how it is being spent, and what they are getting for their money. Despite the level of attention that scientists pay to funding matters, we know very little of how concerns with finances are dealt with on a day-to-day basis by practicing scientists. Scientists' funding-related activities include: identifying sources of funding, preparing grant applications, entering partnerships, marketing services, equipment and material, and making adjustments. A detailed examination of these activities will help us to understand how scientists, and others, deal with the various ambiguities, constraints, and opportunities that they encounter on a daily basis. Part of what the data show is that in exploring multiple sources for funding scientists communicate their ideas in diverse ways, sometimes emphasizing their personal accomplishments, sometimes indicating how their work fits into a larger ongoing project, or at other times drawing attention to the particular problem to be solved.

Chapter 8, *Managing Equipment*, presents data on activities such as selecting equipment, acquiring equipment, and maintaining and operating equipment. Part of the common perception of science is to associate the research process with elaborate and exotic pieces of equipment (electron microscopes, radio telescopes, and particle accelerators). The mystification associated with technology may blind us to the notion that it is the people who work with (and maintain) the technology who undergird the scientific enterprise. Understanding the part that these machines and devices play in science is a matter of exploring how scientists deal with these objects, as they carry out their research activities. Among other things, I suggest here that

the notion of the “black box,” a device whose inner workings are often taken for granted by scientists, should be associated more closely with a set of meanings than with a particular piece of equipment or technique.

The ninth chapter, *Evaluation and Implications*, contains a summary of the findings of this project and provides a list of suggestions regarding further research. As part of this final chapter, I review the so-called “science wars” (see Ross, 1996) to show how the continued dominance of a research agenda set by philosophers has led to a highly destructive dialogue in the social study of science. By taking a symbolic interactionist approach to the study of science researchers are provided with a flexible and naturalistic framework within which to arrive at an intersubjective understanding of what is going on in science. The generic social process scheme provides a mechanism for constructing a highly detailed understanding of the diverse and complex elements of science, while at the same time facilitating comparisons across situations and contexts. Also, by drawing attention to some little-studied areas of the scientific enterprise, this study demonstrates that there are seemingly unlimited opportunities for research on science, outside of some of the constraints that have previously been placed on such research.

Chapter Two

THE ESTABLISHMENT OF SCIENCE STUDIES

This chapter and the next establish a context for the present project by surveying some of the major contributions from the philosophy, history, and sociology of science. One characteristic of this literature is a trend towards the establishment of a more general approach to the study of science that would incorporate elements from these and other disciplines in such a way as to facilitate a sustained examination of the scientific enterprise. The selection of authors, concepts, and studies examined here provides a representative, though certainly not comprehensive, introduction to the social study of science for those readers that may not be familiar with the literature in this area. At the same time, the reviewed material is evaluated in terms of how it can be used to support my objective of establishing a rationale for a symbolic interactionist approach to the study of science.

The diversity and complexity of the material covered here makes it difficult to follow a strict thematic or chronological order for the presentation of ideas. Consequently, every effort is made to focus on what is most relevant for present purposes and to demonstrate why this is the case. The first area to be examined is the philosophy of science, with particular reference to the work of Karl Popper. This is followed by considerations of the pioneering work of Robert Merton in the sociology of science and the revolutionary work of Thomas Kuhn in the history of science. A fourth section introduces the so-called "strong programme" in the sociology of scientific knowledge that marks the transition from the more traditional approaches to the study of science covered in this chapter to the more recent "constructionist" initiatives discussed in the next chapter.

Philosophy of Science

The discussion that follows attends to four central issues within the broad range of philosophical concerns with science. These are: the demarcation of science, the unity of science, the relation of theory to evidence, and how scientific theories change. While a number

of alternate issues or ways of framing such issues could have been selected, my point in emphasizing these particular concerns is twofold. First, an examination of these issues allows me to demonstrate how in their efforts to understand science philosophers have created a whole series of problems which they have not been able to solve through the application of logic, or other philosophical methods. Second, some knowledge of the content and perceived importance of these problems is necessary to understand how much of the research in the history and sociology of science has been constrained by the philosophical agenda. A final subsection addresses the "pragmatic turn" in the philosophy of science (Solomon, 1995), that advocates a move away from the preoccupation with the search for truth that undergirds all of these issues (see Feigl and Brodbeck, 1953; Nagel, 1961; Hempel, 1966; Fuller, 1989; Laudan, 1990), towards the more direct empirical examination of scientific practice.

Ian Hacking (1983:1-6) identifies the major themes examined in the philosophy of science as belonging to what Hans Reichenbach (1938) called the "context of justification," as opposed to the "context of discovery," discussed later in this chapter. The idea of justification refers to the ways in which scientific ideas are tested and evaluated, regardless of their origin. In fact, Karl Popper (1963:31) argues that it makes no difference how an idea comes about, "whether it is a musical theme, a dramatic conflict, or a scientific theory." What philosophers of science are concerned with is explicating the internal workings of the scientific enterprise in terms of the logical consistency of the methods employed by scientists. The basis for this emphasis is the belief that it is the method of science that provides the foundation upon which scientific claims are justified. Philosophers generally frame their analyses in terms of ontology and epistemology, and, with respect to science, philosophers traditionally have tended to be realists and rationalists.

The focus of ontological inquiry is the determination of what the world is and what kinds of things are in it (see Leplin, 1984; Fine, 1986; Hooker, 1987). Philosophers want to determine if the entities postulated by scientists are real, or whether they are just constructs of the human mind. In other words, when scientific theories are accepted, they are to be accepted on the basis of their ability to represent some aspect of the world that does exist. As an example, for a

realist, it is not enough for atoms to be a useful theoretical construct. Rather, for realists, atoms must exist. Alternately, some philosophers argue against realism indicating that various historical scientific theories (e.g., phlogiston, ether, heliocentrism), while serving as useful approximations of reality, have turned out to be false (see Laudan, 1984). With respect to the study of science, historians and sociologists of science have tended to devote what I would consider to be an inordinate amount of effort to providing support for their acceptance or rejection of the realist position.

Turning to epistemology, philosophers want to determine how we really know, what counts as evidence, and what criteria we should use as a basis for our belief in some particular theory or other. The traditional responses to these questions have often been framed in terms of some form of rationality (see Newton-Smith, 1981; Hollis and Lukes, 1982; Brown, 1988). Going back to Aristotle, philosophical conceptions of rationality are generally categorical (see Brown, 1988:43-44; Giere, 1988:7). That is, rationality, or logical reasoning, is considered to be an essential characteristic of being human, and it is most manifest in science. Rationality protects us from answers to our questions that may be arbitrary or based on faith. As Brown (1988:35) argues, the value of rationality lies in providing us with "reliable" solutions to the problems we encounter, along with a way to "recognize" such solutions when they arise. While all human activity may be in some sense rational (see Hacking, 1983), science is considered to be the exemplar of rationality. A precise statement on what exactly constitutes rationality, however, is the subject of ongoing debate. Much of this debate has in turn been focused on determining the precise nature of science, since, scientific activity is commonly thought to be the most significant manifestation of rationality.

The demarcation problem

In light of the above focus, one of the most important issues that arises out of the philosophical study of science is the determination of what makes science different from other disciplines. This so-called "demarcation problem" is associated primarily with Karl Popper (1959:34-39), who sought to establish a criterion for differentiating between science and non-

science in terms of method. Consistent with the search for the logical foundations for science, as found in the works of Bertrand Russell and others, Popper set out to demonstrate how evidence of the rationality of science could be found in scientific practice. The evidence that science is rational, and the criterion that demarcates science from non-science, Popper calls falsifiability.

For Popper, the notion of falsifiability offered a more powerful and definitive view of scientific practice, than the more prevalent inductive and verificationist view advocated by Rudolf Carnap (1955) and others. According to Carnap (see Hacking, 1983:3-4), observations are the foundation of knowledge, and what makes scientific statements meaningful is that they are based on empirical observations, rather than being "mere" theoretical constructs. Scientists first make precise observations and perform experiments, then they make generalizations or draw conclusions based on their findings. What demarcates science for Carnap is the ability to verify, and thus confirm, theories through the accumulation of evidence. However, philosophers were not able to agree on a precise method for determining when scientific theories could be considered confirmed. This problem led Popper to conceive of the relationship between evidence and proof in another way. He felt that scientific theories could not be confirmed through observation, and that (metaphysical) speculation was not entirely bad, because it might lead to new and interesting conceptions (conjectures) of nature, that could then be tested and potentially shown to offer better explanations or not (refutation).

I shall certainly admit a system as empirical or scientific only if it is capable of being *tested* by experience. These considerations suggest not the *verifiability* but the *falsifiability* of a system is to be taken as a criterion of demarcation. In other words: I shall not require of a scientific system that it shall be capable of being singled out, once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be singled out, by means of empirical tests, in a negative sense: *it must be possible for an empirical scientific system to be refuted by experience.* (Popper, 1959:40-41, emphasis in original)

The ideas introduced here imply that while it may not be possible to determine with certainty whether in fact some statement *is* a scientific statement, it is possible to determine if it *is not* a scientific statement. The basis for this determination is the existence of empirical evidence that refutes whatever is claimed by that statement. In other words, some statement can be

considered scientific only if the possibility exists for some body of evidence to be brought forward that would contradict that statement. Thus, for Popper, such human endeavors as psychoanalysis and Marxism cannot be considered as sciences because the statements made by supporters of these theories are not refutable, in a conclusive manner, through empirical means (see Popper, 1963:334; Gieryn, 1995:395; Dolby, 1996:197-203).

Both of these approaches to science are based on the assumption that it is the method that scientists employ in their work that makes them different from members of other human groups. Whether this method is verificationist or falsificationist in nature is secondary to the basic acceptance of this methodological criterion for demarcation. The critical point that I want to make here is that philosophers have not been able to establish a universally accepted logical basis for selecting between these two positions (see Newton-Smith, 1981; Hacking, 1983). Further, the empirical evidence obtained through research in the history and sociology of science does not provide us with sufficient grounds for selecting one of these positions over the other. Consequently, attempts to solve the demarcation problem distract researchers from getting on with the empirical exploration of the scientific enterprise, to see what actually goes on in daily practice.

The unity of science

The issue of the unity of science came to light during the middle of the nineteenth century, as part of the ideology of German unification. Science and the rationality it represents were seen as an integral part of the social and political order of a unified Germany (see Galison, 1996). Since that time, the unity of science has been conceived as a means of protecting knowledge claims from prejudice, dogma, class interests, and various kinds of fanaticism (see Dewey, 1938). The rationality that unifies science is set in opposition to the divisive irrationality of arbitrariness and bias.

While there may be more than one way to conceive of the unity of science (see Hacking, 1996:43), the most relevant understanding of this issue for present purposes is with respect to logic and methodology. This conception is based on the notion of a fundamental "scientific

method" uniting all inquiry. Following this notion to its logical conclusion, many philosophers argue not only that all sciences should share the same method, but that all sciences are reducible to physics, which is considered to be the most fundamental of all sciences.¹ At the same time, the concept of a single science implies that knowledge is of one kind, and that it will accumulate as science is practiced.

As Hacking indicates (1983:5), in spite of their differences on the demarcation problem, both Carnap and Popper think that science is the best example of rational thought and that scientific knowledge is cumulative and evolving towards "one true theory of the universe." Both believe in the unity of science, and both subscribe to the idea that the world of sense experience not only provides the data for science, but that scientific theories must be empirically validated in order to establish their truth or falsehood. These ideas are largely consistent with a positivistic view of the world that has been central to many debates in the physical and social sciences (see Dawson, 1988; Fuchs, 1993; Fuller, 1993; Prus, 1996).

The major threat to the positivist conception of unified science is the idea of relativism. That is, relativists argue that there is no one method through which scientific inquiry should take place, and therefore there is no one science. Rather, it may be the case that the methods which scientists feel are most appropriate for studying the world emerge most directly out of the particular contexts and situations within which they find themselves (see Galison and Stump, 1996).

Perhaps one of the most radical relativists is Paul Feyerabend, who (1975:307) urges those that study science to "free society from the strangling hold of an ideologically petrified science just as our ancestors freed *us* from the strangle hold of the One True Religion!" (emphasis in the original). As Newton-Smith (1981:125) points out, Feyerabend claims that there is no such thing as a scientific method and that science should be removed from its pedestal because it is just one tradition among many (e.g., astrology, witchcraft). Even though he takes a less radical position, Thomas Kuhn is accused of instigating a "crisis of rationality"

¹ Jon Elster (1989:74) posits that the hierarchical scheme of the sciences from most fundamental to least is as follows: physics, chemistry, biology, psychology, sociology and economics.

(see Hacking, 1983:2) in science, because his work demonstrates that the historical record does not match the philosophical picture of science. The acknowledgement of method as the unifying feature of science is not required in order for studies in the history and sociology of science to proceed. As with arguments over demarcation, trying to resolve the unity issue stands in the way of carrying out research into the day-to-day practice of science.

Theory and evidence

In response to the positivistic emphasis on empiricism and verification, Pierre Duhem [1914](1954) argued that no scientific model could be evaluated directly through observation. Rather, data will only have significance within the context of a particular experimental setting and all of the models that come to bear in that situation. The implication of this concept is that negative experimental results do not necessarily disprove the theory being tested. Instead, it may be the case that some auxiliary hypothesis, having to do with the operation of a piece of equipment, for example, is the root of the problem. W.V.O. Quine [1953](1980) extended this idea to a more general form, and provided a logical notation and representational scheme that allowed philosophers to incorporate this principle into their discussions in a consistent and coherent manner. So, for example, if some theory T is consistent with a body of evidence, there may also be some theory T^* that while different from T is just as consistent with that same body of evidence. Thus, both the Copenhagen and many-worlds theories of quantum mechanics, not to mention several alternate theories (see Cramer, 1986) are capable of providing explanations for the behavior of sub-atomic particles. The critical point for the philosophy of science is that in some cases no particular body of evidence provides a sufficient basis for choosing between theory T or T^* , in a conclusive or definitive manner.

The so-called Duhem-Quine hypothesis (see Hooker, 1987:331ff.) implies that scientific theories are largely “underdetermined” by the facts that emerge from observations made in scientific practice. In other words, the theoretical and methodological contexts within which research is performed are necessary for drawing conclusions from such research, but they are not sufficient to provide an absolute determination of the truth of the findings. What we learn

from this is that the study of actual scientific practice can make a valuable contribution towards our understanding of the scientific enterprise. At the same time, it demonstrates that empirical evidence cannot be used in a direct manner to confirm or deny philosophical conceptions of science. Rather, the empirical investigation of science may bring to light alternate insights into the workings of science that philosophical and historical explorations have not been able to provide.

Scientific theory change

A related area of concern within the philosophy of science is to provide an explanation for the growth of science and the changes that accompany that growth. The objectives here are to determine the nature of scientific theories, how scientists choose one theory over another, and, by extension, how old theories come to be replaced by new ones. As with the three problems examined so far, notions of scientific change are generally based on the perception that scientific inquiry illustrates the ultimate application of rational processes.

Perhaps the best known contribution to resolving these issues is the debate between Karl Popper and Thomas Kuhn (for details and extended commentary, see Lakatos and Musgrave, 1970). As has already been stated, Popper not only champions scientific rationality, but he conceives of the work of science as centering around the activity of falsifying theories. In other words, scientific practice consists of challenging and systematically testing the ideas put forward by scientists by trying to falsify them. For Kuhn (more details of Kuhn's conception of science are provided later in this chapter), this kind of revolutionary or destabilizing activity is the exception rather than the rule in scientific practice. According to Kuhn, the normal day-to-day activities of scientists are carried out within the bounds of accepted and stable theories and practices. It is only when these stable theories and practices cannot account for an increasing number of anomalies or deviations that scientists shift into a more revolutionary mode to seek out alternate explanations. The rest of the time, they are content to expand upon and further verify seemingly known facts and true theories.

David Bloor (1991:61-62) identifies four major differences between Popper's and Kuhn's conceptions of science. First, Popper's account is far more prescriptive than Kuhn's, which is more descriptive in nature. In other words, Popper is more concerned to argue how science should be carried out, while Kuhn offers an historical account of how science has been practiced. Second, Popper stresses debate, disagreement, and criticism, while Kuhn emphasizes areas of agreement that scientists take for granted in their work. That is, in looking at the social aspect of science, Popper tends to focus on conflict, while Kuhn emphasizes consensus. Third, Popper draws attention to those aspects of science that are universal and abstract, while Kuhn highlights those things that are local and concrete. In other words, Popper is concerned with methodological canons and the intellectual environment of science, while Kuhn draws attention to the more concrete aspects of scientific work. Fourth, Popper's account of science is linear and homogeneous, whereas Kuhn's version is more cyclical and heterogeneous. For Popper, the same methods and processes apply at all stages of scientific investigation, whereas, for Kuhn, scientists employ qualitatively different procedures at various stages of their work. These last two points in particular indicate the difference between a holistic conception of one "science" (Popper), as contrasted to a conception of many "sciences" (Kuhn). The debate on scientific change, however, goes well beyond Popper and Kuhn.²

The whole debate over scientific theory change is important for the present project because we would like to know how scientists both instigate and respond to change on a daily basis. No amount of grand theorizing on general explanations of change, that are consistent with some philosophical conception of science, will help us to understand the ways in which practicing scientists deal with the ambiguities and uncertainties that continuously emerge as they attempt to accomplish their goals.

² Larry Laudan et al. (1986) present a comprehensive analysis of philosophical models and historical research on theories of scientific change. The authors are critical of most of these theories for failing to address the more short-term, localized processes that take place in scientific practice.

The pragmatic turn

So far in this chapter we have briefly discussed four problems in the philosophy of science: the demarcation problem, the question of the unity of science, the relationship of theory and empirical evidence, and scientific theory change. These four issues are important for the present project because alternate solutions to the problems identified here emerge out of later developments in the sociology of science. As I have already stated, the fact that many sociologists have been working in the service of philosophy means that they have been unable to devote sufficient attention to the exploration of the day-to-day practice of science. At the same time, an awareness of these philosophical issues helps us to understand more clearly the significance of the developments in the history of science discussed later in this chapter.

Within philosophy, these same issues are being examined at a fundamental level by scholars advocating a "pragmatic turn" from traditional views of science. Miriam Solomon (1995:208), for example, indicates that philosophers, and others that study science, tend to assume that truth is the most important goal of scientific inquiry, that scientific disputes are settled by reference primarily to cognitive or epistemic factors, that all scientists make decisions for the same reasons, and, that scientific theories are linguistic entities assessed in terms of logical relations. Alternately, pragmatic approaches to science (e.g., Hacking, 1983) attempt to incorporate notions of practical accomplishment that, for example, may not be related to the exercise of logical or linguistic skills.

Human beings over the course of their lives certainly pursue goals other than truth. They collect stamps, listen to music, become marathon runners, nurture their children, fight wars, and vote in elections. (Solomon, 1995:212)

By analogy, there is no reason to believe that all activity within science is aimed at the attainment of truth. As Solomon (1995:210) indicates, the specific goals of individual scientists, or groups of scientists, may not be the same as those of the scientific enterprise as a whole.

While we must be careful not to get bogged down in internal disputes within philosophy, developing an appreciation of research being carried out in the philosophy of science is important for the present project. One of the basic reasons for this is that, to date, much of the

vocabulary used in the presentation, analysis, and evaluation of sociology of science research comes directly from philosophy. Similarly, as the material covered in this chapter and the next will demonstrate, the research agenda for the sociology of science has been set to a large degree by the concerns of philosophers. In the present work, I intend to draw attention to the necessity of grounding theory in the world of human experience, in a way that many philosophers (and many sociologists) have yet to appreciate. First, however, let us proceed with our coverage of background material by examining some of the earliest efforts in the sociology of science.

Sociology of Science

Prior to examining the history of science, this section is devoted to the early development of the sociology of science, with a particular emphasis on Robert K. Merton. The major reason for selecting this order of coverage is that many of the ideas expressed here predate the important work of Thomas Kuhn in the history of science. Merton's work reflects two notable orientations with respect to science and sociology. First, Merton's research on science was influenced by the ideas Max Weber, particularly as they were being interpreted by his supervisor Talcott Parsons. Second, Merton's attitude towards science is consistent with Karl Mannheim's (1936, 1952) sociology of knowledge, which exempted natural science from the purview of sociology. The main issue covered in this section is Merton's notion of the normative structure of science, but prior to that a brief mention of Merton's work on the historical development of modern science needs to be made. The reason for this is that much of Merton's later work, and that of his students, develops out of his findings in this regard.

Perhaps the first work to be widely recognized as "sociology of science" was Merton's 1938 doctoral dissertation *Science, Technology, and Society in Seventeenth-Century England* (1970). In his work, Merton hypothesizes a link between the Puritan ethic and the rise of modern science in England. Drawing on Max Weber's [1904](1930) analysis of the social impact of Calvin's theology on the rise of capitalism, Merton links the normative elements of diligence in serving God, the renunciation of material satisfaction, and the constructive use of time, associated with Calvinism, with the rise of science. These Puritan norms functioned both to

support the agenda and create a receptive atmosphere for the early growth of the scientific enterprise in England.

Merton's main concern in this work was with how science became institutionalized in the West. A disproportionate number of the founding and central members of the Royal Society (i.e., the first scientific institution), Merton noted, were Puritans. Seeking to explain this state of affairs, Merton (1973:228) was led to conclude:

What we call the Protestant ethic was at once a direct expression of dominant values and an independent source of new motivation. It not only led men into particular paths of activity; it exerted a constant pressure for unswerving devotion to this activity. Its ascetic imperatives established a broad base for scientific inquiry, dignifying, exalting, consecrating such inquiry.

While determining the precise implications of this statement has fuelled an ongoing debate (see Becker, 1984; Merton, 1984; Shapin, 1988), what is most important for the present project is Merton's contention that "science, like all other social institutions, must be supported by values of the group if it is to develop" (1984:1097). In other words, Merton's early and influential approach to the sociology of science focused the sub-discipline on the study of the values that shape the institutional context of science, rather than on the nature of the actual practice of scientists, in and out of the laboratory.

Merton treated the sociology of science as a sub-aspect of his broader functionalist approach to the study of society. As Giere (1988:30) indicates, one of the basic tenets of functionalism is the idea that the existence and maintenance of norms within social institutions can be explained by the function that those norms play in perpetuating the goals (values) of that institution and more broadly of the society of which they are a part. In discussing the role and function of science in modern society, Merton [1942] (1973:270) argues that the "institutional goal of science is the extension of certified knowledge." As Thomas Gieryn (1995:398) points out, it is the "institutionalized ethos of science" that, for Merton, demarcates science from non-science.

The ethos of science is that effectively toned complex of values and norms which is held to be binding on the man of science. The norms are expressed in the form of prescriptions, proscriptions, preferences, and permissions. They are legitimized in terms of institutional values. These imperatives, transmitted by precept and example and reinforced by sanctions are in varying degrees

internalized by the scientist, thus fashioning his scientific conscience. (Merton, 1973:268-269)

Scientists learn these norms as part of their socialization into the discipline, and they are reinforced throughout the scientific community by a series of sanctions against transgressors, and through rewards for conformists who contribute to the stock of knowledge. Consistent with a functionalist perspective, scientists, like members of other social institutions, are seen as rule followers.

The normative structure of science consists of both technical and social norms, which reflect the two distinct parts of the scientific enterprise; the scientific and the social. As Giere (1988:31) points out, for Merton, the behavior of scientists is constrained by both "rules of rationality," and "rules of social action." The rules of rationality deal with issues of logic and evidence.

The technical norm of empirical evidence, adequate and reliable, is a prerequisite for sustained true prediction; the technical norm of logical consistency, a prerequisite for systematic and valid prediction. (Merton, 1973:270)

These technical norms, which are exempt from sociological examination, are consistent with the positivist insistence on empirical evidence and verifiability, and with the notion of a unified or singular scientific method. Coupled with these are four social norms that reflect Merton's emphasis on the ways in which institutional structures determine the behavior of individuals. These norms are: universalism, communism, disinterestedness, and organized skepticism.

Universalism forms the basis for the meritocratic system in science, whereby evaluation of research results is carried out in an objective manner by a scientist's peers.

The acceptance or rejection of claims entering the lists of science is not to depend on the personal or social attributes of their protagonist; his race, nationality, religion, class, and personal qualities are as such irrelevant. (Merton, 1973:270)

This norm is strictly opposed to any form of particularism that would undermine the scientific enterprise by giving consideration to the personal attributes of a scientist rather than concentrating on the nature of the contribution made by that scientist.

The norm of communism is consistent with a view of the cumulative nature of science, and an understanding that the advancement of knowledge relies on the collective efforts of many

scientists. Scientists are encouraged to share their work openly rather than engage in any kind of secrecy or hoarding.

The substantive findings of science are the product of social collaboration and are assigned to the community. They constitute a common heritage in which the equity of the individual producer is severely limited. (1973:273)

Scientists' "property rights" are limited to the prestige and esteem that arise from having a discovery or theory named after them (e.g., Boyle's law, Creutzfeldt-Jakob disease, Heisenberg's uncertainty principle).

With the norm of disinterestedness, Merton emphasizes an institutional basis for the behavior of scientists, rather than looking for any inherent individual virtues, such as a "passion for knowledge" or an "altruistic concern with the benefit to humanity."

The quest for distinctive motives appears to have been misdirected. It is rather a distinctive pattern of institutional control of a wide range of motives which characterizes the behavior of scientists. (1973:276)

Scientists conform to this norm because it is in their own best interest to do so. Fraud and chicanery are avoided, as are attempts at self-aggrandizement, because scientists are ultimately judged by their peers.

The final norm, organized skepticism, involves the "temporary suspension of judgment" and the "detached scrutiny of beliefs" until such time as sufficient evidence and argument have been presented to support a claim.

The scientific investigator does not preserve the cleavage between the sacred and the profane, between that which requires uncritical respect, and that which can be objectively analyzed. (1973:278)

In this way, certified knowledge is arrived at, and mere dogma and ideology can be avoided.

Merton's work on the normative structure of science was widely accepted and was incorporated into the research agenda of the sociology of science (see Cole and Cole, 1973; Zuckerman, 1988, Cole, 1992), but it also met with substantial criticism (see Barnes and Dolby, 1970; Mulkay, 1976; Toren, 1983; Leydesdorff, 1992).³ Much of this criticism was based on the lack of evidence in support of a normative view of science. Michael Mulkay (1976:641) argues,

³ Zuckerman (1988:516) indicates that the level of controversy around the norms of science "is a self-exemplifying instance of the norm of organized skepticism in practice."

for example, that there is little empirical support for the notion that the norms of science have become institutionalized (i.e., form the basis for the reward system in science). Similarly, he (1976:645-646) suggests that these norms do not constitute a true and distinctive basis for the practice of science. Rather, they are part of a "vocabulary of justification and evaluation" that scientists employ to portray their actions to lay audiences, provide misleading accounts of what actually takes place in science, and further their own social interests. For Mulkay, then, the notion of a normative structure of science is more accurately seen as an "occupational ideology."

In another instance, Michael Lynch (1993:63-64) indicates that there are three main points on which criticisms of Merton's views are based. First, the norms were stated in an abstract manner, and it was unclear how these related to the actual conduct and behavior of scientists. Second, the norms assumed a coherent view of scientific methodology based on positivist ideals that would lead to a progressive accumulation of certified knowledge. Both historical and philosophical explorations of science provided little evidence to support such a view. Third, Merton viewed the sociology of science as being "self-exemplifying." In other words, the institutional arrangement that typified natural science could also be seen in the sociology of science, thus making it a stable scientific field. The problem with this is that within the sociology of science, and sociology more generally, there is no internal consensus on fundamental matters of theory, fact, and practice. What binds these three statements together is the idea that the sociological examination of science is constrained by the philosophical conception of science as a unified and self-regulatory sphere of activity, whether the basis of this unity and self-regulation is conceived in terms of either methods or social norms. What the historical and sociological analysis of the actual practice of science reveals, however, is that scientific activity is far messier, and far more contingent on local conditions, than any rigid model can account for. Thus, this philosophical framework inhibits the open exploration of science as it is actually practiced, for which sociology is ideally suited.

History of Science

If work in the philosophy of science is dedicated to the “context of justification,” then research in the history of science centers on the “context of discovery” (see Hacking, 1983). In other words, historians set out to uncover the, who, what, when, and where of events that have taken place within the scientific enterprise. As Giere (1988:18-19) indicates, historians of science present “descriptions of how science has been pursued,” through an examination of, “the ideas and choices of individual scientists.” They also look for evidence of error, myth, or other circumstances that may have impeded scientific progress. In this way, historians provide a detailed record of what happened in science such that it looks like it does today. Unlike the scientists they study, however, historians typically do not seek to generalize. As a result, this work is often carried out without the historians questioning the internal workings or the practical accomplishment of the scientific enterprise. This sort of presentation of the history of science, which dominates science textbooks, puzzled Thomas Kuhn, when he moved from being a physicist to being an historian of science.

Thomas Kuhn

Thomas Kuhn's *The Structure of Scientific Revolutions* (1970, first published in 1962) represents the *transitional work* in science studies in that it brought about a revolution in the way that historians, philosophers, and sociologists of science defined and approached their subject matter. Specifically, Kuhn's work points out that many of the problems encountered in trying to understand and explain what goes on in science are not solvable through strictly philosophical means, nor through the kind of empirical evidence presented by historians. As will become clear, Kuhn established the foundation for a truly social study of science, one which has its empirical base in communities of practicing scientists, in the here and now.

As a graduate student in theoretical physics, Kuhn was involved in presenting a course on the history of science for non-scientists. During this time, Kuhn began to realize, he says, that “exposure to out-of-date scientific theory and practice radically undermined some of my basic conceptions about the nature of science and the reasons for its special status” (1970:v). After

leaving physics to examine the history of science in more detail, Kuhn puzzled over “the number and extent of the overt disagreements between social scientists about the nature of legitimate scientific problems and methods” (1970:viii). After reflecting on these experiences, Kuhn came to realize that the historical study of science has the potential to bring to light more than just the details of the context of discovery. The historical record also provides information about the internal workings of science and the basis for differentiating science from other areas of human activity. This notion forms the opening statement of his work.

History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed. (1970:1)

Envisioning science in both more pluralistic and generic terms, Kuhn became interested in determining just how science “progresses.” At the same time, Kuhn’s approach to science placed a strong emphasis on notions of community and the collective action of scientists. These ideas, however, were not Kuhn’s alone. Rather, they reflected the influence of an earlier scholar, Ludwik Fleck [1935](1979), a much neglected figure in the history, philosophy, and sociology of science. Kuhn (1970:vi-vii) indicates that reading Fleck made him realize that many of his ideas should be set in the context of a “sociology of the scientific community.”⁴ Before turning to a brief examination of Fleck’s work, though, I want to introduce Kuhn’s notion of a “paradigm,” which provides a kind of central conceptual framework for Kuhn’s analysis of science and for the discussion in this section.

For Kuhn (1970:12), scientific progress or development is marked by “the successive transition from one paradigm to another via revolution.” As this indicates, the concept of the “paradigm” is integral to Kuhn’s work, and yet he himself has been criticized for failing to adequately define this term.⁵ In the postscript to the second edition of his work, Kuhn (1970:175) indicates that the paradigm concept has two major meanings, namely, paradigm-as-shared-

⁴ A number of authors (Golinski, 1990; Lynch, 1993; Restivo, 1995) have recently commented that Fleck’s work presages many of the developments in the sociology of science that took place in the 1970s and 1980s.

⁵ Margaret Masterman (1970) identifies at least twenty-two different uses of the term paradigm in Kuhn’s work.

commitments and paradigm-as-shared-examples.

On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science. (1970:175)

The notion of paradigm-as-shared-commitments (1970:181-187) refers to the "symbolic generalizations" that are used without question or dissent within a certain "disciplinary matrix," as well as the more "metaphysical" aspects of the scientific enterprise such as beliefs and values, which may affect judgment. The notion of paradigm-as-shared-examples (1970:187-191) is a more technical concept based on the existence of models that allow scientists to carry out their work. In other words, normal science is carried out through the application of "acquired similarity relations" whereby prevailing conceptualizations, technologies, and practices are applied to new puzzles.

These discussions should make it clear that, for Kuhn, the proper unit of analysis for the study of science is not science as a whole, but rather sets of scientific communities, each constituting and based on a somewhat different paradigm. "A paradigm governs, in the first instance, not a subject matter but rather a group of practitioners" (1970:180). What this statement implies is that no single paradigm can be said to exist for science in any overall sense, or even for a branch of science, like physics. Even within a small specialty like high-energy physics, each separate community of researchers has its own paradigms (see Traweek, 1988). Edge and Mulkay (1976), for example, were able to identify four separate research traditions in the astronomy department at Cambridge University alone.

Turning now to Ludwik Fleck, he set out to describe the genesis and development of the scientific fact that "the so-called Wasserman reaction is related to syphilis" (1979:xxviii). The crucial element to recognize is that the "fact" referred to in Fleck's analysis is the relationship between a repeatable laboratory procedure and a socially recognizable disease. In other words, Fleck argues that the conception of syphilis emerged, in both the scientific community and among the wider public, once the test for detecting the disease became more accurate and reliable. Thus, scientists' fact-making activities reflect not only their observations, but the ways

in which these observations are interpreted by various groups under the influence of technological developments and other extra-scientific concerns.

For Fleck, the unit of analysis for science is the "thought style," a concept similar to Kuhn's notion of a paradigm. Truth or correctness in science is judged against the prevailing thought styles, and is thereby conditional on acceptance of a judgment by the "thought collective" (i.e., those individuals who share particular thought styles). In other words, Fleck (1979:100) favors an explanation for scientific practice based on the idea that there is a culturally informed or intersubjective basis for establishing facts within science. This process of knowledge production is composed of "active" and "passive" elements. Those things over which the actor is seen to have some level of control are considered to be the active elements of knowledge production, while those over which the actor has no apparent control are considered to be passive elements.

More specifically, active elements can be defined as the historical, sociological, and psychological factors that make up the thought style and that are manifested in the practices of the thought collectives or communities of scientists. The passive elements of knowledge production are associated with conceptions of fixed reality, such as the measurable physiological responses of the human body to the introduction of various chemical compounds. For Fleck, even these passive elements are subject to change over time, and what may be considered fixed reality, for one community of scientists, does not necessarily remain so for another group of scientists, when thought styles change (1979:51). For example, the discovery of the red shift in the spectra of the stars, which eventually led to the development of big bang theory, altered the way that astronomers thought about the structure, size, and evolution of the universe. So, for Fleck, scientific progress tends to take place over two different time scales. Active elements of science may change on a day-to-day basis in response to certain circumstances, while changes in the passive elements tend to reflect longer-term assessments of various theoretical and methodological constructs, or alternate sets of assumptions (e.g., number of degrees in a triangle). In either case, change is always rooted in the goals and activities of particular groups of scientists.

Kuhn's account of the history and progress of science is much like the one presented by Fleck. He proposes the following scheme to demonstrate the path that science takes:

normal science → crisis → revolution → new normal science

The term normal science is used to describe a period of scientific activity carried out within the framework of an accepted paradigm. That is, the work of scientists in these periods is "firmly based upon one or more past scientific achievements" (1970:10), and is associated with three kinds of problems: the determination of significant facts, the matching of facts with theory, and the articulation of theory (1970:34). In other words, normal science may involve such activities as: measuring the specific gravities and compressibilities of materials, constructing a giant scintillation counter to demonstrate the existence of the neutrino, and determining the numerical value of the universal gravitation constant.

Kuhn refers to these kinds of activities as comprising the regular "puzzle-solving" work of scientists. He argues that most scientists are engaged in the systematic and often creative working out of the details of a particular worldview to which they are exposed. Their efforts are directed at providing ever more finely grained detail, and applying their methods to a larger and more varied selection of cases. For Popper, these activities constitute mere "hack work" (see Bloor, 1991:60), because they are not directed at falsification. Kuhn argues that individual skill is directed at efficiency and comprehensiveness, rather than at looking for alternative conceptualizations. Science within an established paradigm can be viewed as a kind of normative or preferential state, in which the day-to-day work of most scientists is carried out. Anomalies arise when the existing framework is not capable of providing a model for the solution of some particular problem. For example, Roentgen's discovery of X-rays was not only a surprise, it "violated deeply entrenched expectations" (1970:59). In this case, observations of a phenomenon could neither be predicted nor explained by the framework of beliefs within which the scientist was working.

A crisis normally occurs when the number, diversity, or centrality of these inexplicable anomalies raises doubts within the community of scientists about the efficacy of a paradigm. In their attempts to deal with anomalies, the result may be a transition to revolutionary science, as

scientists search for and apply new theories and methods. For example, the failure of Newtonian mechanics to describe either very large-scale phenomena on the one hand, or very small-scale phenomena on the other, led to the development of relativistic mechanics and quantum mechanics, respectively.

The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgment leading to that decision involves the comparison of both paradigms with nature *and* with each other. (1970:77, emphasis in original)

What emerges from a period of revolutionary science is a new normal science that may share little with previous ways of doing science.

One of the conclusions that Kuhn draws from his work is that science is non-cumulative. While acknowledging that normal science is somewhat cumulative in nature, Kuhn (1970:84) argues that science, as a whole, is not. The notion of cumulation is “the ideal that historical development would display if only it had not so often been distorted by human idiosyncrasy” (1970:96). In other words, in an evolutionary theory of science, “new knowledge would replace ignorance rather than replace knowledge of another or incompatible sort” (1970:95). But, according to Kuhn's theory of science, which rejects any necessary teleological perspective, scientific development is seen to evolve “from” rather than “to” a certain state (1970:172). So, while some accumulation of knowledge may take place, there is every possibility that new knowledge will replace old knowledge, thus leading to the establishment of a drastically different worldview.

This revolutionary theory of scientific change allows Kuhn to offer an alternate criterion for the demarcation of science from non-science, which is based on the paradigmatic consensus that characterizes normal science. Kuhn argues that: “As in political revolutions, so in paradigm choice – there is no standard higher than the assent of the relevant community” (1970:94). This view is opposed to Popper's, for example, in that Kuhn does not demarcate science by some foundational or essentialist criterion, but rather by a socio-cultural one, which is constructed by the scientists themselves.

A very important point, for the present project, is the claim that “after a revolution scientists are responding to a different world” (Kuhn, 1970:111). This phenomenon leads to the

notion of "incommensurability." Following a revolution in science, meanings and standards have changed (see Giere, 1988:36-37), such that, what may have previously counted as a valid scientific problem, or solution, may no longer count as such. Kuhn compares the transition that scientists undergo to a gestalt shift or perhaps more accurately a religious conversion. In terms of the gestalt shift analogy, which Kuhn borrows from Wittgenstein, a pattern of dots may be perceived of as depicting a duck or a rabbit, and once observers are aware of this they can switch back and forth between these two quite readily. The transition in science, however, is even more like a religious conversion where once the shift has been made, there is no going back. The worldviews are now incommensurable. So, for example, following the shift from Ptolemaic to Copernican astronomy, Kuhn (1970:115) indicates that a convert would say, "I once took the moon to be a planet, but I was mistaken."

There are three practical consequences of these changes of worldview, the first of which is disagreement over "the list of problems that any candidate for paradigm must resolve" (1970:148). For example, as a result of the shift from phlogistic chemistry to Lavoisier's chemical theory, chemists were inhibited from investigating an explanation for similarities in the properties of metals. This is a problem that had previously been addressed under phlogistic chemistry, and to which a satisfactory answer had been found. However, within the context of the new chemical theory, the question made no sense. Second, there is disagreement over the standards or definitions of science. Kuhn (1970:149) states that: "Within the new paradigm, old terms, concepts, and experiments fall into new relationships one with the other." For example, according to geocentric astronomy, the concept "earth" implied not only a planet, but also the notion of fixed position. With the advent of heliocentric astronomy, the notion of fixed position had to be discarded and this required scientists to adopt a new perspective on both the concepts of earth and motion. Third, there is the idea that "the proponents of competing paradigms practice their trades in different worlds" (1970:150). This can lead to a communication breakdown, in which the participants must "recognize each other as members of different language communities and then become translators" (1970:202). This final notion, in particular,

has given rise to the criticism that Kuhn is a relativist and that his work gives rise to a "crisis of rationality" (Kuhn, 1970:205; Hacking, 1983:2).

Not only does Kuhn question established notions of justification, he is also critical of traditional approaches to the study of the context of discovery. Reflecting on the ideological rewriting of history, he argues (1970:136) that:

Both scientists and laymen take much of their image of creative scientific activity from an authoritative source that systematically disguises – partly for important functional reasons – the existence and significance of scientific revolutions.

In other words, the revolutionary development of science is suppressed in textbooks and in histories, by the members of the scientific profession, who fear that a more accurate and detailed portrayal of the scientific enterprise would undermine their authority by giving "artificial status to human idiosyncrasy, error, and confusion" (1970:138). Pedagogical interests have blurred the important historical lesson that "concepts like that of an element can scarcely be invented independent of context" (1970:142).

Kuhn's research in the history of science provides a number of important observations for the present project. First, his contention that science is not a uniformly focussed or cumulative enterprise paves the way for a more open-ended exploration of the scientific enterprise through which the diversity and complexity of observed phenomena can be accepted at face value. Second, his observation that the distinction between the "context of discovery" and the "context of justification" is problematic means that sociologists can explore all aspects of science on an equal footing with philosophers and historians. Third, and perhaps most importantly, Kuhn contends that the direct observation of the scientific enterprise would seem to indicate that the logical methods used in the production of scientific knowledge (see Hacking 1983:6; Gieryn, 1995:401) are best envisioned in terms of emergent human interchange. Underlying all of these ideas is the conception of science as being comprised of communities of practitioners that are constantly engaged in a process of defining and redefining their worlds. All of these observations are consistent the symbolic interactionist approach to the study of science advocated here.

The Strong Programme

In the late 1960s, members of the Science Studies Unit at the University of Edinburgh set out to take an integrated and multi-disciplinary approach to the study of science, one that would demonstrate that “the cognitive is social, and vice-versa” (Leydesdorff, 1992:241). That is, they agreed that no analytical distinction was to be made between the cognitive and the social dimensions of science, or between internal and external dimensions. To do so, this group of scholars built on Kuhn's conceptual framework (see Barnes, 1982), and at the same time, incorporated insights from a diverse array of sources: concepts of knowledge as found in Emile Durkheim (1915) and Karl Mannheim (1952), the critical theory of Jurgen Habermas (1972), the philosophy of Ludwig Wittgenstein (1953; see Winch, 1958), the anthropology of Mary Douglas (1970), and the ethnomethodology of Harold Garfinkel (1967).

In his analysis of this initiative, Steve Woolgar (1988b:40) indicates that, historically, the functionalist sociology of science was largely confined to the study of social factors as they impinged (improperly) on the practice of science and the irrational elements of the scientific enterprise, while the philosophers of science examined the rational bases for truth statements in science. The so-called “strong programme” broke with both of these approaches and set in motion a series of controversies centered as much on disciplinary boundary disputes, as on the substantive elements of their “new paradigm” in the social study of science (see e.g., Hollis and Lukes, 1982; Manicas and Rosenberg, 1985, 1988; Brown, 1989). As Martin Hollis (1982:68) points out, the *strength* of the strong programme is in its rejection of the more traditional (scientific) view that: “A man who knows believes because he knows; a man whose beliefs can be explained by custom or imagination does not know.”

David Bloor, in his *Knowledge and Social Imagery* (1976, 1991), presents the following framework, which he says captures the essence of the strong programme's view of the proper way of doing sociology of science.

The sociology of scientific knowledge should adhere to the following four tenets:

1. It would be causal, that is, concerned with the conditions which bring about belief or states of knowledge. Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief.
2. It would be impartial with respect to truth and falsity, rationality and

irrationality, success or failure. Both sides of these dichotomies will require explanation.

3. It would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs.

4. It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. Like the requirement of symmetry this is a response to the need for general explanations. It is an obvious requirement of principle because otherwise sociology would be a standing refutation of its own theories.

These four tenets, of causality, impartiality, symmetry and reflexivity define what will be called the strong programme in the sociology of knowledge. (1991:7)

The first of these tenets implies that there may be political, economic, religious, psychological, historical, or social considerations that lead scientists, and members of society as a whole, to accept something as knowledge. This tenet is based on the notion that it should be possible to identify the specific "social interests" of one group or another (see e.g., MacKenzie, 1978, 1981; Shapin and Schaffer, 1985), that "cause" members to align themselves with some particular version of a scientific fact.

Social interests are expressed as claims on resources by the members of various groups as they compete or cooperate to achieve their goals. Reflecting on his own study of phrenology in late eighteenth century Edinburgh, for example, Steve Shapin (1982:4) argues:

Reality seems capable of sustaining more than one account given of it, depending upon the goals of those who engage with it; and in this instance at least those goals included considerations in the wider society such as the redistribution of rights and resources among social classes.

Thus, the social interests of some group or other may "cause" knowledge to develop in one way or another. In a similar study, Donald MacKenzie (1978, 1981) argues that the development of statistics in Britain between 1865 and 1930 was influenced by eugenics. Social control through selective breeding was of interest to the rising professional class in Britain, and the kinds of problems undertaken by statisticians like Galton, Pearson, and Fisher reflected their interests in both the ideology and political implications of eugenics.

The second tenet implies that sociologists are to be impartial with respect to their assessment of knowledge claims. In other words, all beliefs are to be explained causally, with no exemption for those beliefs that some people may hold to be rational and true. As Woolgar (1988b:42) indicates, this tenet goes contrary to the "received view" that sociologists investigate

only social factors that produce erroneous knowledge, while philosophers are responsible for investigating rationality, the basis of true knowledge.

The third tenet is closely related to the second in that it stipulates the need to offer the same kind of causal mechanisms to explain what comes to be classified as good and bad science, or right and wrong science. For example, the explanation offered for the belief in geocentric astronomy must be of the same kind as that offered for heliocentric astronomy. The sociologist cannot say, for example, that one of these theories can be explained rationally, while the other can be explained psychologically or politically.

The fourth tenet implies that sociology is no different from science, in that any sociology of scientific knowledge must apply to sociology itself. In other words, it is part of its own subject matter. To say, for example, that science is socially constructed is to also say that the sociology of science is socially constructed. The "knowledge" that emerges from either process cannot be privileged over the other (see Mulkay, 1985; Manicas and Rosenberg, 1985, 1988; Woolgar, 1988b). All four tenets are opposed to those theories of science that argue for some kind of scientific exceptionalism, and the ideas expressed in these tenets have given rise to heated debates among philosophers, historians, and sociologists of science.

On one level, criticism of the strong programme is based on the fact that the tenets are neither self-evident nor self-explanatory (Manicas and Rosenberg, 1985:77). That is, not only has a systematic explanation of these tenets beyond the brief statement cited above not been forthcoming, but, as with the Mertonian norms of science, Steve Woolgar (1988b:50) suggests that we might be being presented with yet another set of "anti-guidelines" for conduct. In other words, the strong programme is more prescriptive than descriptive.

A more specific criticism is leveled by Martin Hollis, who (1982:80-81) argues that:

If there is to be the same impartial and symmetrical style of explanation for all beliefs, it should, presumably, apply to the beliefs of those who advocate a strong programme. Yet these beliefs lay claim to a scientific status, which the programme dare not forfeit and dare not assert, since both would subvert the programme.

In other words, in claiming scientific status for itself, the strong programme is exempt from its own tenets, and therefore it destroys its credibility by serving as its own counterexample. If, on

the other hand, the strong programme does not claim scientific status, then there is no reason to believe that the tenets of this programme are any more valid than those of any other view of science.

An equally damaging criticism is that the proponents of the strong programme have not been able to demonstrate a distinct causal connection between specific social interests and the development of science. Knorr-Cetina (1982:322) observes that research carried out under the strong programme fails to “demonstrate exactly *how* and in virtue of which *mechanisms* social factors have indeed entered (and are hence reflected in) particular knowledge claims” (emphasis in the original). In addition, Harriet Zuckerman (1988:553) argues that the proponents of the strong programme “have yet to address the basic questions of which interests will be activated in identified types of situations and how it is that scientists of evidently different social “interests” often maintain the same theoretical position.” So, according to critics at least, the strong programme is not only without logical foundation, it also does not appear to provide clear direction for the empirical study of science.

While the strong programme can be viewed as a systematic effort to apply the insights into the scientific enterprise provided by Thomas Kuhn, another group has had much greater success in advancing our knowledge of science. These scholars (see, e.g., Latour and Woolgar, 1979; Knorr-Cetina, 1981), who entered the laboratories to observe scientific practice directly in the here and now, are the subject of the next chapter. To conclude this chapter, the following summary indicates how the philosophy, history, and sociology of science have tried to understand science, and how there is an opportunity and justification for an alternate approach to science that can provide insights not readily accessible through the application of these more traditional approaches.

Summary

Researchers in the philosophy, history, and sociology of science have tried to understand the nature of the scientific enterprise. Work in each of these disciplines has uncovered different aspects of science, through the application of methods peculiar to that

discipline. For example, philosophers, who generally frame their concerns in terms of ontology and epistemology, have tried to apply logical analysis to establishing a comprehensive picture of the scientific method, which reflects their conviction that scientists investigate a real world in a rational way. Historians, on the other hand, have accumulated the details of the record of scientific activity in an effort to demonstrate how scientific investigations were carried out. Unfortunately, most of this work has been carried out in accordance with the philosophical perception of science. Early efforts by sociologists, who were in the best position to explore scientific activity in the here and now, largely missed their chance also by adopting much of the philosophical worldview. In the case of Merton and his followers, the central place of the scientific method was taken for granted, and added to this was a conviction that scientific activity was constrained by a set of social norms that formed the ethos of science.

Thomas Kuhn was able to demonstrate that the historical record did not in fact match the philosophers' model of a rational unified science. Instead, science appeared to be a far messier, ambiguous, and contingent enterprise. Kuhn's contention that scientific knowledge emerged through negotiations within communities of practitioners threatened the foundation of philosophical, historical, and sociological examinations of science. A major limitation of his work, however, is that he stopped short of actually following through with testing his findings in contemporary settings. One response to this "crisis" was provided by the advocates of the strong programme, who responded to Kuhn's insights by crafting an alternative, sociologically-informed, philosophical framework for science. However, like Kuhn, they failed to carry out empirical investigations of science as practiced.

So, while these different approaches have brought to light various elements of the scientific enterprise, we are still left with an incomplete and in many ways distorted picture of science. Philosophical approaches to science have become bogged down in trying to resolve fundamental philosophical arguments, to the exclusion of increasing our knowledge of science. Historical studies of science are hampered by their efforts to work within and contribute to philosophical models of science, and by their inability to study contemporary scientific practice. The same criticism can be leveled at much of the research in the sociology of science. What is

required is a sociological approach to science that moves beyond the limitations of philosophical and historical studies, and offers a framework within which to carry out the empirical investigation of science in the here and now. I contend that such an approach can be found in symbolic interactionism.

Chapter Three

STUDYING SCIENCE BY STUDYING SCIENTISTS

In the late 1970s, as part of a move away from traditional studies in the history, philosophy, and sociology of science, a new initiative arose that clearly demonstrated the influence of the work of Thomas Kuhn. It was, for the most part, consistent with the ideas advocated by the proponents of the strong programme in the sociology of scientific knowledge, namely, "laboratory studies." The niche carved out by these studies is the examination of "knowledge that is yet in the process of being constituted" (Knorr-Cetina, 1995:141). In other words, these studies set out to investigate the ongoing social processes that constitute the internal workings of science. This research uses ethnographic methods (e.g., open-ended interviews and participant observation) and emphasizes the cultural activity of scientists as opposed to the logical-rational methodology or organizational structure of the scientific enterprise. Regrettably, only a small number of these studies of an extended nature have been performed. Knorr-Cetina (1995:147) identifies only four: Latour and Woolgar (1979), Knorr-Cetina (1981), Lynch (1985), and Traweek (1988).

In order to assess these laboratory studies, especially with respect to the present project, I begin with a brief coverage of the fieldwork carried out by Harry Collins in support of his empirical relativist program. Collins did not engage in a full-blown ethnographic study of science (hence his exclusion from the list just given), but he did use evidence gathered in the field to support his ideas about how science is practiced. I devote particular attention to *Laboratory Life*, by Bruno Latour and Steve Woolgar (1979, 1986). This work is distinguished by being the first book-length ethnographic study of science, and it most clearly demonstrates how various methods of studying science (both qualitative and quantitative) can be applied in the field. Next, I discuss the work of Karin Knorr-Cetina, which strongly parallels Latour and Woolgar's study, but is perhaps more noteworthy for its statements (especially as presented in her summary articles) on what "constructivist" and "laboratory" studies are attempting to accomplish. Finally, I want to

draw attention to the work of Sharon Traweek (1988) which, I feel, represents a more consistent and sustained effort at ethnographic inquiry than is offered by Latour and Woolgar.

The Empirical Relativist Program

A few years before the other scholars covered in this chapter entered the laboratory, Harry Collins (e.g., 1974) was already publishing the results of fieldwork carried out with practicing scientists to provide support for his Empirical Program of Relativism (EPOR), which he (1992:25-26) describes as having the following objectives:

1. Demonstrating the interpretive flexibility of experimental data.
2. Showing the mechanisms by which potentially open-ended debates are actually brought to a close – that is, describing closure mechanisms.
3. Relating the closure mechanisms to the wider social and political structure.

Before examining these three objectives in more detail, it is important to note two guiding principles that are at the foundation of EPOR. First, Collins stresses obtaining direct empirical evidence of scientific practice in the here and now. Second, he (1992:16) adopts what some might consider to be a radical form of relativism, based on the notion that: "If cultures differ in their perceptions of the world, then their perceptions and usages cannot be fully explained by reference to what the world is really like." From this perspective, social explanations are to take precedence over logic and evidence, and researchers are advised "to treat descriptive language as if it were about imaginary objects" (1992:16). Thus, for Collins, "the natural world has a small or nonexistent role in the construction of scientific knowledge" (1981:3).

Given Collins' form of relativism, the three objectives of EPOR can be interpreted in the following way. First, if the same (or a seemingly similar) scientific procedure is carried out in different settings it will produce different results. Second, scientific debates are brought to a close, and new knowledge is certified, through a process of social negotiation that takes place among particular groups of scientists. Third, these negotiations take place within the framework of existing perspectives or worldviews. The remainder of this section emphasizes the empirical support that Collins gathers in support of these ideas, rather than the details of the

(philosophical) arguments themselves. A first step is to describe more precisely how Collins views science.

Collins describes scientists as being in the business of putting ships in bottles. Generally, we only see the finished product (knowledge), and all traces of how the ship got into the bottle in the first place have been erased. Further, and to some extent following Kuhn, Collins divides scientific activity into three phases: normal (ordinary), extraordinary, and revolutionary. Normal science (most scientific activity) is carried out in a stable comfortable environment, whereas extraordinary science is characterized by small-scale controversies that are largely unsettled (i.e., scientists have not quite figured out how to get the ship in the bottle). Revolutionary science (the ship will no longer go in the bottle) brings about large-scale widespread changes to the environment in which science is done. Collins (1992) presents the results of three case studies to illustrate his ideas, the first of which represents normal scientific activity (building the TEA-laser). The other cases belong to extraordinary science -- one (gravity waves) in a well-established science (physics) and the other (the emotional life of plants) in what some might consider a pseudo-science (parapsychology). Coverage of these last two examples would draw us into a variety of debates that are not directly relevant to the examination of the daily activities of scientists in the here and now as they engage in normal/scientific activity.

The normal science case involves several scientists trying to build a copy of a Transversely Excited Atmospheric pressure CO₂ (TEA) laser. The field research carried out by Collins provides us with a picture of how scientists responded to the various challenges they encountered in trying to build this device, based on the published record of those that had successfully built such a laser.¹ Collins identifies a series of problems that the scientists had to deal with, such as: arc breakdown among the components, reducing probe coil noise, correctly sizing wire leads and glass tubing, and properly marking the anode. While scientists tried to use

¹ Collins visited eleven laboratories engaged in laser building, where he spent a small number of days, one or two, collecting information, which he followed up on in a couple of telephone conversations. This observation is not intended as a criticism. Rather, my point is that even extremely limited exposure to actual scientific practice (compared say with the nearly two years of observation carried out by Latour) can provide a wealth of insights that philosophical and historical studies are unable to provide.

their own problem-solving skills (generally based on trial and error), solutions only emerged through interaction with successful laser builders.

At the core of this problem-solving method is the notion of tacit knowledge (see Polanyi, 1962, 1967). Collins makes the point, later reinforced by the work of Sharon Traweek (1988), that the transmission of skill is not done through the medium of written words, but rather through practice and interaction. In other words, the written instructions only provide the basic knowledge needed to build a laser. The additional information has to be acquired through direct interaction with a more knowledgeable (experienced) member of the scientific community.

Part of Collins' concern here is establishing the idea that the precise replication of scientific experiments or procedures is impossible. As he (1992:2) argues,

since experimentation is a matter of skilful practice, it can never be clear whether a second experiment has been done sufficiently well to count as a check on the results of the first. Some further test is needed to test the quality of the experiment - and so forth.

Collins refers to this predicament as the "experimenters' regress,"² where the endless spiral of testing tests can only be broken when some criterion is found that is independent of the experiment itself (1992:84). In the absence of some physical confirmation (you know you have successfully created a bomb when you blow something up), "core sets" of scientists negotiate closure in a way that reflects their interest in the results, whether positive or negative. As Collins (1992:152) states: "The magic of the core set lies in the way it uses anything to make a scientific fact yet also renders all the ingredients invisible to all but the very determined investigator." According to Collins, our attention should not be on the facts but on the core sets themselves. In other words, scientific knowledge has little to do with nature and more to do with those that construct it.

What is most relevant to the present project is the fact that Collins entered the laboratories of practicing (ordinary) scientists, to see how they were able to carry out their work.

² Collins and Pinch (1993) provide further illustrations of closure and the experimenters' regress from both "high" and "low" science. That is, their examples range from some well-established and historically quite significant cases like experimental proofs of relativity theory to the case of the chemical transfer of memory, where the "jury is still out."

His findings that scientists rely on tacit knowledge and interaction with colleagues to accomplish their goals are also important for the approach to studying science developed throughout this work. Of less interest are Collins' ideas around the closure of scientific debates, which draw us back into the debate between Camap and Popper on whether, and in what way, scientific theories can be confirmed (i.e., the epistemological agenda of the philosophers).

Entering the Laboratory

Laboratory Life (Latour and Woolgar, 1979, 1986) is based on the experience of Bruno Latour, who spent almost two years as a participant observer in a neuroendocrinology laboratory. The authors indicate that their research objective is to answer two questions regarding the social construction of scientific facts:

How are... facts constructed in the laboratory, and how can a sociologist account for this construction? What, if any, are the differences between the construction of facts and the construction of accounts? (1986:40)

For the purposes of the present study, what is important is that this research agenda reflects an emphasis on process, rather than structure, and places sociological concerns ahead of philosophical or historical ones. It also implies that what scientists *are* doing may not be the same as what they *report* they *were* doing.

Latour and Woolgar's volume can be interpreted as presenting the findings of four distinct research projects. The first of these (1986:Chapter 2) recounts the adventures of a fictional observer relying on "anthropological strangeness" as he enters the world of the "tribe of scientists." What emerges from this exploration is a difference of opinion between the observer and the informants, which establishes a hypothesis that is tested throughout the remainder of the work. While "they claimed merely to be scientists discovering facts; he (Latour) doggedly argued that they were writers and readers in the business of being convinced and convincing others" (1986:88). In other words, the authors set out to "portray laboratory activity as the organization of persuasion through literary inscription" (1986:88).

Central to this argument is the notion that the main product of the laboratory is the scientific paper, or journal article. The production process itself is comprised of "operations on

statements” (1986:86). As scientists become more convinced of the factual nature of their findings, the statements they use to describe their work become more assertive. For example, the statement that certain experimental findings *may be* associated with the presence of a certain chemical substance does not carry as much “weight” as the statement that the association between the experimental findings and the chemical substance is *definitely established*. The insertion, modification, and elimination of these “grammatical modalities” not only forms the central activity in the laboratory, it provides the basis for all other activities.

There are two important aspects of this fact/article construction process that need to be emphasized. First, facts are constructed in the laboratory through an ongoing process of negotiation and social interaction among the scientists. In other words, scientific facts are a social product. Second, in constructing these facts, scientists do not have direct access to objective reality. Rather, their hold on reality is mediated through various “inscription devices,” which are defined as

any item of apparatus or particular configuration of such items which can transform a material substance into a figure or a diagram which is directly usable by one of the members of the office space. (1986:51)

The various instruments found in the laboratory are used to produce lines on graphs and numbers on charts. These are transformed by the scientists into a representation of nature that is reported in a journal article.

The second project (1986:Chapter 3) is a content analysis of historical documents (published articles) covering the period from 1962 to 1970, that trace the social construction of a scientific fact.³ This “particularly solid fact” is TRF(H), or thyrotropin releasing factor (hormone), which the authors contend is actually an artifact of a multitude of inscription processes.

Despite the fact that our scientists held the belief that the inscriptions could be representations or indicators of some entity with an independent existence “out there,” we have argued that such entities were constituted solely through the use of these inscriptions. It is not simply that differences between curves indicate the presence of a substance; rather the substance is identical with perceived differences between curves. (1986:128)

³ Latour’s first public report (1976) on his experience in the laboratory is a citation analysis of the first paper published by Guillemin’s group (1962) on the discovery of the new hormone.

Not only do the authors want to dispel the "misleading impression that science is about *discovery*" (1986:129, emphasis in original), they also want to demonstrate "what processes operate to remove the social and historical circumstances on which the construction of a fact depends" (1986:105). In other words, not only are scientific "facts" the products of operations on inscriptions, but the details of the production processes become lost, or hidden, in the more critical process of preparing "accounts" of what was accomplished in the laboratory. The basis for this situation may be that all members of the scientific community are aware of the processes that take place and thus take these activities for granted. Also, it may be the case that scientists, and the public, are less concerned with the process, than they are with the outcome.

People who had been totally disinterested by the production of forty-one articles over ten years could now become interested in the final event, which they, in turn, helped to highlight and dramatise. (1986:149)

In other words, once a phenomenon has attained "fact" status, it seems to take on a new meaning, even though there have been a number of otherwise interesting and consequential incidents along the way.

Latour and Woolgar's third project (1986:Chapter 4) puts them back into the laboratory to discover the microprocesses involved in the construction and dismantling of facts. The authors (1986:183) argue that by introducing the reader to

the microprocesses of the fact production, we have tried to show that a close inspection of laboratory life provides a useful means of tackling problems usually taken up by epistemologists; that the analysis of these microprocesses does not in any way require the a priori acceptance of any special character of scientific activity; and finally that it is important to eschew arguments about the external reality and outside efficacy of scientific products to account for the stabilisation of facts, because such reality and efficacy are the consequence rather than the cause of scientific activity.

This "deconstruction" of reality, whereby the existence of a fact "melts" back into a statement, exposes the thought processes of the scientists which arise out of "relatively brief conversational exchanges." The authors present a number of extracts from conversations within the laboratory to demonstrate how these exchanges are transformed into accounts of the genesis of ideas, that eventually lead to the construction of a fact. Latour and Woolgar (1986:153) argue that: "Scientists thus appear to operate scientifically because they are scientists." In other words,

references to logic, reason, reality, and evidence are inserted by scientists into their accounts of their everyday activity, in such a way as to differentiate themselves from non-scientists. Thus, we might conclude that scientists engage in a number of activities that are common to several aspects of everyday life, but that they also engage in a certain *rhetoric of demarcation* (see Taylor, 1996), through which they construct and maintain their special status.

The fourth project (1986:Chapter 5) is an analysis of cycles of credit, the construction of individual careers, and of those things that motivate scientists to "set up inscription devices, write papers, construct objects, and occupy different positions" (1986:189). The authors' analysis of this cycle of activity, which is accomplished largely through citation studies and examining the details of the scientists' *curricula vitae*, displays a focus on competition among scientists and the ways in which economic interests may influence the course of research. Latour and Woolgar (1986:198) explain it this way.

The essential feature of this cycle is the gain of credibility which enables reinvestment and the further gain of credibility. Consequently, there is ultimate objective to scientific investment other than the continual redeployment of accumulated resources. It is in this sense that we liken scientists' credibility to a cycle of capital investment.

Rather than being in the business of producing or discovering knowledge, scientists are in the business of being scientists (i.e., sustaining scientific activity); to be able, financially and otherwise, to engage in more scientific activity.

Economic forces tie down the researcher both as an independent capitalist and as an employee; in this position it is easy enough to squeeze him so as to extract a fact. (Latour and Woolgar, 1986:230)

These findings are consistent with the interests model advocated by the strong programme, and support a notion that the knowledge production aspect of scientific activity might be influenced by the consideration of other goals. On a day-to-day basis scientists may respond to a particular set of circumstances in such a way as to solidify or preclude a longer-term course of action. Not only do these situations illustrate the contingent nature of the scientific enterprise, they also demonstrate the fact that actions have consequences. While this latter statement may sound simplistic, the point is that this evidence contradicts a perception of science as progressing in some linear logical fashion.

Looking at the broader area of the sociology of science, the work of Latour and Woolgar contains two significant accomplishments. First, they went into the laboratory, thereby opening the "black box" of science to external examination. Second, they demonstrated, in a reflexive manner, a methodological relativism. In other words, they were able to demonstrate not only that there is no one way to do science, but that there is equally no one way to study science. With respect to the first issue, in spite of the precedent being set, very few researchers have taken up the opportunity and challenge of entering the laboratory to observe what is taking place, or even to speak directly with practicing scientists to hear what they have to say. In some ways, the second item may help to explain why scholars have not enthusiastically run out to carry on this kind of research. Latour and Woolgar were criticized by many for their relativistic portrayal of science (see Tilley, 1981; Stewart, 1982; Westrum, 1982; Cole, 1992; Weinberg, 1992), and much scholarly activity has been devoted to arguing about laboratory studies, rather than doing them (see Pickering, 1992; Lynch, 1993).

One criticism that I would have of Latour and Woolgar's project is that much of the research could have been carried out without ever entering the laboratory, or speaking directly with scientists. For example, two of the projects discussed above were based on the examination of documents. In the first instance (1986:Chapter 3), published papers were examined, and in the second case (1986:Chapter 5), a combination of citation counts and curriculum vitae material was used. With all of the time that Latour spent in the laboratory, it would have been of great interest to be presented with more evidence directly from the scientists on how they carried out their day-to-day activities.

Of more direct benefit to the present project, Latour and Woolgar draw attention to a number of processes that scientists engage in, in their daily practice: performing experiments, calibrating instruments, preparing samples, caring for animals, transcribing records, calculating statistical indices, writing drafts of papers, eliminating modalities, releasing information, and influencing others. However, rather than looking at why scientists engage in these activities, it would be interesting to examine how these activities are constitutive of the daily life of scientists. Whether scientists are engaged in providing certified knowledge about a real world, or whether

they are “merely” fabricating a worldview in support of their own political and economic interests, we will better understand science by looking past the products and motives to see the activities that take place on a daily basis, as scientists carry out their work.

Manufacturing Knowledge

Karin Knorr-Cetina (1981) carried out an “anthropological investigation of knowledge production” that is very similar to the laboratory study done by Latour and Woolgar. Her study, however, had a much stronger epistemological emphasis. Like the other scholars mentioned in this chapter, Knorr-Cetina advocates a micro-level approach to science studies, as opposed to the more macro-level approaches represented by Mertonian science studies and the case studies carried out by the advocates of the interests model. Reflecting the influence of debates in the philosophy of science, Knorr-Cetina indicates that micro-social studies of science should provide answers to such questions as:

whether scientific consensus is formed solely on the basis of evidential considerations... or... whether knowledge-constitutive laboratory selections can be fully accounted for in terms of technical rationales. (1983b:116)

More specifically, in contrast to the interests model which seeks to establish social causes, laboratory studies seek to demonstrate how scientific beliefs take shape through social interaction. Knorr-Cetina highlights four issues that emerge through the ethnographic study of knowledge production.

First, Knorr-Cetina makes the point that scientific practice is carried out on and within “a highly preconstructed artifactual reality” (1983b:119). In other words, as much as possible, the “natural” world is replaced by a collection of “products” that allow scientists to make things work. Measurement instruments, as well as articles, books and other printed materials, are all the products of previously carried out human processes. Similarly, plants and animals are specially bred and grown to meet the needs of particular types of experimental procedures. Substances and chemicals, including the water that comes out of the laboratory taps, are manufactured in “pure” form to serve as “raw” materials for the exploration of the natural world. Thus, much of scientific activity can be viewed as fabrication.

Knorr-Cetina's second point has to do with the selection processes that scientists engage in as they fabricate the products of science. In order to carry out their work, scientists select particular procedures, specific equipment, and an array of particular raw materials. Regardless of the criteria used to select one item over another, the fact that alternate selections exist (see Knorr-Cetina, 1981:12) highlights the "decision-ladenness" of scientific operations. While the details of the (social) decision-making processes may disappear in the published record (see Latour and Woolgar, 1979), Knorr-Cetina (1983b:122) argues that the "constructive" aspect of scientific work can be seen as

the sum total of selections designed to transform the subjective into the objective, the unbelievable into the believed, the fabricated into the finding, and the painstakingly constructed into the objective scientific fact.

Thus, in the presentation of "facts," which *ipso facto* are seen to emerge from a series of "right" decisions, "the natural becomes dissociated from the social" (Knorr-Cetina, 1983b:123).

The third issue centers on how these selection processes are carried out within the apparent disorder and messiness of the laboratory setting. Knorr-Cetina (1983b:123) makes the following observation:

It is perhaps the single most consistent result of laboratory studies to point to the indeterminacy inherent in scientific operations, and to demonstrate the *locally situated, occasioned* character of laboratory selections. (emphasis in original)

In other words, decisions are not made with reference to some neutral or objective set of parameters. Rather, decisions are made within specific contexts in response to whatever contingencies the scientists are dealing with at that time. These can include such things as: routine analyses offered by service laboratories, access to trained technicians, availability of adequate facilities and measurement devices, and what books and journals are at hand.

Based on her own observations in the laboratory setting, Knorr-Cetina (1981:chapter 2) provides a detailed analysis of these operations in terms of what she calls "indexicality," which refers to the "*situational contingency and contextual location* of scientific action" (1981:33, emphasis in the original). The author indicates how specific products of scientific work can be indexed as emerging out of a combination of opportunism, contingency, and idiosyncrasy. Another way to express this is to say that a unique alignment of circumstances surrounds

specific courses of action. Thus, with respect to scientific facts, “the circumstances of production are an integral part of the products which emerge” (1983b:124).

Finally, Knorr-Cetina argues that scientific activity is socially situated, in that accomplishment arises out of the social interaction that takes place not just among members of the laboratory, but also with others that may fill “non-scientific” roles.

The scientists’ practical reasoning routinely refers not only to specialty colleagues and other scientists, but also to grant agencies, administrators, industry representatives, publishers, and the management of the institute at which they work. (1983b:132)

In this way, the relationships in which scientists are involved are considered to be “trans-epistemic,” in as much as they go beyond the boundaries of a specific community of scientists, to include members of groups who may have very different perceptions of the nature and consequences of scientific work.

Based on these four observations, Knorr-Cetina argues that there are some interesting epistemological implications of the constructivist approach. The first of these is the recognition of the “indeterminacy inherent in social action” (1983b:134). Observations of scientific work do not support the search, by philosophers of science, for a single set of criteria that govern scientific activity. In the author’s (1983b:135) words, “practice can no longer be conceived as the mere execution of a predetermined order of things, but rather this order is itself a function of local closure reached in practical action.” Perhaps even more significantly, science, rather than providing a reflection of natural order or being concerned primarily with problem solving, is to be conceived of as “world-building” activity.⁴ In other words, the manufacture of knowledge is “the process of secreting an unending stream of entities and relations that make up ‘the world’” (1983b:135). Knorr-Cetina argues that this epistemological position is neither subjectivist nor relativist. Rather, it is empirical constructivist, “which conceives of the order generated by science as a (material) process of embodiment and incorporation of objects in our language and practices” (1983b:136).

⁴ This theme is explored in detail by Latour (1988), in his study of Pasteur.

In a more recent review of laboratory studies, Knorr-Cetina (1995) argues that the concept of the "laboratory" is just as problematic, to those that study science, as any other theoretical construct. "The laboratory is a means of changing the world-related-to-agents in ways that allow scientists to capitalize on their human constraints and sociocultural restrictions" (1995:145). In other words, the "enhanced" environment of the laboratory provides a place for scientists to manipulate nature in such a way as to maximize their cultural concerns, and minimize public opposition to their activities. In the laboratory, not only are "natural order time scales surrendered to social order time scales" (1995:145), but the laboratory is unable to accommodate natural objects and events, as, when, and where they take place.

Part of Knorr-Cetina's significant and lasting contribution to the sociology of science is that she has become a sort of unofficial spokesperson for the "constructivist" approach.

It is the thrust of the constructivist conception to conceive of scientific reality as progressively emerging out of indeterminacy and (self-referential) constructive operations, without assuming it to match any pre-existing order of the real. (1983b:135)

Sociologists and others interested in science may find that this approach provides a conducive point of entry into this controversial area of study.

In her review of laboratory studies, Knorr-Cetina (1995) deals with three of the major criticisms that have been leveled against the social constructionist approach to science that these studies represent. The first of these has to do with the perception that, for constructionists, material objects appear to be a consequence of scientific inquiry rather than the basis for such inquiry (see Giere, 1988; Sismondo, 1993). Knorr-Cetina suggests that a more sympathetic reading would be that the facts discovered by scientists allow for the "encounterability of material objects" (1995:161). In other words, it is not so much the case that the work of scientists creates microbes or subatomic particles, for example, but that through scientific investigation these objects become differentiated from other objects, acquiring a name, a set of descriptors, and so on.

A second criticism centers on the limitations of microsociological studies to deal with the larger scale issue of consensus formation (see Pinch, 1986; Cole, 1992). In this instance, Knorr-Cetina is in full agreement, arguing that constructionist studies have been limited to local

scenarios, and have not given sufficient attention to the ways in which the content of scientific knowledge is negotiated across multiple sites, or within specific disciplines (e.g., neuro-endocrinology).

A third related concern that scholars have with constructionist studies is that their narrow focus leads them to ignore the societal context (economic, political) within which science is carried out (see Fuller, 1993). Knorr-Cetina also agrees with this criticism, but argues, in a sense, that there is enough work for constructionists to do just concentrating on the production of knowledge, without entering more directly into considerations of social policy.

Summarizing the present state of laboratory studies, Knorr-Cetina (1995:163) states that

a most interesting area of future extension would be one in which we move from science and technology studies to sociology in general and consider the possibility that laboratories exemplify features also present in organized settings such as the clinic, the factory, the garden, the government agency.

This is precisely the kind of contribution that the present project is trying to make. If the sociology of science is to be taken seriously by other sociologists, then some kind of common ground must be established so that research results from one area can be used to shed light on research into other areas of human group life. This will be accomplished in part by a move away from historical and philosophical concerns with science, and “will profit from (but need not be bounded by) the microscopic investigation of scientific practice” (Knorr-Cetina, 1983b:136).

The High Energy Physics Community

Sharon Traweek's *Beamtimes and Lifetimes* (1988)⁵ presents the results of an ethnographic examination of the high energy physics community. Traweek's study is, I would contend, more successful than the study by Latour and Woolgar (for a dissenting opinion, see Latour, 1990). What I mean by successful is that Traweek provides greater descriptive detail of the artifacts, customs, and language of the scientific community, while at the same time avoiding the temptation to look for answers to fundamental philosophical questions about the nature of science. Traweek's (1988:1) goal is to demonstrate “how high energy physicists see their own

⁵ As with the other initiatives discussed in this chapter, the fieldwork for this study was carried out in the mid-1970s.

world; how they have forged a research community for themselves, how they turn novices into physicists, and how their community works to produce knowledge." At the base of this inquiry is the anthropological assumption that high energy physicists are part of a community, which Traweek (1988:6) defines as

a group of people who have a shared past, hope to have a shared future, have some means of acquiring new members, and have some means of recognizing and maintaining differences between themselves and other communities.

This emphasis on community is consistent with the work of Kuhn and Fleck, and allows the researcher to describe a broad array of processes and activities that take place not just within the community, but during encounters with other groups.

Traweek begins her presentation with a description of the environment in which the physicists carry out their work. On one level, Traweek is referring to the literal physical environment (laboratories) in which she carried out her research, namely, SLAC (Stanford Linear Accelerator Center) in California and KEK (Ko-Enerugie butsurigaku Kenkyusho) in Tsukuba, Japan. These two labs are among a handful of high energy physics facilities around the world (others include Fermilab near Chicago and CERN in Geneva, Switzerland). SLAC covers a 480-acre site and consists of a complex of buildings, the two-mile long particle accelerator, the Beam Switchyard and the Research Yard, where the actual experiments are carried out. The buildings are dedicated to the work of specific groups (the Central Lab, Administration and Engineering, Test Lab, Computer Center), and the highly structured nature of the physical environment is reflected in nearly every aspect of what goes on at the facility. So, for example, the offices of the theoretical physicists are on the same floor as the laboratory directors' offices, one floor above those of their lower status colleagues, the experimentalists.

On another level, Traweek introduces us to the human environment, which is comprised of different groups and the relationships among the members of these groups. In 1978, SLAC employed over twelve hundred people, of whom only about eight percent were actively engaged in physics research. The rest of the employees were clerical, operational, and technical staff, and various managers and administrators. As an example of the structuring of jobs at SLAC, Traweek mentions the so-called "scanners," who scan photographic film (the experimental

record) for particle traces. This job was originally performed by young physicists, then by graduate and undergraduate physics students, and then by members of the general student population. Eventually, it was determined (by physicists) that people did not need to know any physics to perform this task, and so the job was relegated to a low status, technical line function.

With respect to the relationships among the various employees at SLAC, Traweek indicates that group members tend to stick together and that they "tend not to mingle across job classifications" (1988:25). Office location, style of dress, and selection of lunch partners (not to mention gender and race) all give clues to group membership. Even among the physicists there are boundaries with respect to daily interaction, reflecting the differentiation between those that carry out experiments and those that do their work "at the blackboard." As one informant indicated to Traweek, "an experimentalist would probably feel awkward among the theorists, who have more status" (1988:33). What appears to bind the members of all of the various groups together, however, is a "great reverence for science, scientific discovery, and the laboratory" (1988:31).

At the center of Traweek's study are the "detectors," which consist of the targets, at which accelerated particles are directed, and the various recording devices and computing systems that provide an interface between the targets and the physicists. Following a brief history of the development of detectors from bubble, spark, and streamer chambers to the newer devices such as the large aperture solenoid spectrometer (LASS), Traweek (1988:49) discusses four different detectors (three at SLAC and one at KEK) that represent "four different strategies for making discoveries: four different strategies for spending money, organizing a research group, and becoming a success in science." Irrespective of the differences among these detectors, and the strategies surrounding them, the critical element in high-energy physics research is gaining access to "beamtime." That is, much of the work that is carried out by the groups of researchers can be conceived in terms of how it will help the group to get their detector in front of the stream of accelerated particles for as long as possible. Traweek (1988:49) argues that the detectors serve as the "signature" of the group, and that "their conception and development, their maintenance, their performance during the precious allotment of beamtime

for an experiment – are the stuff of frustration, hope, heartbreak, and triumph for research groups.” With respect to her own research, Traweek (1988:17) states that: “The detectors in the end are the key informants of this study; physicists and nature meet in the detector, where knowledge and passion are one.” The detectors, in some sense, embody each research group’s model of how physics and, by extension, science are to be carried out.

One interesting observation that Traweek makes in her discussion of detectors is with respect to the difference between American and Japanese experimenters, in terms of the division of labor in the research process. In Japan,

the experimenters did not have the means to make and unmake their own equipment... Their own training had not prepared them for dismantling and rearranging detectors, but for overseeing the work of engineers. (1988:69)

So, even though the primary goal of these experimenters may be the same as that of their American counterparts – namely, access to beamtime – the dynamics of the human interactions that take place throughout the research cycle may be quite different.

The next section of Traweek’s study deals with the ways in which the careers of physicists can unfold. In this regard, Traweek (1988:74) identifies three stages that students go through in order to establish themselves within the high energy physics community: undergraduate, graduate, and research associate. These three stages generally comprise about fifteen years of formal study and apprenticeship, and individuals can be eliminated, or leave voluntarily, at several points along the way. Beyond becoming an established member of the community, a limited number of individuals can go on to fill roles as group leaders and “statesmen.” In this latter role, which involves interaction with people outside the community, physicists no longer carry out research.

Teaching, administration, and consulting for the government are potentially contaminating because they require the cultivation of skills not thought to be based on reason - in particular, the power of persuasion. (Traweek, 1988:102)

Even though these statesmen hold senior administrative positions within the community, they can become marginalized by other more active scientists who feel that they no longer have “any science left in them” (1988:102).

Following her discussion of the individual's "life in physics," Traweek goes on to examine those elements that bind the community together. The first of these are the networks, or sets of relationships (usually international), that serve as channels of communication for the planning and evaluation of work, and through which students are placed at various points in their careers. Included in these networks are physicists at a number of universities (65 in the case of SLAC) that are affiliated with the labs, who will enter into various exchange relationships (e.g., training a student in exchange for greater access to data) with the research groups at the lab. All of the institutions within and between these networks are ranked (according to human and material components), and even though some national preferences emerge, there is wide international recognition of the hierarchy. Physicists believe that the best people end up in the best places, and that the system is designed such that "knowledge trickles down and students percolate up" (1988:110).

Another important aspect of the high energy physics community is the division of labor between experimentalists and theorists. The experimentalists, who deal directly with the detectors, tend to work in larger groups for longer periods of time than the theorists, who work mainly in their offices. While these two groups work closely together, they do so with a "studied disregard for each other's judgment" (1988:12). Even within these groups there are further subdivisions. For example, some theorists (phenomenologists) work on matching data with existing models, while another group (mathematical physicists) work on linking developments in mathematics with ideas emerging in particle physics. A third group of theorists, who generally consider themselves superior to the previous two groups, work on developing new models. Experimentalists tend to be divided by the issue they are dealing with (e.g., neutrino production) and the type of equipment they are using (e.g., spectrometers).

Traweek (1988:117) argues that when it comes to communication, restricted information is spoken, while public information is written. In other words, physicists use conversation to evaluate the work of others, to negotiate for resources, and to pass on "insider" information about the group's work or how to move up the career ladder. "Physics and its culture is produced and reproduced through talk which is storytelling, talk which is judgmental, talk which punishes and

rewards." Most of what appears in writing is either out-of-date, or meant for public consumption (i.e., it has been diluted).

As a final brief point on community, Traweek notes that in spite of the best efforts – with respect to objectivity and rationality – that physicists can put forward, “uncertainty and error remain present and acknowledged in scientific work” (1988:125). Peer review and collective surveillance ensure that established standards of objectivity are adhered to, but “the final degree of order comes from human institutions” (1988:125).

Following a discussion of some differences in the ways that the American and Japanese high energy physics communities negotiate their place in their respective cultures, Traweek completes her presentation with an epilogue that contains her reflections on what “knowing” means for the physicists. Traweek observes that physicists strongly deny human agency in the construction of science, and that they tend to “see data and nature as equivalent” (1988:160).

They have a passionate dedication to [a] vision of unchanging order: they are convinced that the deepest truths must be static, independent of human frailty and hubris. (1988:17)

Their continued efforts depend on the belief that they are making discoveries about a real world. For them, it is common sense that the things they are working on (subatomic particles) exist. Traweek believes that it is not the position of the ethnographer to question this assumption, but rather to gain insight into how physicists construct a world in which these claims can be made. Just as the physicists must start somewhere when they set out to accomplish their goals, Traweek (1988:162) argues that “unlike some of (her) more reflexivist colleagues, (she) finds it appropriate to assume that physicists exist.”

Traweek, more than Latour and Woolgar or Knorr-Cetina, demonstrates the diversity and complexity of the everyday world in which scientists work. The image we get is one of science as a “human” enterprise, where people with different skills, perceptions, and ambitions are continually interacting with each other and interpreting the things around them. For Traweek, science and scientists are not static entities. Rather, both the nature of the scientific enterprise and the identities of the people engaged in it emerge out of the daily practice through which goals are accomplished. An integral part of this intersubjective accomplishment is the process of

maintaining a community of practitioners who share a common worldview. As Traweek indicates, the primary mechanism involved here is verbal communication.

So, Traweek's study shifts our focus away from a concentration on the products of science, and away from trying to solve philosophical issues of what is real and how do we know. Consistent with the approach advocated in the present study, Traweek draws attention to the messy and contingent nature of the everyday life of scientists, and shows how the dynamics of dealing with circumstances that might arise depends on the establishment of a meaningful environment within which to work. In this way, Traweek's work clearly illustrates Kuhn's notion of paradigm-as-shared-commitments. One drawback, however, is that Traweek carries out her research with a fairly elite (extraordinary) group of scientists, that might be considered by some to be different in some way from the vast majority of scientists. Thus, the present study advocates examining a cross-section of scientists engaged in a variety of pursuits in different disciplinary areas. I suggest that the kinds of activities carried out within the high energy physics community are common to other areas of science as well.

Summary

This chapter has reviewed four important initiatives in the sociology of science that were carried out in the mid-1970s in response to developments in the history, philosophy, and sociology of science. As mentioned at the start of this chapter, these laboratory studies represent an effort to study the processes of science, as they are engaged in by working scientists. All four of these projects are "anthropological" in that they involved an outsider entering the world of a group of "others" to study the members of that group in their own environment. However, each of these initiatives presents a somewhat different view of what constitutes science and how it should be studied.

Collins studied normal science activities carried out by a variety of research scientists at a number of separate locations. Unfortunately, this transcontextual and transsituational element of laboratory studies has not been followed up on to the extent advocated in the present project. Collins also emphasized the critical role that face-to-face interaction plays in the accomplishment

of scientific goals. Again, this focus has not received the attention it deserves. Perhaps it is Collins' radical relativism that has led potential researchers to distance themselves from Collins' approach. Similarly, Collins' emphasis on closure mechanisms may have led him and others away from the direct observation of practicing scientists towards more philosophical pursuits.

For Latour and Woolgar, science differs from non-science through the use of journal articles to construct facts. Members of other human groups may produce written output, but it is scientists that produce facts thereby. The determination of who is, and who is not, a scientist is made by other scientists through the examination of this written record of activity. The way to study science, then, according to Latour and Woolgar, is to "deconstruct" these products of the scientific enterprise to determine exactly what these products represent. By entering the laboratory, the researcher can witness the processes of social interaction and interpretation that scientists engage in as they deal with the inscriptions that emerge from the various devices that mediate between nature and themselves. At the same time, the many details of the complex process of paper production can be described and analyzed. The picture that emerges is one that calls into question any kind of exceptionalist notion of the scientific enterprise. Scientists, like members of other human groups negotiate their daily lives, and work towards ensuring their own survival, in a highly competitive and uncertain environment.

While Knorr-Cetina does not limit her definition of scientific output to papers that get published in scientific journals, she nonetheless sees the activities of scientists as centering on the construction of facts, whether or not these facts match anything that might be considered objectively real. Similarly, for her, the construction process represents a series of compromises and practical decisions that cannot be conceived as strictly rational or objective. Rather, science is carried out in a highly controlled "partial" environment that results from a series of "transepistemic" considerations. In other words, the goals and perceptions of a number of groups (scientists "inside" the lab, scientists "outside" the lab, funding agencies, government policy agencies) are taken into account in the making of all decisions. Thus, scientific facts are manufactured out of these social processes. For those that study science to select the laboratory as the environment in which to carry out their research is to engage in the same kind of

fabrication process that the scientists themselves engage in. In other words, those studying science cannot assume that what they are observing is in any way "natural" or "real." Like science itself, the study of science is indexed by a number of selections that make it more manageable on a day-to-day basis.

For Traweek, science is a community-based activity aimed at constructing and maintaining a certain working environment and worldview. This position is similar to that arrived at by Thomas Kuhn, and it is most consistent with the position advocated in the present project. In other words, rather than starting from some *a priori* notion of what constitutes science, Traweek accepts the physicists' "definition of the situation," and then examines the consequences of that definition. The boundary between science and non-science is based on a number of practical considerations that center on the survival of the group. If group members decide that they are engaged in the "pursuit of truth," and this helps them to maintain their identity and recruit new members, then they will act in such a way as to convince outsiders that this particular activity is one of the things that makes them different. The acceptance of this demarcation criterion by insiders and outsiders is something concrete and practical in that it shapes courses of action. It is not a philosophical abstraction, to be contemplated apart from human group life.

All four of these initiatives clearly demonstrate that going into the laboratory to study science can result in significantly different perceptions of what is going on in science, than would be produced by studying it as the historians and philosophers of science do. For example, the conception of the "product" of scientific activity shifts from published articles (Latour and Woolgar), to facts (Knorr-Cetina), and finally to community (Traweek). One consequence of the presentation of these diverse views has been that historians, philosophers, and sociologists of science have expended considerable energy defending their own conceptions of science and attacking the ideas of others. Unfortunately, very little effort has been expended in sustaining the exploration of scientific practice, as it is practiced. So, as Knorr-Cetina (1995:141) states:

The real-time processes through which scientists, one of the most powerful and esoteric tribes in the modern world, arrive at the goods that continuously change and enhance our "scientific" and "technological" society are still hardly understood.

There are a number of themes that emerge from the material covered in this chapter that can serve to demarcate an alternate formulation of the social study of science. First, science is not monolithic. The social study of science means the study of particular groups of scientists that may or may not share anything in common beyond their conviction, recognized by others, that they are doing science. Second, the social study of science has to involve contemporary, practicing scientists as research subjects. No amount of historical or material evidence can substitute adequately for direct contact between the researcher and practicing scientists. Whether or not this contact should take place in the laboratory is an open question. Third, the researcher should not enter the field with the assumption that scientists are, by definition, seekers after truth. Scientists, like members of other human groups, work towards accomplishing goals that they have set for themselves. What the researcher should be interested in is how scientists go about accomplishing these goals, whatever they may be.

While the works examined here provide the general context for the present project, and for anyone attempting to study science, a handful of scholars have tried to draw together some of the disparate elements outlined here to construct what they consider to be more coherent and systematic approaches to understanding the scientific enterprise. The next chapter is devoted to an examination of a selection of these initiatives.

Chapter Four

MOVING SCIENCE STUDIES FORWARD

At the end of the last chapter the phrase, "social study of science," was used to express an alternate approach to the study of science defined by three premises. First, even though the study of science will be carried out in particular situations and contexts, there is no reason to believe that what is taking place in these individual scenarios is any more or less "scientific" than what might be taking place elsewhere. In other words, we must be careful not to limit the selection of potential research sites, based on the notion, for instance, that studying physicists is somehow preferable to studying biologists, or that studying award winners is somehow preferable to studying scientists in the early stages of their careers. Second, research on the scientific enterprise should involve, as much as possible, direct interaction with practicing scientists as research subjects. Finally, researchers should not enter the field with the assumption that the only goal of scientific activity is to arrive at truth. At the same time, however, the phrase "social study of science" allows us to make some other useful distinctions. First of all, Mertonian-inspired research in the sociology of science is still being carried out, and this kind of research activity may not fit the criteria just stated. Secondly, some historians, philosophers, and sociologists of science are engaging in various multi-disciplinary approaches to the study of science, and the phrase is a convenient way to express, first, that these efforts do not fall within strict disciplinary boundaries, and, second, that these research initiatives are very much concerned with the social aspect of science, variously defined. Clearly, a degree of caution is required in the use of this phrase, just as in the case of using the term "science."

With respect to the latter conceptualization of science studies, the kind of openness that characterizes research of this type can give rise to such a degree of fragmentation that individuals or groups of researchers are often unable to appreciate or sufficiently comprehend the work of others. Too often, this situation can lead to energies being expended on polemics rather than field research (see Pickering, 1992). The symbolic interactionist approach to the social study of science, advocated in this project, is designed specifically to meet the premises

stated above. The present chapter is devoted to examining a group of works that fall under the broader definition that encompasses multi-disciplinary approaches.

Four particular works have been selected for examination here that not only provide a representative sample of recent research initiatives being carried out, but that make specific contributions to the position developed in the present project. The first work examined is Susan Leigh Star's *The Regions of the Mind* (1988), which represents research in the "social worlds" perspective established by Anselm Strauss, and which is characteristic of a series of science studies carried out by scholars associated with the Tremont Research Institute in San Francisco. The second work, Stephan Fuchs' *The Professional Quest for Truth* (1992), advocates an organizational approach to the study of science that is based on the assumption that technology and social structure determine the way scientists carry out their research. Andrew Pickering's *The Mangle of Practice* (1995) presents a view of science as practice and culture. For Pickering, scientific activity must be viewed as a process, as taking place in time, and those that study science must take into account the active role of both human and non-human agents.¹ Finally, Stephen Barley and Beth Bechky's "In the Backrooms of Science: The Work of Technicians" (1994) represents an attempt to study science from within the context of other studies of work and occupations being carried out by sociologists. This study, more than the other three, provides insights into the daily practical accomplishments of working scientists, and thus comes closest to the agenda of the present project.

With all of these research initiatives, it is important that the details of how the researchers define science and how they propose to study science be drawn out. One of the primary benefits of any study of science is that it provides a way for others to carry on with the research agenda that it establishes. While each of the four works examined here may meet this requirement, our specific concern is to determine if any of these works represents a "social study of science," as outlined above.

¹ The concept of non-human agency in science comes from Bruno Latour's *Science in Action* (1987). While Latour's work could have been covered in this chapter, it is not. Its influence and significance do become clear in my discussion of the works of both Fuchs and Pickering, however.

Social Worlds Perspective

In her introduction to a special issue on the sociology of science and technology in the journal *Social Problems*, Susan Leigh Star (1988) describes the beginnings of the "Tremont" school.

The combination of fieldwork and anti-positivism was familiar to a group of researchers... at the Tremont Research Institute in San Francisco. We had come from symbolic interactionism and pragmatism and... Among our common interests and beliefs was the necessity... in order to demystify science... to analyze the process of production as well as the product. (1988:188)

As indicated in a number of their works (see Fujimura et al., 1987), the Tremont school scholars have attended to earlier studies by such symbolic interactionist oriented scholars as Howard Becker, Anselm Strauss, and Everett Hughes, rather than to other "sociologists of science," to provide the basis for their approach to science. One example of this early work, by Strauss and Rainwater (1962), attempted to discover chemists' opinions on the role and function of a professional society for its members. While many of the findings were based on survey material, Strauss did complete a number of open-ended interviews to better ascertain how members felt and acted toward other chemists.

A more critical foundation, however, was provided by the work of Everett Hughes on institutions as "going concerns" (1971:52-64). Hughes advocated looking at collective enterprises and how people cope with the various contingencies that arise through their life-courses. Hughes also identified the necessity of getting to the "inner workings" (1971:14-20) of institutions, rather than settling for a more structuralist view of them as objective entities. The influence of these ideas can be seen in the following outline of the Tremont research program.

In their paper "Symbolic Interactionism in Social Studies of Science" (1990), Adele Clarke and Elihu Gerson present the following four assumptions as the basis for interactionist science studies:

- a. all scientific facts, findings, and theories are socially constructed.
- b. knowledge represents and embodies work, a particular way of organizing the world through a series of commitments and alliances.
- c. science is best approached as a matter of work, organizations, and institutions.
- d. scientific work, institutions, and knowledge are not essentially

different from other kinds, nor in any way sociologically special.
(Clarke and Gerson, 1990:181-2)

This approach appears to advocate using the concepts of work, organizations, and institutions to understand both what makes science distinct from, and similar to, other areas of human endeavor. The empirical content of these conceptual structures is seen to be the outcome of social construction processes carried out by the members of the group being studied. In other words, work, organization, and institution are situational and contextual. Another feature of this approach is that knowledge is conceptualized in practical terms, implying a desire to avoid the kind of epistemological speculation characteristic of many other approaches to science. According to this program, then, science is a particular social construct within which a group of practitioners carry out work directed at producing a certain type of knowledge, the methods for obtaining this knowledge, and the structures within which their work is carried out. What is less clear, however, is how social researchers are to go about studying these scientists.

The Tremont researchers' emphasis on structure is consistent with the so-called "social worlds" interpretation of symbolic interactionism (see Bucher and Strauss, 1961; Strauss, 1978, 1983, 1984; Gerson, 1983). Social worlds, which can be defined as "dynamic groups that are constantly aligning, claiming territory and resources, and merging and splitting" (Star, 1989:39), are seen as "contexts for work and for the negotiations associated with work organization" (Gerson, 1983:357). Gerson (1983:370) describes three kinds of social world processes that are important for understanding scientific work:

segmentation processes, in which lines of work split and diversify;
intersection processes, in which lines of work interact and influence one another; and *legitimation* processes, which establish boundaries among lines of work as well as standards and criteria for evaluating research efforts.

So, for example, myrmecology (the study of ants) is a segment of entomology, which is a segment of invertebrate zoology, which is a segment of zoology, which is a segment of biology. At the same time, certain concepts (e.g., gene) or material resources (e.g., microscope) may be common to all of these segments. Similarly, researchers in all of these segments will be concerned with what constitutes a legitimate research topic and how others will react to their work. With respect to future efforts to increase our understanding of these processes, Gerson

(1983:370) recommends studying long-term trends, revolutions and sudden changes, timing issues in discovery, and programs and institutional contexts.

The most extended example of research in the Tremont tradition² is Susan Leigh Star's *Regions of the Mind* (1989), in which the author traces the development of the localization theory of brain function in the late 1800s and early 1900s. The overall goal of Star's study is to determine the nature of scientific change, and she argues that this process is situated in scientific practice and work organization (1989:3). Star's historical study of theory change demonstrates how over a thirty-six year period, one particular view of how the brain functions, and therefore how it should be studied, emerged to dominate research and medical practice with respect to neurological disorders.

Rather than comparing ideas, Star (1989:7) states that the argument of her book is that

because the theory of localization of function was embedded in, and indistinguishable from, scientific practice, it was not the logical evolution of the theory that was compelling to practitioners, but rather the way their work was structured. By understanding the way work was organized, we can find the only possible solution to the problem.

The problem referred to here is the problem of how, in the face of uncertainty (anomalies), a group of scientists was able to select one model of brain function over another. Through the examination of historical documents, Star claims "to attend to the ways in which scientists merge different types of work to produce a theory" (1989:34). For Star (1989:187), "the truth of a theory does not rest upon its match with logical formulations but derives from historical, work-based, and institutional processes within lines of work." In other words, theory choice, and thus theory change, emerges out of how scientists deal with practical work-related conditions. This conclusion is certainly consistent with the agenda established by the Tremont group, and it clearly emphasizes the role of the social in scientific practice. It also supports a view of science as a primarily pragmatic process (see Solomon, 1995).

The evidence that Star presents is historical, primarily from documents regarding activities at the National Hospital for Nervous Diseases, Queen Square, London, in the period

² Other examples of Tremont school research include: Star (1983); Fujimura (1987, 1988, 1991, 1992, 1996); Fujimura, Star, and Gerson (1987); Star and Gerson (1987); Clarke (1990).

from about 1870 to 1906. The materials provide details of the organizational and institutional working environment in which two competing theories of brain function, the "diffusionist" and the "localizationist," emerged, were tested and contested, with the localizationist theory finally emerging victorious. The central foci of Star's analysis are the principal advocates of the opposing views, and the networks of allies that they were able to bring together. Throughout the work, we learn a great deal about the state of brain science at the time, the clinical and basic research that was used to support these theories and the ambiguities and uncertainties that accompany scientific work.

Star indicates that one of the principal elements of the organization of work is the "management of uncertainty," and that scientists deal with four specific types of uncertainty: taxonomic, diagnostic, organizational, and technical (1989:67). The first of these types refers to the ambiguities that arise as scientists attempt to classify the phenomena that they encounter in their work. These ambiguities can arise from a lack of information or from the scarcity of experimental material (1989:70). So, for example, in the 1880s, neurosurgery was a new specialty and strictly enforced antivivisection restrictions were in place. Two ways that scientists responded to this situation were to standardize the form in which they handled data and to seek out exemplary, unambiguous cases that would provide them with high quality data. Diagnostic uncertainty has to do with the problems of applying these classification systems in new situations and contexts. This was particularly problematic for the scientists studied by Star, because many diseases, or conditions, of the brain shared a number of symptoms (1989:74). One way that this was dealt with was to use secondary symptoms to support one diagnosis or another (e.g., headache, impaired vision).

Organizational uncertainty refers to the creation and maintenance of the various relationships in which scientists find themselves. These relationships are important to ensure access to information and other resources, and can reflect a variety of financial and political alliances. One critical element at the turn of the century in medicine was the conflict over status and the division of labor between physicians and surgeons (1989:80). The final type of uncertainty arises out of the daily use of various instruments and materials by scientists to help

them accomplish their goals. For example, at that time, one could never be sure how much, or what type of, electrical current would be available. The goal in all of these cases is the creation of certainty, but as Star (1989:66) notes,

I have interviewed many scientists who characterize the management of uncertainty as "not really science" (and with pejoratives including "mere administrative work," "dirty politics," "bean-counting," "mere logistics," and even "sociology"). *Real* science, on the other hand means contributing to Truth *despite* these local "glitches" or "kludges." But certainty develops as local contingencies are jettisoned, minimized, distributed, or otherwise resolved. Taking care of local uncertainty, including its transformation to certainty, is not incidental; it is central to research organization, despite its depreciation by scientists. (emphasis in original)

While these observations may represent the way in which science is viewed by a particular group of scientists in the 1980s, how are we to determine if scientists at the center of Star's project, one hundred years ago, shared this perception? The simple answer is that we cannot. However, I think that it is clear from Star's research that the activities around managing uncertainties consumed a considerable amount of time and other resources, and that the result of these efforts played a significant part in determining the scientific outcome. One clear advantage of being able to study these processes in the here and now is that the researcher can gain a better appreciation of just how much effort, and of what kind, goes in to managing uncertainty. Scientists may, upon reflection or after the fact, deprecate this kind of activity, but that does not mean that it is not integral to the scientific enterprise as it is practiced on a daily basis.

One interesting observation from Star's study is her recognition of the diversity of activities that comprise scientists' statements about their work (see Latour and Woolgar, 1986).

For example, she presents the following list of activities:

- a problem was posed;
- monkey bought;
- animal fed and housed;
- skills in surgery and chloroforming acquired;
- an incision made;
- electrodes applied;
- muscle twitches observed;
- twitches coded and written down;
- twitch codes organized and analyzed;
- results noted;
- hypotheses tried out;
- report written;
- report submitted to professional society;
- report refereed and revisions argued about,

some included, some ignored;
 report accepted for publication;
 report published. (1989:33)

Star indicates that this set of processes should be substituted for the scientist's statement that "my experiment proved x." Here, Star recognizes that science is not just about the epistemologically and rhetorically loaded statement "my experiment proved x," but it is also about the diversity of activities and choices that lead to such a statement being made (see Knorr-Cetina, 1981). The apparent linearity or ordering to the activities in this list does not reflect the possibility that many of these activities may be taking place simultaneously, at various levels of efficacy and efficiency. Recognizing this, Star (1989:20) indicates that science should be viewed as an "open system" (see Hewitt, 1985), which is decentralized and continuously evolving. This view is consistent with an interactionist perspective on science that sees the meanings that are attached to objects as arising out of, and being refined through, processes of social interaction and interpretation.

Star (1989:197-198) identifies the following implications of her study:

theories about nature have enormous political implications. ...the kind of theory/work links sketched here are useful for researching the history of scientific disciplines. ...the study of how theories take hold and become seen as "natural" is important in answering some basic questions in the sociology of knowledge and epistemology. ... Implicit in this approach is an equation between *knowing* and *working*. These two kinds of events do not proceed in parallel: they are the same activity, but differently reported. (emphasis in original)

There are a number of elements here that need closer examination.

First, Star's work is an exemplary demonstration of how to carry out research in the history of science incorporating the ideas of Thomas Kuhn. The study deals with a specific community of scientists and draws attention to the debate over the emergence of a particular paradigm. The evidence presented clearly shows how localization theory (paradigm) can be seen as both shared commitments and shared examples. The historical analysis also focuses on the resolution of anomalies that arise not just around the more strictly scientific aspects of the debate, but also with respect to the economic, social and political environment within which the choice of paradigm is made. Another important element here is the equation of knowing with working. Not only does this statement suggest that the dichotomy between philosophy

(epistemology) and sociology should be broken down, it also implies that philosophers and sociologists may be talking about the same things, just in different languages. This observation points to Kuhn's notion of incommensurability, and highlights the fact that progress in science studies may be retarded by the inability or unwillingness of the different groups studying science to reach some common ground. Moving beyond these ideas, though, it will be instructive to examine how Star's work represents a social study of science, as defined by the present project.

To begin with, Star's project, which is historical in nature, starts with a scientific product, namely, localization theory, and tries to establish the causal path that "determined" this theory. One potential drawback of this kind of analysis is that details of other activities that the scientists may have been engaged in, that do not directly contribute to the goal selected by the modern researcher, may get filtered out. Even though Star draws attention to the parallels between working and knowing, she appears to maintain a distinction between those activities that can be considered scientific and those that are a consequence of the social organization of work. The risk involved with this stance is in reproducing the positivistic and Mertonian separation of the context of justification and the context of discovery, which leaves open the question of determining when scientists are doing science. Finally, Star's project is guided by an effort to come to grips with the relationship between theory and evidence (Duhem-Quine thesis), and she concludes that no matter how logical the theory or compelling the evidence, the deciding factor in paradigm choice is social. The problem with this emphasis is that, to some degree, Star is being drawn back into attempting to resolve philosophical problems through the social study of science.

Star's study appears to meet our first criterion for a social study of science, in that it is based on a particular group of scientists dealing with a specific set of circumstances, as they attempt to achieve a specific goal. The historical nature of the study, however, precludes it from meeting the second criterion of involving practicing scientists in the here and now. With respect to the third criterion, the decision is harder to make. At one point, in support of a more social and pragmatic orientation, Star (1989:19) indicates that "[t]ruth, viewed pragmatically, means

consequences. In the case of a collective enterprise, it means shared consequences." On the other hand, she (1989:65) argues:

It is in understanding the practices by which uncertainties are resolved that the sociology of science can make a real contribution to the old philosophical discussions of generalizability and falsifiability.

So, even though Star may be seen to be supporting a change in the ways that scholars talk about truth (see Solomon, 1995), we are left asking why research in the sociology of science should be carried out in support of an "old" agenda set by philosophers. Should the sociology of science not have an agenda set by sociologists?

The point of this brief review is not to argue that the Tremont researchers have not contributed to our understanding of science. Rather, my point is that the kind of interactionist science study represented by Star's work is not fully as consistent with the interpretation of symbolic interactionism advocated in the present project as I would prefer, nor with the criteria established for an alternative approach to social studies of science. Historical research can document what has gone on in science in the past, and it can give rise to a number of questions that are well worth exploring in contemporary settings. The goal of interactionist science studies, though, should be to observe how meanings emerge in daily practice. As Star (1989:190) acknowledges, the details of scientific fact production are likely to disappear in the writing of articles for publication (see Latour and Woolgar, 1986). Similarly, interactionists studying science, or any other realm of human group life, should be wary of the historical record of events.

An Organizational Approach

Stephan Fuchs' *The Professional Quest for Truth* (1992) is similar to Star's study and other works of the Tremont school in that it emphasizes the way in which scientific work is organized, and it does not posit some essential difference between scientific work and work of other kinds. Rather, Fuchs presents a "theory of scientific organizations" (1992:7), whereby science is conceived of "as a conflictual and stratified struggle over organizational and symbolic

property" (1992:6, see Restivo and Collins, 1982; Collins and Restivo, 1983a, 1983b). What differentiates science from non-science is that science

can draw upon more and more powerful material and symbolic resources ...this is the structural reason for the authoritative status of science: there is too much capital invested in science to successfully challenge it. (1992:14)

Comparing his work to other efforts at understanding science Fuchs indicates that:

There is a strong tendency in social studies of science, most notably in the sociology of controversies, to interpret the term "social" to mean that the task is to show how social factors influence the internal workings of science. But in this interpretation, the "social nature of science" reinstalls the old epistemological dualisms between "internal" and "external" aspects of science. (1992:75)

In contrast to this tendency, Fuchs argues that a strong social theory of science must meet three criteria:

it must show how the very core processes of science, such as fact production, are social processes; it must have a strong explanatory orientation (which includes the reflexive application to sociology itself); and it must provide a comparative framework for assessing historical and disciplinary variations in scientific practices. (1992:109)

As with the strong programme (see Bloor, 1991), what Fuchs is trying to develop is a general theory of knowledge from within the framework of sociology that can account for the apparent unity and superiority of the scientific enterprise while at the same time recognizing its diversity and complexity.

Part of Fuchs' strategy is to retain Robert Merton's emphasis on the organization of scientific communities, particularly regarding stratification, without, at the same time, accepting the functionalist and normative biases implicit in Mertonian science studies.

[T]he Mertonian celebration of the ethos of science results from the captivating and persuasive force of professional ideologies and mystery production. This celebration is part of the uncritical worship of the professions practiced by functionalism. ...to start a sociology of science with buying into its ideological mystifications is hardly a reasonable strategy. (1992:147)

Building on the work of Bruno Latour (1987), Fuchs argues that professional scientists mobilize elaborate networks in support of their statements and constructs. "To induce other scientists to turn statements into facts, researchers draw upon a variety of textual (reality and rationality) and non-textual (references, numbers, laboratories, property) resources" (1992:75). In this way,

“scientific knowledge appears to have special epistemic authority; but it is the special social authority of science’s superior resources that lies at the core of its privileged epistemic status” (1992:76).

Coupled with this structural focus, Fuchs makes use of neo-Durkheimian grid-group theory, particularly as found in the work of Mary Douglas (1966, 1970), to demonstrate the continuity between scientific and other forms of knowledge. The “grid” concept refers to vertical lines of social stratification, while the “group” concept refers to horizontal lines of social integration. Social structures can exhibit high or low levels of each of these variables and thus can be placed within a specific quadrant on a graph. For the purposes of Fuchs’ analysis, the group concept is replaced with the notion of “mutual dependence,” which signifies “the extent to which scientists are dependent on particular network of collegiate control that organize the distribution of reputational and material rewards” (1992:81). Similarly, the grid concept is replaced with the notion of “task uncertainty,” which indicates “the extent to which scientific production is routinized and predictable” (1992:82).

Fuchs (1992:97) argues that what we generally conceive of as the “hard” sciences would be located in the box signifying high mutual dependence and low task uncertainty. In other words, a group like high energy physicists (see Traweek, 1988) that shares an extremely expensive and highly concentrated resource base are likely to share extended periods of time together and create a more unified worldview. At the other end of the spectrum, the “soft” (social) sciences are characterized more by low mutual dependence and high task uncertainty. These groups are largely autonomous, and may represent a broad array of worldviews. The continued existence of these groups is supported by their relatively low demand on capital resources.

As an extension of these notions, Fuchs associates the dominance of realist ideologies with communities that display high mutual dependence and low task uncertainty. On the other hand, “[r]elativism, or the absence of secure epistemic foundations, is the ideology of highly fragmented and individualistic social groups with low internal solidarity and cohesion” (1992:101).

Similarly, Fuchs extends the application of the grid-group scheme to examine the coordination and control of scientific work, the stratification system in science, and how scientific change takes place.

In a final section, Fuchs explores the relationship between sociology and the physical sciences, with an emphasis on the methodological status of the interpretive paradigm in sociology. Fuchs supports a hermeneutics,³ or method of understanding human life-worlds, which he sees as “practical, participatory, and lay-oriented” (1992:201), and which is not methodologically inferior to more so-called “scientific” methods of research.

Hermeneutics does not relate to positivism as fiction relates to truth, or as stories relate to science, but like uncertainty and organizational openness relate to routinization and organizational closure. (1992:202)

In other words, the interpretive method emerges out of the organizational structure of sociology and the nature of the object which sociologists study, namely, society.

Fuchs does not clearly indicate whether this kind of hermeneutic research is to be carried out by sociologists as they study science, or whether the results of such a research effort could be interpreted within the analytical scheme he presents. In other words, he is unclear whether the interpretive methods that characterize sociology are appropriate for the study of science. Similarly, even if science was approached hermeneutically, it is unclear what he thinks such research might tell us. Instead, Fuchs leaves us to contemplate whether we, as sociologists, are willing to accept the kinds of organizational changes (i.e., increased rigidity and formality) that would accompany the shift to a more positivistic methodology. For Fuchs, “the conversational life-form is more pleasant than a bureaucratic one” (1992:215). At this point, then, our task becomes one of determining in what ways Fuchs’ project can be considered a social study of science.

With respect to the first criterion, Fuchs’ more general social theory of science provides scholars with a framework within which to offer explanations that would account for characteristics of the scientific enterprise as a whole. At the same time, however, the analytical scheme

³ The term hermeneutics, which basically means interpretation, has a long and complex history in the social sciences and humanities (see Prus, 1996:34–46).

allows for the comparison of various “networks” of scientists that may not share the same degree of mutual dependence and task uncertainty. As for the second criterion, it is difficult to see how research on practicing scientists would fit into Fuchs’ system. In terms of the present project, the importance of observing the structurally determined path taken by a group of scientists, as they compete for resources, is to see how the scientists respond to the circumstances they encounter on a daily basis.

While Fuchs accepts that scientists are in the knowledge business, his criterion for examining truth has shifted from a philosophical one to a sociological one. In other words, truth no longer represents some absolute insight into a “real” world. Rather, it represents whatever the group of scientists commanding the greatest resources determines it to be. Thus Fuchs’ concept of truth is both pragmatic and social. Within the context of the organizational focus of his analysis, however, the goal of scientific activity appears to be structural in a somewhat abstract sense, rather than practical. That is, we would still like to know how particular scientists, working in particular situations and contexts, accomplish things on a daily basis.

The Mangle of Practice

In a review of the reciprocal relationship between historical and sociological efforts to study science, Jan Golinski (1990:500) argues that more effort must be expended to uncover “ways of explaining the actions of scientists in particular contexts,” by

developing a “theory of practice,” which would enable scientific work to be related to the aims and resources of its practitioners and to the structure of constraints within which they find themselves.

The individual whose work appears to most consistently reflect this emphasis is Andrew Pickering (1990, 1992, 1993, 1995), who advocates a shift towards conceiving science as “practice and culture.”

In *The Mangle of Practice* (1995), Pickering differentiates between ideas of “practice,” in a more comprehensive or generic sense, and specific “practices,” in the more limited sense of particular activities that scientists may carry out on a day-to-day basis. Specific practices (e.g., laboratory techniques) are part of the resources available to scientists, while Pickering sees the

broader concept of “practice as the work of cultural extension and transformation in time” (1992:4). This distinction is related to his broad conception of “culture” as the “made things” of science, which include skills, relationships, machines, instruments, facts and theories. In practice, scientific practices change, and along with them the whole culture of science.

Pickering's overall project is to emphasize two major themes in the social study of science: temporal emergence and post-humanism. The first of these refers to an interest in achieving “a *real-time* understanding” of scientific practice (1995:3), while the second concept concerns the displacement of the emphasis on human agency that was typical of constructivist science studies in the 1970s and 1980s. What is substituted for human agency is the recognition of “material agents” as developed in the actor-network theory of Bruno Latour (1987). By emphasizing these two elements, research in the social study of science can focus on the “mangle” of practice, which Pickering (1995:22) defines as the “dance of agency” that “takes the form of a dialectic of resistance and accomodation,”

where resistance denotes the failure to achieve an intended capture of agency in practice, and accomodation an active human strategy of response to resistance, which can include revisions to goals and intentions as well as to the material form of the machine in question and to the human frame of gestures and social relations that surround it. (1995:22)

As will become clearer in the next chapter, this stance is similar to symbolic interactionism's emphasis on the intersubjective accomplishment of everyday life, but at this point, it will be useful to take a closer look at some aspects of the work of Bruno Latour.

In *Science in Action* (1987), Latour presents a series of reconceptualizations, through which he attempts to alter the course of science studies. One of his key ideas is to dismantle the differentiation between science and technology, as he sees this distinction as having been constructed by scientists themselves, in an effort to establish and maintain a boundary between the purer and more noble pursuits of science and the hands-on nuts and bolts of applied technology and engineering.

I will use the word **technoscience** from now on, to describe all the elements tied to the scientific contents no matter how dirty, unexpected or foreign they seem, and the expression ‘**science and technology**’ in quotation marks, to designate *what is kept of technoscience* once all of the trials of responsibility have been settled. (Latour, 1987:174, emphasis in the original)

The use of this distinction is seen as another weapon in the scientists' arsenal, as they accumulate credit and resources. While a number of scholars, including Pickering, have applauded Latour's collapsing of these two disciplines (see Fuchs, 1992; Giere, 1993; Jasanoff et al., 1995), I would contend that as long as the participants in these disciplines see such a distinction, then, as social researchers, we are obliged to respect that stance. To observe an intricate relationship between the activities of scientists and technologists is quite different from saying that they are the same.⁴

Another important concept is that of the "actor-network," which Latour uses to describe the way in which collections of scientists, and those outside of science with whom they interact, are ordered. However, these networks are not simply a kind of diagrammatic representation of relationships, designed to demonstrate lines of communication and academic heritage.⁵ Rather, a better analogy is that of an electric circuit, where power is transmitted from place to place to keep things going. If a break occurs in the circuit, the power ceases to flow, the lights go out, and everything grinds to a halt.

The word network indicates that resources are concentrated in a few places – the knots and the nodes – which are connected with one another – the links and the mesh: these connections transform the scattered resources into a net that may seem to extend everywhere. (Latour, 1987:180)

For Latour, the network extends to every member of the public, as we are all "made to give a hand" in support of the scientific enterprise.

[W]hen scientists appear to be fully independent, surrounded only by colleagues obsessively thinking about their science, it means they are fully dependent, aligned with the interests of many more people; conversely, when they are really independent they do not get the resources with which to equip the laboratory, to earn a living or to recruit another colleague who could understand what they are doing. (Latour, 1987:158)

This seeming paradox implies that scientists appear independent only when they are not, and that the more independent they actually are, the more obvious the consequences of the dependency relationship become.

⁴ In my opinion, to conflate science and technology is similar to conflating theology and ministry. Regardless of my opinion, though, the people I interviewed for this project identified themselves, and were identified by others, as scientists, not as technologists or engineers.

⁵ This notion is more consistent with the Mertonian inspired research of Crane (1972) and Mullins (1973).

In some ways, the name “actor-network” theory is a misnomer, for Latour replaces the concept of “actor,” with that of “actant,” meaning “whoever and whatever is represented” (1987:84). Not only are we to dismantle the distinction between science and technology, but we are to move past any distinction between human and non-human agency. All things, animate and inanimate, are part of the network that supports the scientific enterprise, and therefore all things can be treated in the same manner by anyone studying science.⁶

While Pickering (1995:11) applauds the “symmetry” between human and material agents advocated by actor-network theory, he also argues that it is difficult to conceive of machines displaying “intentionality,” that is, as “*having specific plans and goals*” (1995:17, emphasis in original). In other words, while “[s]emiotically, these things can be made equivalent; in practice they are not” (1995:15).⁷

The key to understanding how Pickering wishes to use these ideas is to return to Fleck’s notions of passive and active elements.

As active, intentional beings, scientists tentatively construct some new machine. They then adopt a passive role, monitoring the performance of the machine to see whatever capture of material agency it might effect. Symmetrically, this period of human passivity is the period in which material agency actively manifests itself. (Pickering, 1995:21-22)

During this process, problems will arise and adjustments will be made. This temporally emergent “dialectic of accommodation and resistance” is what scientific practice is all about and the “stuff” of which scientific culture is constructed.

Pickering (1995:213) advocates dropping the “multidisciplinary eclecticism” (see Giere, 1993) typical of the present state of science studies, in favor of an “antidisciplinary synthesis,” in which the “microdisciplinary fractures in and around science studies are more or less erased” (1995:216). This approach would recognize both the epistemic and social elements of science, and it would show an appreciation of the “cultural multiplicity” of science. Pickering (1995:220-

⁶ For a systematic critique of Latour’s ideas, see Amsterdamska (1990).

⁷ A recent theoretical analysis of Pickering’s seemingly inconsistent application of the concept of non-human agency (Jones, 1996) can be read to support the symbolic interactionist approach to science taken in the present study.

221) proposes that research in the "cultural" study of science would be carried out along three lines: an historicizing approach, a disciplinary approach, and a practical approach.

The historicizing approach starts with the standard vocabulary and concerns of the philosophy of science and adds to them an element of historical grounding. The idea here is to take a concept such as "objectivity," and demonstrate its contextual and situational variability. The second approach is based on Michel Foucault's (1979) interest in how specific disciplines and practices characterize and are constitutive of specific cultures. The objective here is to demonstrate the heterogeneity and stability of scientific "cultures" (e.g., Traweek's study of the high energy physics community). The third approach is based on Latour's (1987) advice to "follow scientists and engineers around." Studies of this kind would provide data on the diversity of human and material resources that constitute a particular scientific culture, along with an examination of the internal and external relationships and influences affecting the scientific enterprise. However, this kind of study is to be carried out "not through time but in cultural space" (Pickering, 1995:221). In other words, this is not to be an historical approach, but rather a study of "science in action."

More detail of Pickering's discussion on machines and material agents will be provided in Chapter 8 on managing equipment, however, his emphasis on temporal emergence and studying "science-as-practiced" is generally consistent with the symbolic interactionist (SI) approach advocated in this project, and as he (1995:63) notes, SI differs from his approach "only" in its "human-centeredness." In other words, SI does not make the transition to the kind of post-humanist studies envisioned by Pickering, that would allow for some element of non-human agency. Needless to say this is a critical difference, and, as will become clear in the next chapter, the interactionist emphasis on intersubjective accomplishment is at the heart of the present project. Before turning to the final work to be examined in this chapter, we must determine to what extent Pickering's project meets the three criteria for a social study of science.

Not only does Pickering advocate the examination of particular groups of scientists and gaining a real-time appreciation of scientific practice, he also puts aside any attempt to follow a research agenda set by philosophers. Where the problem arises, though, with respect to the

interactionist approach advocated in the present study, is in Pickering's alteration of the concept of the social to include non-human actors. Only humans engage in processes of symbolic interaction and interpretation. Material objects are important because humans attribute meanings to them, as they carry out their daily activities.

The Work of Science

In their ethnographic study of laboratory technicians (1994), Stephen Barley and Beth Bechky criticize much of the sociology of science literature for following a pattern more appropriate to the sociology of knowledge rather than one more suited to the sociology of work (see Fuchs, 1992). These authors suggest a shift away from epistemic issues to a closer examination of how lab workers experience their practice, and how labor is divided in the laboratory setting. Informed by the work and occupations literature, the authors are concerned primarily with elaborating the status, role and skills of technicians, who "work at the empirical interface between a physical or biological system and the symbolic world of the scientist's work" (1994:89). In conjunction with this, the authors provide a description of the work carried out by technicians at this interface.

The two processes that Barley and Bechky (1994:89) identify as taking place at the interface between the phenomenon under study and the more theoretical world of the scientists are "translating" and "caretaking." The process of translation is composed of those activities that provide scientists with data in a form that can be analyzed, such as producing the "inscriptions" from which scientists construct arguments (see Latour and Woolgar, 1986). Caretaking refers to the process of caring for the physical entities that the technicians oversee. In other words, "technicians are usually charged with ensuring that machines, organisms, and other physical systems remain in good working order" (1994:89).

Barley and Bechky (1994:91) contend that a large part of the technicians' work, both with respect to translating and caretaking, centers around "troubleshooting," and the monitoring and repairing of "key technical interfaces" that are essential in the carrying out of scientific research. On the basis of data obtained through observation and interviews, the authors construct a

structural matrix of technicians' theories of trouble that identifies: sources of trouble, types of trouble and strategies for either averting or resolving trouble. The majority of the elements of this matrix are expressed in processual terms reflecting the practice of the technicians. For example, trouble is avoided through processes of: documenting procedures, ensuring redundancy, keeping things clean, and learning from past experiences. If troubles do arise, then they can be dealt with through processes of recovering, which often involves retracing steps and starting over, fixing, where technicians "break a machine down and put it back together," and discovering, which involves interpreting an anomaly in a positive light. Not only does the examination of these routine activities help us to understand some of what Thomas Kuhn meant by his categorization of normal science as puzzle-solving, but it demonstrates the value of examining the scientific enterprise as a practical realm.

One of the strengths of this work is that it draws attention to aspects of the scientific enterprise that have largely been ignored by the field work carried out in the sociology of science. As Barley and Bechky (1994:86) observe, "[b]ecause ethnographies traditionally portray the round of life in a social setting, one would expect technicians to have appeared prominently in ethnographies of science labs; yet they have not." Additionally, the insights provided by Barley and Bechky support the notion that it is not necessary to approach the study of the scientific enterprise from within the context of the history and philosophy of science, or even within the context of the sociology of science, narrowly defined. Rather, science can be approached from a variety of sociological perspectives, particularly drawing on the insights from research in work and occupations.

One final point to emphasize is that Barley and Bechky examined twelve technicians in ten different settings in an attempt to draw attention to the "commonalities" in technicians' work. Of the two main labs studied, one of them was involved in producing a product (monoclonal antibodies) for use in other labs, and the other was involved in providing an analytical service (flow cytometry) to other labs. This kind of transsituational and transcontextual examination is consistent with the approach advocated in the present project.

With respect to the three criteria of a social study of science, Barley and Bechky's work comes very close to meeting all three. Their research looks at scientific activity being carried out in particular settings and contexts, and they do not filter their analysis through some *a priori* philosophical objectives or criteria. With respect to the second criterion, the research subjects in this case are technicians working for scientists. A social study of science would extend this project to include the scientists themselves. As for the third criterion, Barley and Bechky looked directly at the mundane, routine day-to-day activities that technicians carried out, to highlight the fact that technicians constructed and employed a body of knowledge based on "a *contextual understanding* of materials, instruments, and techniques that was grounded in hands-on experience" (1994:116, emphasis in original).

Summary

The objective of this chapter has been to examine a number of works in the social study of science, broadly defined, to determine if they match the three criteria set out as appropriate for a proper social study of science and, in this process, to show how they conceive of science and how they propose to study science. The particular works covered here were selected not only because they represent the broad array of recent initiatives in science studies, but because they draw attention to some aspect of studying science that will be incorporated into the interactionist approach to science advocated in the present project.

While Star's work is based on symbolic interactionism, and more particularly the interpretation of SI supported by the Tremont researchers, namely, the social worlds perspective, her approach does not appear to support the kind of emphasis on examining the practical accomplishment of everyday life in the here and now that is an integral part of the objectives of the present project. However, Star does draw attention to the fact that scientists are likely to deal with a variety of uncertainties and ambiguities on a regular basis, as they attempt to accomplish their goals.

The primary benefit of Fuchs' work is the emphasis that he places on the generic aspect of human activity, and how the ways in which we analyze what goes on in science can also be

applied to other fields of human endeavor. Also, the importance of considering how structural constraints can shape the path of scientific research is highlighted. Fuchs offers an explanatory framework within which the success of scientists, as compared to members of other human groups, can be analyzed and understood. The problem is determining how science should be studied, in order to gain insight into how these elements play out on a daily basis in scientific practice.

Andrew Pickering's emphasis on the culture and practice of science is very consistent with the approach advocated in the present project, but one major divergence emerges. Pickering chooses to sustain Latour's conflation of science and technology, and of human and non-human actors. I would contend that neither of these simplifications is consistent with an approach to science that respects the nature of the phenomenon being studied. As I think will become clear in the next several chapters, Pickering's conception of scientific practice as the dialectic of resistance and accommodation can find a more coherent and sustainable expression within a symbolic interactionist framework.

The great advantage of Barley and Bechky's study is that they followed in the footsteps of the original "laboratory studies," and entered the working environments of scientists. Even though they limited their observations to the work of technicians, we gain insight into the daily activities of scientific practice, in a way that none of the other studies examined here was able to accomplish.

The goal now is to present an approach to the social study of science that meets the criteria set out at the beginning of this chapter, and which offers both a feasible definition of science and a way for researchers to study that science. As was stated earlier, I contend that a symbolic interactionist approach is the best suited to these objectives. The next chapter provides a description of just such an approach.

Chapter Five

AN INTERACTIONIST APPROACH TO SCIENCE

The purpose of the present chapter is to address directly the first goal of this project by outlining just what a symbolic interactionist approach to the study of the scientific enterprise would look like. Part of this process will be to supply a working definition of science and provide a way for researchers to carry out social studies of science. At the same time, it must be clear how this approach meets the criteria of viewing science as situational and contextual, of involving practicing scientists as research subjects, and of following a sociological agenda that recognizes the possibility that truth is not the only goal of scientific activity.

As mentioned earlier, for the purposes of the present project, science is defined as what scientists do. The material covered in the last three chapters has demonstrated a vast array of conceptions of, and approaches to, the scientific enterprise, that may or may not be complementary. While I have attempted to show how most of these approaches fail to achieve intimate familiarity (Blumer, 1969) with their subject matter, it is important to state at this point that the approach to science advocated here is in no way being presented as the only way to study science. Rather, it is being presented as the best way to gain insight into what scientists do on a day-to-day basis. Not only will this approach help scholars to better understand science, it should also help us to gain a better understanding of the broader nature of human group life.

The chapter is divided into three sections. The first section is devoted to an examination of the symbolic interactionist perspective within sociology in general terms.¹ This perspective provides both a theoretical and methodological framework that are consistent with the three criteria established for the approach to social studies of science advocated here. The second section introduces a generic social process scheme that provides a method for carrying out such research. This particular scheme was selected for its broad applicability, and the way it

¹ Several introductions to symbolic interactionism are available: Mead (1934), Rose (1962), Blumer (1969), Meltzer et al. (1975), Rock (1979), Adler and Adler (1980), Charon (1989), Fine (1990, 1992, 1993), Strauss (1993), Prus (1996).

integrates theoretical and methodological concerns. The final section draws these various elements together, and presents the details of how the fieldwork portion of the present study was carried out.

Symbolic Interactionism

Symbolic interactionism is part of a broader category of interpretive and constructionist approaches within sociology. Related approaches include dramaturgical sociology (Goffman, 1959), and two variations on what may be termed phenomenological sociology,² namely, ethnomethodology (Garfinkel, 1967) and social constructionism (Berger and Luckmann, 1966). As Robert Prus (1996:34-46) indicates, some of the early precursors of the interpretive tradition can be found in the hermeneutics of Wilhelm Dilthey (see Ermarth, 1978), Georg Simmel's (1950) conceptualization of forms of human association, Max Weber's (1947) emphasis on understanding social action, and Wilhelm Wundt's (1916) discussion of folk psychology. In a more direct manner, however, the two principal roots of symbolic interactionism are to be found in American pragmatism and the urban ethnography of the so-called "Chicago" school of sociology.

Pragmatism refers to a philosophical movement associated with Charles Peirce, James Dewey, and William James (see James, 1907; Shalin, 1986). The pragmatists attempted to develop theories that recognized the more practical elements of human accomplishment. As Charon (1989:27) indicates, pragmatism stressed four basic ideas:

1. Truth is possible for the human being only through the individual's own intervention.
2. Knowledge for the human being is judged on its usefulness.
3. Objects we encounter are defined according to their use for us.
4. Understanding about the human being must be inferred from what he or she does.

The principal idea advocated here is that things "do not tell us what they are," we must interpret them. As practical active human beings, we test what we know and if it works for us we keep it, if not, we discard it. Any particular object that we encounter in our lives may have a multitude of

² Phenomenology is associated with the work of Edmund Husserl (1970) and Alfred Schutz (1967, 1972).

meanings, and therefore, can be defined in many ways. Consistent with these notions is the precept that our understanding of people must arise out of the empirical observation of human action. The pragmatist perspective seems to have had a particular resonance for Americans in the early part of this century, as it was consistent with the "hands-on," "show-me" approach of a nation of "doers." As mentioned in Chapter 2, some philosophers are advocating a return to the more practical emphasis of the pragmatist perspective, as an antidote to abstract theorizing and debate about the nature of science.

The pragmatist approach found particular expression within sociology in the work of Charles Horton Cooley at the University of Chicago. Cooley was especially interested in the importance of human groups, and in the development of a concept of self, which he says arises out of our perceptions of how other people see us (1922:184). Along with this notion of the "looking-glass self," Cooley also proposes a general methodology, which he calls "sympathetic introspection" (1909:7). The idea here is for researchers to establish "intimate contact with various sorts of persons," in order to understand their world on their terms. As Prus (1996:51) indicates, Cooley's method is at the base of what we would now call participant observation, naturalistic inquiry, or ethnography.

As mentioned earlier, symbolic interactionism relies centrally on the ethnographic method of carrying out research, particularly as it was developed at the University of Chicago in the early part of this century. Thus, it will be valuable to look briefly at the history and development of ethnography, and the first step in this process will be to offer at least a preliminary definition of what ethnography is. A more systematic examination of the practice of ethnographic research is included in the final section of this chapter.

In their historical survey of qualitative research methods, Vidich and Lyman (1994:25) define ethnography as "a social scientific description of a people and the cultural basis for their peoplehood." Hammersley and Atkinson (1995) express a reluctance to draw too sharp of a distinction between ethnography and other qualitative methods, emphasizing that in its most basic form, ethnography "bears a close resemblance to the routine ways in which people make sense of the world in everyday life" (1995:2). Prus (1996:18-21) associates ethnographic

research with the processes of observation, participant observation, and interviewing, indicating that “[e]thnographic inquiry requires that researchers pursue and present the viewpoints of those with whom they have contact” (1996:103).

Popular notions of ethnographic research may be most closely associated with the studies of “primitive” or “exotic” cultures, in the tradition of Bronislaw Malinowski and A.R. Radcliffe-Brown that receive considerable media attention. The development of ethnography within sociology, however, is primarily associated with the University of Chicago. Here, scholars were concerned with a broad range of issues that included moral reform, and understanding the urban ecology of the environment in which they found themselves. Ethnography was one of many methods employed to gain a better understanding of what was going on in the city, and to help with the kind of conceptual development that would support a more practical emphasis on human group life.

The practical orientation of Chicago sociology is particularly evident in the so-called “student ethnographies” that were carried out in the 1920s and 30s. Robert Park and Ernest Burgess, even though they did not venture into the field themselves, encouraged their students to view the city as a sociological laboratory (see Kurtz, 1984:60).

Go and sit in the lounges of the luxury hotels and on the doorsteps of the flophouses; sit on the Gold Coast settees and the slum shakedown; sit in the orchestra hall and in the Star and Garter burlesque. In short, gentlemen, go and get the seat of your pants dirty in real research. (Park, cited in Prus, 1996:119)

Some examples of these studies are: Nels Anderson's *The Hobo* (1923), Paul Cressey's *The Taxi-Dance Hall* (1932), Clifford Shaw's *The Jack-Roller* (1930), Edwin Sutherland's *The Professional Thief* (1937). These early efforts demonstrate both the evolution of ethnographic fieldwork in sociology and the kind of “learning by doing” that is consistent with symbolic interactionism (see Prus, 1996:119-124).

In later efforts, this approach found particular application in studies of deviant subcultures and other “closed” groups, of interest to sociologists, that might otherwise be difficult to study. Two examples that stand out in this regard are Erving Goffman's *Asylums* (1961), which examines the “careers” of mental patients in the environment of a so-called “total

institution,” and Howard Becker’s *Outsiders* (1963), which examines, amongst other things, the “subculture” of jazz musicians who labeled themselves as “outsiders.” Further examples include studies of: outlaw biker gangs (Wolf, 1991), religious cults (Lofland, 1966; Barker, 1984), fantasy role-playing games (Fine, 1983), stand-up comedians (Stebbins, 1990), the professional development of medical students (Haas and Shaffir, 1987), drug dealers (Adler, 1985), college athletes (Adler and Adler, 1991), student life (Albas and Albas, 1984), and tattooing (Sanders, 1991). While each of these studies focuses on a particular subculture within modern society, they are well suited to comparison and broader conceptual development. Insights gained in one area can readily be adapted to helping in the exploration of some other aspect of human group life.

Leaving aside the ethnographic research tradition, for the time, perhaps the central figure in the early development of symbolic interactionism is George Herbert Mead, who taught philosophy at Chicago. Three aspects of his thought are particularly important for understanding symbolic interactionism, namely, his ideas on the mind, the self, and society. For Mead, the mind is in no way a static or predetermined entity. Rather, the mind emerges as an internalization of the interaction that one has with other members of the community (1934:49). A shared system of symbols, and particularly language, form the medium for the development of mind, and also provide a way for the individual to engage in “minded behavior,” through which viewpoints are expressed and actions are initiated (1934:132). As Prus (1996:53) indicates, communication, conceptualization, and reflection are all symbolic processes.

For Mead, thinking is a reflective process, which means that the individual also carries on symbolic exchanges internally, and thereby establishes an identity or conception of self. This internal process, however, is not done in isolation from other people.

The organized community or social group that gives the individual his unity of self may be called “the generalized other.” The attitude of the generalized other is the attitude of the whole community... (Mead, 1934:154)

In other words, the reflective process arises when people take the role of the other, or begin to adopt the attitudes and actions of others. The behavior of the individual in any situation

represents the culmination of a number of interpretive and interactive processes. In this way, the concepts of mind, self, and society can be seen to be intertwined and interdependent.

Human society as we know it could not exist without minds and selves, since all its most characteristic features presuppose the possession of minds and selves by its individual members; but its individual members would not possess minds and selves if these had not arisen within or emerged out of the human social process in its lower stages of development. (Mead, 1934:227)

So, we can see in Mead's work the kind of emphasis on symbols and social interaction that we would now associate with symbolic interactionism. Of even more direct consequence to the present project are Mead's ideas on social action.

In his analysis of social action, Mead (1934) draws attention to four stages of the act: impulse, perception, manipulation, and consummation. While symbolic interactionists try to avoid talking about the causes of human action, the concept of "impulse" is based on the notion that humans have a "generalized disposition to act." There is always a problem to be solved, a goal to be obtained, or some obstacle to be overcome in the environment in which people live. The second stage of the act implies that humans interpret, define, or analyze the situations in which they find themselves. They do not merely react in some unthinking or non-reflective manner. Humans also manipulate their environment through direct contact with physical objects. In this way, goals are achieved and life is accomplished through practical activity. The final stage of "consummation" might be interpreted to imply that action comes to an end. However, while this may be the case in some limited sense, the potential for other outcomes also exists; goals may not be accomplished or one course of action may lead to another. In other words, the stream of human action is continuous. Consummation or accomplishment might more significantly represent continuity, or living to fight another day.

Mead's handling of these concepts is neither systematically nor extensively developed, but Herbert Blumer offers this synopsis of what Mead had in mind.

Action is built up in coping with the world instead of merely being released from a pre-existing psychological structure by factors playing on that structure. By making indications to himself and by interpreting what he indicates, the human being has to forge or piece together a line of action. In order to act the individual has to identify what he wants, establish an objective or goal, map out a prospective line of behavior, note and interpret the actions of others, size up his situation, check himself at this or that point, figure out

what to do at other points, and frequently spur himself on in the face of dragging dispositions or discouraging settings. (Blumer, 1969:64)

At the same time, participants in any particular life-world, in an effort to make their life more manageable, also tend to conceptualize the stream of action as being comprised of discrete acts. Charles Warriner (1970:17-18) indicates that an act

is recognized by the members of society as a unit act with a particular meaning... The "sawing of a board" by a carpenter has primary significance for sociological purposes, not because it involves certain muscles and nerves, not because the carpenter has particular motivations, not even because the board gets sawed, but because the act is given a name, conceptually separated from the other parts or aspects of the stream of action... "Thumbing one's nose," "punishing the child," "giving a speech," "going to church," "riding a bus," "going to class"...are all... acts because they are named and are identified by the conventional understandings of the members of the society.

In other words, these discrete components of action, which are interpreted by members of a particular group, serve as meaningful symbols to facilitate social interaction. To understand these actions is to understand the group that carries out these actions.

Herbert Blumer

Herbert Blumer coined the term symbolic interactionism in 1937, and it is Blumer who contributes most to the systematization of the theoretical and methodological aspects of this approach. Prus (1996:74) indicates that Blumer made three major contributions to the social sciences. First, he provided a "sociologically focused statement" of the interpretive paradigm. Second, he identified and addressed "the central weakness of mainstream social science," and third, he provided a "conceptual framework" for ethnographic research in the social sciences.

With respect to the first point, Blumer offers this concise statement of the major premises of the symbolic interactionist perspective.

1. [H]uman beings act toward things on the basis of the meaning that the things have for them.
2. [T]he meaning of such things is derived from, or arises out of, the social interaction that one has with one's fellows.
3. [T]hese meanings are handled in and modified through an interpretive process. (Blumer, 1969:2)

In other words, our understanding of human group life emerges out of our understanding of the meanings that people attach to the objects (animate and inanimate) they encounter in their daily

lives. These meanings are seen to emerge out of the interactive and interpretive processes that people engage in on an ongoing basis. While these statements can be seen to reflect Mead's emphasis on the processual and emergent nature of mind, self, and society, Mead never offered his ideas in a way that could be effectively communicated to others, or tested in the field.³

Blumer accomplishes this task by combining elements of pragmatism with the kind of "hands-on" research being done at Chicago.

As for the second contribution, Blumer is critical of positivist and structuralist approaches in sociology for, in his view, failing to "[r]espect the nature of the empirical world and organize a methodological stance to reflect that respect" (1969:60). For example, Blumer (1969:141-142) is critical of much of social theory for divorcing itself from the empirical world, providing little guidance for field research, and failing to benefit from the vast accumulation of "facts" emerging from empirical research. Contrary to his contention that the value of social theory depends on its connection to the empirical world, Blumer (1969:142) charges that theorists expend too much effort "taking in each other's washing," and "merely interpreting things to fit their theories."

Similarly, Blumer (1969:127-139) is critical of "variable analysis" for missing the process of interpretation that lies between any independent and dependent variable. These variables "conceal or misrepresent the actual operations in human group life" (1969:136). Rather than being reducible to discrete and unitary items (variables), relations in group life are more likely to take place between "complex, diversified and moving bodies of activity" (1969:138). What Blumer (1969:46) advocates is "naturalistic" investigation --

investigation that is directed to a given empirical world in its natural, ongoing character instead of to a simulation of such a world, or to an abstraction from it (as in the case of laboratory experimentation), or to a substitute for the world in the form of a preset image of it.

This statement not only contains a harsh criticism of various "artificial" methods that may be associated with more positivistic or quantitative approaches to social science, it also contains an implicit advocacy of ethnographic enquiry. Ethnography is "naturalistic" because it involves the

³ In fact, the vast majority of what we consider Mead's work has been (re)constructed by his students. In the specific case of *Mind, Self & Society* (1934), the text consists of lecture notes recorded by a stenographer hired by Mead's students.

very thing that it purports to examine, that is, social interaction. In other words, ethnographic research is essentially self-exemplifying; it is symbolic interaction.

Blumer's third contribution has to do with the matter of researchers adjusting and refining their preconceptions as they encounter various phenomena in the field. Blumer advocates the use of "sensitizing concepts," which permit "one to catch and hold some content of experience and make common property of it" (1969:158). Sensitizing concepts, which "merely suggest directions along which to look," are contrasted to "definitive concepts," which "provide prescriptions of what to see" (1969:148). In this way, insights from a variety of settings can be compared and contrasted along the way to a greater overall understanding of human group life.

For Blumer, the use of concepts is an integral part of doing science, such that a "science without concepts" (1969:153) is an impossibility.

The scientific concept, as a way of conceiving, enables one to circumvent problems of perceptual experience; the content of the scientific concept consists of an abstracted relation which becomes the subject of separate and intensive study; the concept because of its verbal character, may be shared, and so it permits concerted activity in scientific procedure; scientific concepts in their interrelation make possible the structure of science. (Blumer, 1969:163)

That is, the concept allows scientists to express points of view, it provides them with a tool for thought (reflection) and communication, and it can provide the basis for deductive reasoning and the anticipation of new experience (1969:163). At the same time, the concept is not a mere abstraction, for "the improper usage of the concept in science comes when the concept is set apart from the world of experience" (1969:168). Symbolic interactionism, then, is the study of human group life and, through the use of concepts, this approach can be distinguished from others which may rely on a variety of theoretical or methodological abstractions to separate analysis from human experience.

Let us recognize the instrumental character of the concept inside of the field of science. By accepting it in this character and using it critically perhaps we can avoid being mere bookkeepers of facts or spinners of metaphysics. (1969:170)

This last point is pivotal for understanding the "generic social process" scheme that will be discussed in the next section, and for understanding how field research in symbolic interactionism can be carried out.

Generic Social Processes

Building on the works of Simmel, Blumer, and others,⁴ Robert Prus (1987, 1996) presents a "generic social process" scheme, both as a way to maximize conceptual development in sociological research and as a way to share the insights contained in the growing number of ethnographic studies of diverse settings. Prus (1996:22) argues that

the study of human behavior requires the blending of (a) an interpretive viewpoint with (b) the practice of ethnographic enquiry, (c) a comparative-adjustive style of analysis, and (d) an ongoing appraisal and adjustment of existing theoretical and methodological positions to actual studies of experiences and practices of the (ethnographic) other.

Advocating a symbolic interactionist approach to the study of human group life, Prus places a strong emphasis on applying this approach in the field and using the results of such research to broaden our understanding of human behavior. The interpretive process to which Prus refers reflects the fundamental social interaction that undergirds the life-world of those that we study.

Prus (1996:149-150) indicates that the generic social process scheme consists of six basic processes that are fundamental to people's interactions and to the communities that people form.⁵ They are:

Acquiring Perspectives

Achieving Identity

Being Involved

Doing Activity

Experiencing Relationships

Forming and Coordinating Associations

The remainder of this section is devoted to describing these concepts in greater detail, to show how they combine the theoretical and methodological emphases of symbolic interactionism and

⁴ See, especially, Miyamoto (1959), Glaser and Strauss (1967), Lofland (1976), Lester and Hadden (1980), Bigus, Hadden and Glaser (1982), Couch (1984).

⁵ Prus has carried out studies in a number of settings using this scheme, including: the hotel community (Prus and Irini, 1988), marketing and sales (Prus, 1989a, 1989b), and hustlers and magicians (Prus and Sharper, 1991).

ethnographic inquiry, and provide a practical device for delineating the processual character of everyday life.

Acquiring Perspectives

Perspectives refer to the group-informed viewpoints or collections of meanings that people invoke in dealing with the situation at hand. Group interaction and the contextualized use of language enable individuals to develop "stocks of knowledge" (Schutz, 1967) and an awareness of the "generalized other" (Mead, 1934). These community-based processes, which at some fundamental (symbolic) level necessarily precede the participation of individuals in particular situations, are built up, assessed and potentially resisted, as people encounter others and deal with particular situations.

Prus (1996:151) indicates that as newcomers to a group people have no choice over the perspectives to which they are first exposed, and they will tend to take these initial viewpoints for granted until they are confronted with a set of circumstances that challenges or threatens their ideas. Just as some situations will lead individuals to explore new interpretations of objects and other people, other events may serve to solidify and reinforce existing viewpoints. Alternate perspectives are evaluated on an ongoing basis, with some being enthusiastically accepted and others being rejected relative to some preliminary or pre-existing viewpoint. This process takes place with respect to religion, education, family, and the broader array of rules, norms, and values that guide action on a day-to-day basis.

Through ethnographic study, Prus (1996:151-152) indicates that researchers can gain insight into the ways in which people encounter perspectives from others, assess new perspectives and resist unwanted viewpoints, develop images of objects, define situations, deal with ambiguity, resolve contradictions, extend on existing perspectives, promote perspectives to others, reject former viewpoints, and adopt new viewpoints.

Achieving Identity

The process of "achieving identity" deals with the notion of defining the self and others in human community life (Prus, 1996:152-153). This "identity work" involves not only receiving definitions from others, but requires a role-taking capacity for self-reflection and the attribution of qualities to the self. This implies a sort of sifting process, whereby an individual compares, rejects, and ultimately selects some combination of attributes, that can then be selectively projected during interaction, to portray some specific identity or other.

This process encompasses what Erving Goffman (1959) refers to as "impression management." As individuals we are constantly engaged in a process of attributing identity to ourselves and others and assessing and refining the definitions that emerge. In other words, even when an individual has achieved a certain identity, a maintenance process is central to continued interaction. This involves constant checking and re-checking to ensure that appropriate impressions are being presented to and received by others. Any uncertainty over these matters can foster considerable anxiety in the individual.

The study of human groups can help us to understand better how people encounter definitions of self from others, attribute qualities to the self, compare incoming and self-assigned definitions of self, resist unwanted identity imputations, selectively convey information about the self to others, glean information about others, assign identities to others, promote specific definitions of others, encounter resistance from others, and reassess identities imputed to others (Prus, 1996:152-153).

Being Involved

The process of "being involved" draws our attention to the temporal aspect of social interaction, and can be divided into segments that reflect the sequence of stages that people pass through in their involvements in specific situations (Prus, 1996:153-155). The first stage of (i) initial involvement consists of examining various ways in which individuals become part of specific communities or subcultures. These processes commonly include seekership by the individual, recruitment by others, and closure (whereby an individual feels compelled to join).

These processes may be present in any combination, reflecting the complexity and diversity of involvements that an individual may participate in. The second stage of (ii) continued involvement can stretch out over a relatively long or short period, and hence will be linked to other social processes. For example, as people's involvement with a particular group continues, they may acquire different perspectives, their identities may evolve, they may engage in alternate activities, and their patterns of relationships may shift. These changes can lead to a third stage of (iii) disinvolvement that may reflect people's problems (and disaffections) within a particular situation. On the other hand, a shift in involvement patterns may reflect new opportunities that the individual perceives as challenging or potentially rewarding. Similarly, individuals may, at some later point, become (iv) reinvolved in a previous situation. This may reflect dissatisfaction with some present activity, or it may reflect changes that have taken place in the original group.

This pattern of activities can be viewed as a life history or "career" (Becker, 1963; Prus, 1984), and as such it provides an excellent framework within which to examine many aspects of everyday life. As Prus (1996:148-149) indicates, the career concept allows the researcher to take "a more holistic approach to the "life spans" of particular phenomena," that goes beyond the limited association of career with occupation. This ability to examine aspects of human group life along a timeline is consistent with the processual focus of symbolic interactionism.

Doing Activity

Within the process of "doing activity," Prus (1996:156) identifies three subprocesses, namely: (1) performing activities, (2) influencing others, and (3) making commitments. As with the process of continued involvement, some amount of overlap is inevitable in the application of these concepts to social settings. However, what differentiates these processes is the emphasis on the "problematics of accomplishment." In other words, people develop strategies for achieving their goals, and they find ways to proceed in the face of ambiguities, uncertainty, and resistance from others.

The subprocess of “performing activities” is integral to the present project, and will be outlined in greater detail in the final section of this chapter. The activities here can be viewed from the perspective of project management: plans are drawn up, preparations are made, skills are acquired, events are coordinated, resources are put into action, obstacles and setbacks are dealt with, and activities are assessed and adjusted.

Another aspect of carrying out activities involves “influencing others.” This subprocess reflects the efforts of individuals and groups to gain cooperation from others, in order to complete transactions or accomplish collective goals. Some of the activities involved here include: formulating plans, promoting interest in one’s objectives, generating trust, proposing specific lines of action, encountering resistance, and neutralizing obstacles.

The third subprocess of “making commitments” represents the decision making process of “buying into” a particular course of action. This step normally involves: exploring and assessing options, dealing with earlier commitments, avoiding commitments, minimizing or diversifying investments, organizing routines, and neglecting other options. In other words, the commitment to follow a specific course of action with a particular group of people means that resources cannot be allocated elsewhere as readily. Those involved may find themselves performing a balancing act between various obligations and desired sets of activities.

Experiencing Relationships

The fifth aspect of the generic social process scheme deals with people “experiencing relationships” which, as Prus (1996:159) indicates, is a vital part of social life associated with the selective formation of bonds between people. Some relationships are more intimate than others and a number of processes come into play as the dynamics of social distancing are played out. Individuals may prepare themselves for encounters, define themselves as available for association, define specific others as desirable associates, and learn to deal with varying levels of acceptance or rejection.

Another important subprocess is that of self-assessment and evaluation, through which people determine “goodness-of-fit” with others. People often juggle a number of relationships,

reflecting the multiple perspectives and identities that they may be managing in the various aspects of their lives. People develop different interactional styles, and as they develop certain understandings, preferences, and loyalties, they will also deal with matters of openness and secrecy. As circumstances change, some relationships will be strengthened, some severed, and perhaps some will be renewed.

Forming and Coordinating Associations

Prus identifies a sixth set of generic processes that provides a mechanism for examining the kinds of activities that take place within and among larger groups of individuals that may share common goals. The process of "forming and coordinating associations" is related to establishing relationships, but also takes into account the advantages of working with others to accomplish certain objectives. Associations can vary in size or duration, and may allow members to pursue activities that they would not be able to manage on their own. Prus (1996:160) identifies three subprocesses that can be used to study this aspect of human group life: establishing associations, objectifying associations, and encountering outsiders.

The kinds of activities pertinent to the subprocess of "establishing associations" include: determining the benefits of collective enterprise, defining membership criteria, positions, and responsibilities for potential team members, formalizing encounters among members, and dealing with internal challenges around conflicts of interest, or loss of interest (1996:161). The degree of formality and complexity of these processes will vary with the intended purpose for association and the duration of that association. This stage is linked to processes of acquiring perspectives and becoming involved.

In describing the second set of subprocesses, "objectifying associations," Prus (1996:161) links his ideas to those of Berger and Luckmann (1966), who describe the social construction of "plausibility structures" or a "nomos" within which people can carry out the normal activities that constitute their everyday lives without having to constantly redefine their world, or their place in it. Thus, this subprocess can be seen in a broader based context that demonstrates a reliance on intra-group and inter-group interaction. In other words, members

work at being "one of us," while outsiders identify members as "one of them." As Prus (1996:162) observes, insiders may develop a group identity, create identity markers, define exclusivity, and demarcate territories or jurisdictions, while outsiders may define a set of people as constituting a specific group within a community, associate specific names or markers with that group, discuss the group with members or groups in the community, make a concerted effort to investigate and possibly eliminate the group.

The concept of "encountering outsiders" reminds us that neither individuals nor groups operate in isolation from others, or from society as a whole. In different cases, outsiders may be customers or competitors, and the group's response to such encounters will reflect their assessment of potential benefit or harm from association with others. Researchers may observe how group members represent the group's interests, make contact with outsiders, define the theatre of operation, identify outsiders, or pursue associational objectives through the others (Prus, 1996:163).

Prus (1996:165) concludes that the generic social process scheme accomplishes six things: (1) it provides a consistent and coherent analytical framework for studying human group life; (2) it allows researchers to transcend diverse substantive areas without losing details that are specific to certain situations or contexts; (3) it allows scholars to engage in a sort of conceptual stock taking through which they can assess and refine their work; (4) it provides an effective and efficient means of communications among researchers with diverse interests; (5) it provides a mechanism for the re-orienting of the sociological enterprise in the direction of human lived experience; and, (6) it similarly provides a mechanism for the establishment of meaningful and sustained interdisciplinary linkages in the social sciences.

The Present Project

Examining the activities of scientists in the making, this project focuses on the process of "*doing activity*," and more particularly, the subprocess of "*performing activities*" with its emphasis on the "*problematics of accomplishment*." Thus, in many ways, the present project is an effort to take seriously Andrew Pickering's statement that: "The attempt to understand

scientific practice is interesting in its own right ...what scientists do is just as important as the knowledge they produce" (1992:6-7). While this argument may seem banal, mundane or inconsequential, it is actually at the heart of the matter. Historical, philosophical, and sociological studies of science have met with limited success because they have often failed to recognize the importance of the day-to-day practical accomplishment of the scientific enterprise.⁶

If we want to know what is "really" going on in science, then our notion of reality must reflect the world as known and engaged in by those we wish to study. In other words, what is perceived as real must reflect the "definition of the situation," as determined by the scientists who are active in that situation. This kind of "pragmatic realism" (see Pickering, 1989; Lenoir, 1992; Murphy, 1993; Solomon, 1995) is consistent with the position that: "If men define situations as real, they are real in their consequences" (Thomas and Thomas, 1928:572). Donald Ball (1972:63) elaborates on this notion, indicating that it is the

sum total of all recognized information, from the point of view of the actor, which is relevant to his locating himself and others, so that he can engage in self-determined lines of action and interaction.

So, reality is neither some external and objective *a priori* entity in which the individual finds him or herself, nor is it the historical record of some deterministic path that constrains future action. Rather, it is an intersubjective collection of those things that people consider important in the specific contexts and situations in which they find themselves.

Because the task of trying to present a comprehensive picture of all of the activities that constitute science is clearly beyond the scope of any one study, I decided to focus on one aspect of this enterprise, namely, "performing activity," a subprocess of "doing activity." Prus (1996:157) indicates that this process can be broken down into a number of even finer

⁶ Sharon Traweek indicates that a shift must be made towards the methodological preference, which she associates with traditional anthropology, for "taking the mundane seriously," and "attending to the profane activities of our subjects at least as much as the sacred rituals" (1992:444). It is not unreasonable to observe that the bulk of research in the "sociology of science" has equated knowledge production with the "sacred" aspect of science and has either ignored what else goes on, or the researchers have filtered their views of these other activities through the knowledge lens (see Bloor, 1976; Fuchs, 1992; Fuller, 1993; Barnes, Bloor, and Henry, 1996). Even Lynch's (1993) emphasis on practical accomplishment and Pickering's (1995) advocacy of studying the mangle of practice do not provide a mechanism for moving past epistemological considerations in order to get at "science in action."

subprocesses, such as: making plans, getting prepared, developing competence, coordinating events with others, and encountering competition. The problems and challenges that actors encounter on a daily basis arise out of the ambiguities, uncertainties, and resistances that people meet with as they attempt to engage in whatever activities constitute their life-world. Developing an understanding of intersubjective accomplishment in the scientific enterprise, then, requires an extended examination of the ways in which practitioners engage this subject matter on a here and now basis.

The choice of "*performing activities*" was made following a series of exploratory interviews and an examination of the existing "science studies" literature. Within the context of the scientific enterprise, the process of performing activities can be broken down into seven subprocesses:

- Getting Ideas
- Assessing Feasibility
- Getting Prepared
- Pursuing Funding
- Implementing Plans
- Obtaining Results
- Disseminating Results

Further inspection of the data reveals that each of these processes can in turn be expanded into a series of more specific subprocesses that allows the researcher to examine, in a more intimate fashion, the details of the day-to-day activity of scientists. The listing that follows illustrates, in a preliminary fashion, some of these elaborations. The process of elaboration is not an attempt to reduce the data to the "bare facts," but instead, represents an attempt to sort the complexity of human group life into more manageable and communicable pieces. The activities of scientists, or members of any other human group, are neither limited to nor constrained by this particular set of processes.

- Getting Ideas**
 - Monitoring other research
 - Attending conferences/seminars
 - Reviewing the literature
 - Conceptualizing projects

Assessing Feasibility

- Evaluating projects
- Determining levels of risk
- Encountering ambiguities
- Changing theoretical directions
- Changing technical/methodological directions

Getting Prepared

- Learning new technologies/techniques
- Making commitments
- Acquiring supplies
- Designing experiments
- Promoting projects
- Discarding projects
- Revitalizing projects
- Hedging bets

Pursuing Funding

- Identifying sources of funding
- Preparing grant applications
- Entering partnerships with government/industry/other faculty
- Providing a service to other researchers
- Marketing equipment or materials to other researchers
- Making adjustments to projects

Implementing Plans

- Performing experiments
- Juggling multiple projects
- Managing students
 - Acquiring/recruiting students
 - Teaching/training in the classroom/lab
 - Supervising programs of study
 - Selecting research projects for students
 - Dealing with personal issues
 - Influencing career decision
- Managing equipment
 - Selecting appropriate equipment
 - Acquiring equipment
 - Borrowing equipment from others
 - Purchasing ready-made/off-the-shelf
 - Designing and building from scratch
 - Operating equipment
 - Maintaining equipment
- Gathering data
- Collecting samples
- Performing tests

Obtaining Results

- Analyzing data
- Assessing progress
- Drawing conclusions
- Determining the next step

Disseminating Results

- Preparing articles for publication
- Presenting results at conferences

Releasing information to a limited audience
Releasing information informally
Presenting seminars

While Chapter 6 presents some data on a number of these processes, two areas have been selected for more in-depth treatment, namely, *pursuing funding* (Chapter 7) and *managing equipment* (Chapter 8). The selection of these two aspects is in one sense purely arbitrary, in that the examination of any combination of the processes listed above would offer a wealth of material and insight. Indeed, one of the essential points of this presentation is that any and all of these facets represent major sustained research projects in its own right. However, as discussed in the first chapter, there are a number of reasons for highlighting these two areas. First, funding and equipment related activities have been unduly neglected in the literature to date. Second, a focus on epistemological concerns has diverted effort away from fieldwork. Third, the activities engaged in with respect to funding and equipment are common to all scientific settings. Fourth, similar activities are carried out by members of other human groups.

Turning now to the practice of ethnography, details of the method can be presented by examining four distinct but highly interrelated areas: the setting or source of the data, gathering data, analyzing data, and presenting data. While there is a range of activities that characterize one or another of these steps, the method is far more holistic, in that it does not rely on the reflective capacity of the researcher to integrate the entire research process. Similarly, the aspects of research design, implementation, and evaluation are seen to take place along a continuum, rather than existing as discrete events. In this way the method is similar to everyday life, as it is experienced by the groups being studied and by the researchers themselves.

The following presentation integrates a more general coverage of the four aspects of the ethnographic research process, along with the specific details of how these steps were carried out within the context of the present project. In this way, it will be much easier to demonstrate how the symbolic interactionist approach meets the criteria for a genuine social study of science, by studying specific groups of scientists, involving practicing scientists as research subjects, and examining in detail the activities in which scientists engage.

Data Gathering

Ethnographers typically rely on three forms of gathering data, namely: observation, participant observation, or interviews (see Lofland and Lofland, 1984; Jorgensen, 1989; Denzin and Lincoln, 1994; Hammersley and Atkinson, 1995; Prus, 1996). While each of these three approaches has distinct features, most projects will involve some elements of all three. The decision of which form is the most appropriate will depend on a number of issues, including the make-up of the subject group and the kinds of activities that they are involved in.

Straight (non-interactive) *observation* places the researcher at the greatest distance from the members of the group being studied. That is, it minimizes the opportunity for interaction. Given the symbolic interactionist emphasis on processes of interaction and interpretation, this method may seem unsatisfactory, but there are occasions when this may be the only possible route to take. This may be the case in covert studies especially, where the researcher does not wish to be identified as such.

Observation, however, is an important part of all ethnographic research, through which the researcher is able to accumulate details of the surroundings, facial expressions, and a number of other visual and aural aspects of group activity, that may provide some clue to the meanings that people attach to others and to the objects they encounter. The examination of various documents or artifacts may also be a part of this procedure.

When researchers assume the role of *participant observer*, they engage, to some degree, in the activities and lifestyle of the subject group. The level of involvement, however, can depend on the nature of the group being studied, or may be related to the level of access or the specific nature of the role that the researcher can obtain. For example, in order to study the activities of an outlaw biker gang, Daniel Wolf (1991) purchased a Harley-Davidson motorcycle and became an accepted participant within his subject group. Bruno Latour (Latour and Woolgar, 1979) was given working space in a neuroendocrinology laboratory, and even participated in the performance of some of the experimental work. However, in contrast to Wolf's becoming a biker, Latour did not become a biologist in order to carry out his research. Claude Vincent (1990) spent time with police officers and was able to ride in the squad car and

observe several aspects of police action. He was not a police officer, nor was he in any way empowered to engage firsthand in any of the activities related to police work. At the same time, however, he was able to interact with the police officers on a daily basis, and was thereby able to obtain valuable data, unavailable to the mere observer or bystander.

The third method, *interviewing*, allows the researcher to interact in a more concentrated manner with individual members of the group (see Spradley, 1979; Measor, 1985). Through this intersubjective process, impressions can be checked out, and greater details on specific issues can be obtained. Consequently, interviewing in ethnography tends to be "open-ended" or "non-structured," as distinct from the more formally structured interviews that are used in survey research.

[T]here is a tendency among ethnographers to favor non-directive interviewing in which the interviewee is allowed to talk at length in his or her own terms, as opposed to more directive questioning. The aim here is to minimize, as far as possible, the influence of the researcher on what is said, and thus to facilitate the open expression of the informant's perspective on the world. (Hammersley and Atkinson, 1995:129)

At the same time, the researcher attempts to interview a sufficient number of subjects, in order that the variety of viewpoints and diversity of members in the group are fairly represented. This kind of judgment will be based on the researcher's level of familiarity with the nature of the group, and will depend on how much data the researcher has been able to accumulate.

One important consideration for the researcher is the interdependence of observation, participant observation, and interviewing. As Hammersley and Atkinson (1995:141) point out,

[t]he differences between participant observation and interviewing are not as great as is sometimes suggested. In both cases we must take account of context and the effects of the researcher.

Similarly, W. Gordon West observes that "the bulk of participant observation data is probably gathered through informal interviews and supplemented by observation" (1980:39). Therefore, it is reasonable to suggest that the three methods are, at least in practice, inseparable (see Lofland and Lofland, 1984:13). At the same time, however, it is possible to carry out a series of in-depth interviews without undertaking prolonged participant observation. Many details of the particular research context and situation must be taken into account in order to determine which course of action is most appropriate in the given circumstances.

Regarding the present project, the vast majority of data are drawn from interviews with scientists. At the same time, elements of observation and participant observation are also present. Perhaps the most fundamental point to make about the data gathering process, though, is that it is consistent with the ethnographic thrust of symbolic interactionism and the generic social process scheme. This is demonstrated in a couple of ways. First, the research process centers on the activities that the members of a specific group engage in on a day-to-day basis. Second, concepts (GSPs) are used: (1) as ways to better understand the idiosyncrasies of the group being studied, (2) as a means to more effectively communicate findings to others, and (3) as ways to compare and contrast the scientific community with other groups. All of these objectives are fostered through direct interactive contact with members of the scientific community. However, research of this kind is not always easy to carry out.

Michael Lynch (1993:104-105) discusses "a few of the pragmatic difficulties faced by would-be laboratory ethnographers." One of these is the difficulty of gaining access to the laboratory setting in order to carry out a participant observation project. Another problem is the "technical jargon" of the sciences that the would-be ethnographer must learn to deal with. A third concern is that the "demands of academia" may not be flexible enough to allow the researcher sufficient time to gain entry, learn a new language, and carry out fieldwork. While perceptions of the validity and/or severity of these limitations may vary, Lynch makes this observation about the present state of science studies.

Rather than undertake the difficult, time-consuming, and epistemologically suspect task of ethnography, many sociologists of science have preferred to take refuge in offices and libraries. There they can act as if they are observing "science in action" while engaging in more respectable academic pursuits: sifting through historical archives and secondary sources, composing scholarly syntheses of the diverse literatures in the sociology of science and related areas, and performing close textual analysis. (Lynch, 1993:105)⁷

While avoiding an extended discussion of what does or does not constitute a "respectable academic pursuit," one point worth mentioning here is Lynch's equation of ethnography with participant observation. While the ethnographic method is based on establishing "intimate

⁷ Lynch openly acknowledges that his own work is not exempt from this criticism.

familiarity" with the setting and the people to be studied, there are a number of ways in which this can be accomplished. Similarly, as some authors have indicated (see Lynch, 1993:113; Knorr-Cetina, 1995:141), more exclusive versions of participant observation research (as practiced to date) have had limited success in enlightening us about the culture and practice of science.

Data Source or Setting

Selecting a data source for ethnographic study in one sense may appear extremely straightforward. Sociologists are presumably interested in every aspect of human group life, and so literally any group offers a potential source for us to increase our understanding of the processes and activities that constitute human group life. Perhaps the principal problem to be dealt with at this stage, though, is determining whether it is possible for the researcher to gain access to some potential subject group. This is particularly important with respect to those groups labeled as deviant, such as outlaw bikers and drug dealers, where it is extremely difficult for outsiders even to locate suitable research subjects, let alone to contact them.

Regardless of what group is selected for study, researchers often benefit greatly from a contact person, who may be an insider, or may have sufficient influence with the group to provide the researcher with some form of initial contact. This person may also be a "gatekeeper," that is, someone whose role it is to keep outsiders out (see Hammersley and Atkinson, 1995:63-67). In this case, if the researcher can provide sufficient information to this individual with respect to their intentions, and to their personal integrity, members of the group may be more open to cooperating and sharing their perspectives. There are no guarantees in any of this, and the researcher may find that even after a prolonged period of work, the potential subject group remains closed to them. In my case, I did not make use of a particular contact person. Rather, I made contact with the chairs of the various departments and made them aware of my activities, both in an effort to enlist their help in selecting interview candidates and so that they would not be "surprised" to hear of my activities, should they receive feedback from department members.

Once contact is made with a group, the researcher then attempts to glean as much data as possible from the members, in ways that minimize intrusion and allow people to carry on activities as if the researcher was either a *bona fide* member of the group or not present. Again, each situation will be different, but much of the success of the research project will depend on the interpersonal skills of the researcher. This aspect is at least as important as any prior knowledge or familiarity that the researcher may have concerning the subject group or its activities.

One technical issue that arises at this stage is that of sampling. It may be the case that the researcher spends an extended period of time with a small group of individuals, or the researcher may spend much shorter periods in a more intensive one-on-one interview situations with a larger group of subjects. The principal criterion should be that of gaining sufficient data to accurately portray part or all of the life-world of the group being studied.

The primary data for the fieldwork portion of this project come from a series of open-ended interviews (n=28) conducted with academic research scientists employed at Canadian universities. Twenty of these subjects worked at a single large university (in terms of student population), with well-established science departments. Four more worked at a significantly smaller institution (about one-tenth of the size) in a different province, and the remaining four each worked at different institutions (in terms of size and location). I was concerned that the bulk of prior research had centered on some specific contribution or discovery made by an individual scientist, or a group of scientists working in the same laboratory. Furthermore, in many cases, the scientists studied were among the elite members of their disciplines (i.e., Nobel prize winners). Consequently, I chose to interview a fairly diverse group of faculty members at various stages in their careers from what are generally regarded to be the four basic sciences, namely, biology (n=9), chemistry (n=8), geology (n=4), and physics (n=7). The decision to select this diverse group of informants allowed me to introduce at least a limited transsituational and transcontextual element into the study. At the same time, the decision was made not to engage in an extended period of participant observation in any one setting.

About two-thirds of the scientists interviewed were contacted by approaching the chairs of their respective departments. I briefly explained my project, and asked them for a list of

potential respondents that would represent a cross-section of the department's interests and activities, and who, in the opinion of the chair, would be receptive to speaking with me. I did not view the chairs so much as gatekeepers, but as knowledgeable informants who could help me to obtain what was, in their view, a representative sample of the range of research activities in their respective disciplines. I then contacted the identified individuals and requested an appointment for an interview. In the majority of cases the initial contact was made in person, rather than by telephone. This allowed me and the potential respondent to put a face with a name. It also allowed me to get an initial glimpse into the setting in which these individuals worked. In a few cases, scheduling conflicts and other circumstances made it impossible to meet with some of the suggested subjects. As well, I received two outright refusals; in one case no explanation was offered, in the other, the scientist indicated that his own research and that of his students was consuming all of his time. The remaining participants were obtained either through recommendations from the other interview subjects or through "cold calls" (in which I approached strangers at their offices or laboratories).⁸

Along with this core set of subjects, I was able to interview a director of research and development at one of the universities, who has had several years experience dealing with government, industry, individual scientists, and handling many other aspects of the management of scientific activity in the academic setting. On a more informal level, I was able to have a series of discussions with other scientists (n=30) over the telephone, and in conference settings. These subjects again represented the four basic sciences, and were from various institutions in Canada, the United States, and the United Kingdom. Also, I was able to attend a series of seminars, in which students presented the details of a major theoretical or methodological controversy within their area of expertise, and then were questioned by fellow students and faculty.

One other important source of data is my own background. I spent nine months as a graduate student in a pharmacology laboratory at a major medical school, where I experienced

⁸ The percentage of female scientists in the departments I studied ranged from about 10% to 30%, with the average closer to the low end of this range. My sample reflects this situation.

many of the processes and activities covered in this project. While this experience took place several years prior to my beginning the present project, this kind of firsthand knowledge of the day-to-day activities that take place in an academic laboratory setting helped me in a number of ways. First of all, I was confident about approaching potential research subjects, without being casually dismissed as an outsider. Secondly, I knew that I would feel comfortable on the scientist's "turf," and would in some sense be a more skilled observer. Most importantly, perhaps, I felt confident that I could carry on an "informed" conversation about science, that might translate into my acquiring better quality data. In retrospect, though, except for the initial burst of confidence, I do not feel that my level of familiarity with natural science necessarily put me in a better position to carry out this research. While my research experience certainly reflects to some degree the perspective I bring to the project, I see no reason why a serious scholar without this kind of background could not carry out a similar research project.

The interviews for the present project took place in either the faculty members' offices or their laboratories. During an initial five to ten minute period, I introduced myself and provided a brief overview of my project, which included an explanation of the interview format and the kind of information in which I was interested. On this last point, I indicated that I was looking for specific examples of everyday practice, rather than generalizations about "types" of things that might be done, or "possible ways" of doing things. In several instances, I was given a tour of the laboratory, a brief glimpse into the courses of action being carried out with respect to ongoing research projects, and I was introduced to some of the technicians and graduate students working with the respondents.

The interviews were tape-recorded (with the express permission of the respondents) for later transcription. The length of the interviews, not including the introductory conversation, varied from about ninety minutes to four hours (the transcription process took about four hours for each hour of interview tape). About eighty percent of the interviews were carried out in a single visit. When subsequent interviews did take place, the interim period was up to sixteen months (in one case). Respondents were willing to meet again, and in all cases the conversation

picked up where it had left off, with no time wasted covering old ground or re-establishing credibility.

The actual interviews were open-ended, in that they started with what Jorgensen (1989:86) calls "grand-tour" questions. In other words, following the introductory conversation, I posed the following sorts of questions to the respondents.

What activities are you involved in on a day-to-day basis?

What projects are you presently working on?

How much time do you spend on this or that activity?

Do you have any graduate students? What are they doing?

The length of the responses to these initial questions varied from about one to ten minutes. Based on the content of these responses, subsequent questions were designed to gain further detail, rather than to shift the focus of the interview. I found that the respondents were very willing to talk about their work and their daily activities, and I rarely felt that I had to prod or intervene in any way. If there was a lull in the conversation, or if the respondent was unsure of what direction to go in, I would ask one of the following sorts of questions.

Can you tell me more about that?

How did you get started on that?

What kinds of obstacles do you encounter?

What kinds of things assist you in your activities?

Every effort was made to allow the subjects to set the direction of the interview, within the bounds of examining the "doing" of science, and avoiding where possible the intricate (technical) details of the science itself. The intention was to focus on the processual elements of the scientific enterprise, rather than the content of the particular branch of science in which the participants practiced.

Data Analysis

The data analysis phase of the present project primarily centers around materials that have been gathered in the field. At the same time, however, these activities of data collection

and analysis tend to take place simultaneously.

Ethnographic research should have a characteristic "funnel" structure, being progressively focused over its course. Over time the research problem needs to be developed or transformed, and eventually its scope is clarified and delimited, and its internal structure explored. In this sense, it is frequently well into the process of inquiry that one discovers what the research is *really* about. (Hammersley and Atkinson, 1995:206, emphasis in original)

In other words, as themes and concerns emerge from the data, the researcher can shift emphases in the field in order to elaborate and draw out particular points of interest. Similarly, given the complexity of human group life, it is impractical for the researcher to attempt to attend to every aspect of what is going in any particular group. So, part of the analysis normally involves evaluating the original design of the project and adjusting the method.

The analysis of field material involves sorting the data into manageable and communicable pieces. This activity is tied into a process of conceptual development that forms the basis for the message that will be transmitted to the readers of the study. Researchers will develop typologies or other categorization schemes that allow them to systematically present a description of the subject matter to a particular audience. One such scheme is the generic social process scheme, which was outlined above.

More specifically, the data analysis portion of this project consisted of four phases. The first stage centered around the transcript material from the first six interviews, which was divided into sections that illustrated a particular process or activity taking place. Looking for some common processual elements in this material gave rise to the seven subprocesses (getting ideas, assessing feasibility, getting prepared, pursuing funding, implementing plans, obtaining results, disseminating results) under the broader process of "performing activities." Excerpts from the transcripts were then placed into one of these seven groupings, and the process was repeated within these areas to establish a further delineation of subprocesses within each group. At the same time, more interviews were taking place.

The second phase involved sorting the newly transcribed material, as it became available, on the basis of these original seven subprocesses, and breaking these down into an expanded set of more manageable subprocesses. At this point, the decision was made to focus on the specific areas of pursuing funding and managing equipment. As a result, subsequent

interview questions were modified to elicit more information on these specific areas. So, for example, I would ask:

How do you fund your research?

What kind of equipment do you use?

What happens when equipment breaks?

What part do your students play in the research process?

At the same time, however, the scientists were encouraged to take the conversation in whatever direction they wanted. I did not want to limit the information that was being provided by trying to direct the interview too much. Generally speaking, I think that I obtained better results when I started an interview on a narrower focus and then let the scientist take it from there.

The third phase involved going back over the complete transcripts to see what had been missed or perhaps misplaced during the earlier steps. Also, the material that had been collected within each subprocess was ordered in such a way as to make presentation and communication of the ideas more coherent and comprehensible. Most of this work was done at the computer. While there are a number of the commercially available qualitative analysis software packages (e.g., CAQDAS, KWALITAN, NUDIST), I worked with a basic word processing package. It provided me with the ability to move large blocks of text between files, and to otherwise manipulate the text in terms of spacing, indenting, and so on.

The final phase of the data analysis to a large degree anticipates the next stage of data presentation, in that it involves drawing out the conceptual material and providing the narrative and explanatory details to accompany the data. Part of this process involves pointing out parallels to other studies of science, and to studies of other human groups.

Data Presentation

The final phase of the research project is the presentation of results, which centers around developing text. Here, researchers aim for clarity and comprehension, while at the same time establishing their own authority with respect to the issues being discussed. Similarly, the

presentation should be balanced with a sense of responsibility to the discipline, and to the research subjects, to accurately portray the life-world that was studied.

One important consideration regarding presentation is to understand the audience to whom the work is directed. Researchers sometimes make the assumption that the primary audience for their work will be limited to those professionals interested in ethnography or in the particular life-worlds that were studied. After reading Daniel Wolf's study of outlaw bikers (1991), I did not think it likely that the research subjects, or members of similar groups, would be overly interested in reading what Wolf had to say (except perhaps for novelty value). On the other hand, Prus (1991:iii) indicates that the 1977 version of *Road Hustler* "had achieved a certain celebrity status in the magic world." In this case, a study of card and dice hustlers had found an audience among another group that recognized a number of similarities between their lives and the lives of the people Prus had studied. With the support of a new publisher, one that specialized in magic, Prus was able to expand his study through a network of highly enthusiastic and open contacts, to include insights from magicians (an otherwise closed, intimate group).

The anticipated audience for this project consists of three groups. The first group is composed of those sociologists that are actively engaged in qualitative research, and who may be seeking insights from studies of other groups for comparative purposes. The second group is made up of the broad array of scholars (historians, philosophers, and sociologists), whose primary interest is in the social study of science. A third potential group is the scientists themselves, who appear to be taking a greater interest lately in what is being said about them (see Weinberg, 1992; Labinger, 1995). One advantage of the style of presentation adopted here is that it provides data in a way that maximizes accessibility and comprehension for members of each of these three groups.

At this stage of writing up the research, it becomes even more evident that the aspects of research design, implementation, and evaluation are intricately linked, and cannot be handled separately. As previously mentioned, the generic social process scheme provides a means by which all of these steps can be carried out in a consistent and coherent manner, that not only benefits the researcher and the research subjects, but all those that read the work.

While the data presentation phase centers primarily on writing, the proportionately large amount of transcript material that appears with this kind of study poses a special problem. The excerpts presented here are arranged according to the subprocesses they illustrate, and they are indented within the body of the text. After each excerpt is a letter indicating whether the subject is a biologist, chemist, geologist, or physicist (B,C,G, or P), and a number (1,2,3,...) to differentiate among scientists in the same discipline. Even with this kind of labeling, the content of the transcript material often provides enough of a clue to allow at least a basic identity to be constructed. For example, a person talking about wasps is likely to be a biologist, and two quotations on wasps are likely to have come from the same respondent. At the same time, there are many more cases where this kind of identification cannot be made.

The particular style adopted here is significantly different from what appears in the majority of social studies of science. While a more narrative style is used with great effect by, for example, Sharon Traweek in her study of high energy physicists (1988), the weakness of these works is in not letting the reader read the words of the scientists for themselves.⁹ This more open and intersubjective style of presentation is the style advocated by Prus (1987, 1996) as being most consistent with symbolic interactionism, and it is the form of presentation used here.

Another issue that needs to be dealt with here, to some extent, is the problem of accounts. For example, I have argued that some of the views developed by Merton and his associates were distorted because they were based largely on the accounts of scientific activity given by Nobel prize-winning scientists. Similarly, I have argued that we should be cautious about the *ex post facto* accounts provided by scientists or ethnographers with respect to the

⁹ Focusing briefly on the scientists, Steven Weinberg (1992:185) indicates that many of the observations made by Sharon Traweek in her study of high energy physicists have a "ring of truth." At the same time, Weinberg is highly critical of what he considers to be the faulty logic of the social constructionists. "No one would give a book about mountain climbing the title *Constructing Everest*" (Weinberg, 1992:188). After examining several works in the social and cultural study of science, Jay Labinger (1995:286) observes that "the bottom-line picture of how science operates always comes out radically different from my own interpretation." He attributes this, in part, to the "virtually total absence of participation by practicing scientists," as active researchers or partners in these studies.

discovery or production of some particular scientific fact. In an examination of the problems associated with accounts, Lorne Dawson (1995) argues that scholars are generally suspicious of accounts because we can never really be sure what someone is thinking or feeling and because accounts are retrospective. That is, they are reconstructions of what took place that reflect the participants evaluation not only of the content and significance of the event, but of the circumstances of the recounting. These two problems constitute a relatively irresolvable methodological dilemma for all social scientists (see Beckford, 1978), and while the debates continue, researchers have generally chosen to get on with the task of studying social phenomena. Dawson goes on to outline four responses to these problems, two of which are based on the notion that as there can be no definitive account of an event. Thus, discourse analysis and ethnomethodology focus on the ways in which social actors construct accounts, with the discourse analysts placing a greater emphasis on the function of accounts. A third type of response is associated with the notion of "vocabularies of motive," as developed by C. Wright Mills (1940). Here, accounts are seen to reflect a negotiated order that might consist of rationalizations, excuses, or metaphysical presuppositions. A fourth approach to accounts is more closely associated with qualitative research and symbolic interactionism. As Dawson (1995:52) indicates, citing Yearley (1988), people's accounts provide a "privileged, although fallible" record for exploring actions and beliefs. In other words, in this view, the decision is taken to work with what we can get, relying on the sensitivity and good judgment of the researcher. The present study can be viewed as accepting some version of this fourth approach, however, in doing so I have tried my best to respect the nature of the social world I am studying, while at the same time eliminating the biases of dealing with extraordinary scientists and extraordinary science.

The combination of a symbolic interactionist perspective, with its emphasis on ethnographic inquiry, and the generic social process scheme provides researchers with an appropriate theoretical and methodological framework for carrying out more genuinely "social" studies of science. Not only does this approach require a focus on particular groups of scientists,

but it allows researchers to take insights gained from the study of one group or other, and apply them to studies of other groups, including those engaged in other realms of human activity.

Similarly, the interactionist approach relies primarily on the day-to-day activities engaged in by practicing scientists for its data. In this way, the researcher is able to show the greatest amount of respect for the activities that constitute the life-world of the group being studied. As with the other elements of this study, this precept would hold regardless of the group being studied.

Finally, someone following the interactionist approach to science does not enter the research process with any preconceived notion of what scientists are doing. It is not the task of the social researcher to determine how scientists obtain the truth, if in fact that is what they are doing. The task of the social researcher is to demonstrate how scientists go about accomplishing their goals on a day-to-day basis, whatever those goals might be. Science is a realm of human action and interaction that emerges out of the intersubjective accomplishment of its practitioners, and thus science is best understood as what scientists do.

Chapter Six

AN EMERGENT VIEW OF SCIENTIFIC PRACTICE

Doing Science

The exploration of the everyday activities of scientists presented here is based on a conception of the scientific enterprise as ongoing instances of intersubjective accomplishment. People engaged in the collective pursuit of specific goals make use of their “stocks of knowledge” (Berger and Luckmann, 1966) as they engage the objects that make up their life-worlds. This knowledge base reflects the “paradigm” (Kuhn, 1970) or “thought style” (Fleck, 1979) of the group into which people have been socialized, and within which the members perform the kinds of activities illustrated in the following pages.

The particular group or “thought collective” (Fleck, 1979) with which the individual identifies not only provides the cognitive environment or conceptual reference points within which science is carried out, but also forms the dynamic forum within which the ensuing processes of social interaction and interpretation take place.

If we define “thought collective” as a community of persons mutually exchanging ideas or maintaining intellectual interaction, we will find by implication that it is also provides the special “carrier” for the historical development of any field of thought, as well as for the given stock of knowledge and level of culture. (Fleck, 1979:39)

Similarly, to conceive of a community of scientists as existing in isolation from other groups of scientists, or from a variety of other human groups with whom they may interact, places too great a restriction on our efforts to understand what is going on in the scientists’ life-worlds and in society more generally.

The complex structure of modern society results in multiple intersections and interrelations among thought collectives both in space and time. We see professional and semiprofessional thought communities in commerce, the military, sports, art, politics, fashion, science, and religion. The more specialized a thought community is and the more restricted in its content, the stronger will be the particular thought nexus among the members. (Fleck, 1979:107)

So, while the diverse array of groups that constitute society is engaged in a continuous process of interplay, some groups may be perceived, by both insiders and outsiders, as being more

coherent or self-sustaining, and therefore less reliant on, or open to, outside influence. As illustrated in chapters 2 through 4, on several occasions scientists have singled themselves out, and have been singled out by others, as one of these special or esoteric groups (see Abbott, 1988; Fuchs, 1992).

One of the bases for this exceptional treatment of scientists is the almost isomorphic identification of the community of scientists with the corpus of scientific knowledge. Furthermore, this scientific knowledge generally is equated with a rather narrow and fixed kind of knowledge, for example, the "laws of physics" (see Rouse, 1996:224-227). By focussing on the day-to-day activities of scientists, the data from the present project support a less limited view of what constitutes knowledge, and therefore a less limited view of the scientific enterprise. Consistent with the symbolic interactionist perspective, knowledge is conceptualized as being comprised of the meanings that people attach to the objects they encounter, as they engage in the activities that constitute practical accomplishment in their life-worlds.

Since everyday life is dominated by the pragmatic motive, recipe knowledge, that is, knowledge limited to pragmatic competence in routine performance, occupies a prominent place in the social stock of knowledge. (Berger and Luckmann, 1966:23)

In other words, there is a well-defined and well-understood aspect of knowledge that, while certainly not in opposition to some broader set of guiding principles or some overarching stock of knowledge, comes into play on a more immediate basis as members of the group deal with the situation at hand. This "recipe knowledge," or "tacit knowledge" (see Polanyi, 1962, 1967; Collins, 1992; Dolby, 1996), encompasses a variety of norms, rules, skills, and ways of doing things, that constitute the perspective within which group members act on a daily basis. Thus, in our efforts to understand science, there is no need for us to assume that everything that takes place in the scientific enterprise is directed at, or guided by, the pursuit of truth (see Solomon, 1995). Rather, we can focus our attention on those understandings and perspectives (knowledge) that members of the group make use of in their daily encounters with others and with the material objects that are part of their working environment.

A similar opinion is expressed by Pickering (1995:3), who advocates that we view science not simply as a "field of knowledge," but as "skills and social relations, machines and

instruments, as well as scientific facts and theories." All of these elements and the ways in which they can be combined constitute the cultural multiplicity and heterogeneity of science. In other words, for Pickering, "[a]ll of the dimensions of science... - the conceptual, the social, the material - have to be seen as fragmented, disunified, scrappy" (1995:3). This apparent dismantling of the scientific enterprise is not intended to undermine the integrity or accomplishments of scientists and their work. Quite the opposite is the case. Examining the cultural multiplicity and heterogeneity of science will help us to understand better how those engaged in the scientific enterprise have achieved so much.

In an effort to move past the limitations of much previous work in the social study of science, Pickering indicates that his "concern is with scientific *practice*, understood as *the work of cultural extension*" (1995:3 emphasis in the original). In other words, shifting to the vocabulary used here, science is to be viewed as practical accomplishment. As previously discussed, the difference between Pickering's view, which incorporates Latour's notion of non-human agency, and that of the present project, is that the symbolic interactionist perspective is based on the concept of (exclusively human) intersubjective accomplishment.¹ The material (non-human) elements of the scientists' environments are best seen as part of the "obdurate reality" (Blumer, 1969), which Prus (1996:246) describes as being reflected in the "irreducibility of intersubjectivity for the human condition."

This is rooted in a pragmatic appreciation of: (1) the most basic resistances to human action experienced daily in the material and social environments of the human struggle for existence, (2) the objectifying nature of being human, (3) the resultant phenomenon of "culturally motivated resistances" stemming from the ongoing and group nature of human life, and (4) the rudimentary and universal social processes undergirding the ongoing accomplishment of human group life. (Prus, 1996:246)

In other words, the perception of non-human agency arises out of, not gives rise to, the day-to-day processes of interaction and interpretation in which scientists are engaged.

Similar to Pickering's notion of "cultural extension," and more consistent with the interactionist conception of obdurate reality, is Sharon Traweek's concept of community that was

¹ Pickering (1995:63) acknowledges this divergence with symbolic interactionism.

introduced in Chapter 3. In outlining the kinds of things that researchers should look for when studying a particular community, Traweek recognizes four domains: ecology, social organization, developmental cycle, and cosmology. The first of these concepts refers to the environment in which activities are carried out and the artifacts that are employed in those activities. The element of social organization deals with the formal and informal aspects of group structure, along with the processes that take place as conflicts are resolved and information and material resources are exchanged. The notion of a development cycle refers to the stages that individual members of the group pass through as they go from being new recruits to seasoned veterans within the community. Finally, the concept of cosmology has to do with the group's system of knowledge, skills, and beliefs, as well as the processes for determining what does and does not count as part of this system. As Traweek indicates, these different aspects of culture are highly integrated and cannot be separated either in practice or in any account of the community put together by the ethnographer. The processes and activities outlined by Traweek in illustrating these domains parallel those that constitute the generic social process of performing activity. By restricting our focus to the scientific enterprise, these activities can be conceptualized as comprising the process of "doing science," and the specific community of practitioners (scientists) can be seen as one of the many subcultures that constitute modern society.

Fine and Kleinman (1979) indicate that the term "subculture" may indicate that its members share some features with the broader social community of which they are a subset, but at the same time possess certain characteristics that set them apart from others. In the urban research conducted at the University of Chicago, for example, certain ethnic and deviant groups were identified as distinct subcultures within the broader culture of the city (see Kurtz, 1984; Prus, 1996). Thus, through Nels Anderson's (1923) study of the homeless, we not only learn about migrant workers and tramps, we also learn about the lives of the people involved in the various businesses (bars, pawnshops), control agencies (police), and social welfare agencies with whom the homeless continuously interacted. Similarly, in Paul Cressey's (1932) examination of taxi-dance halls, where men paid (a dime) to dance with the girl of their choice, we learn about the owner-managers of these recreational facilities, the dancers and other employees, and

the patrons that frequented them. The various subcultures that comprised the urban landscape were not only distinct groups in themselves, they also represented the intersection of a number of other groups. Meanings would develop and be shared across these complex lines of social interaction and interpretation.

While obviously focussing on different sets of attributes, the same kind of observations can be made with respect to the scientific enterprise. The broader scientific community, while sharing certain common perspectives (knowledge), is comprised of a diverse array of paradigms, thought collectives, research programs, and subcultures, each with its own understanding of its immediate life-world. As Traweek (1988) indicates, not only is the high energy physics community distinct from other groups of physicists, but theorists differ from experimentalists, and Japanese high energy physicists differ from American high energy physicists. Similarly, society as a whole is comprised of a diverse collection of cultural, economic, political, and religious groups of which the scientific community is only one part.

The present project identifies the scientific enterprise as a collection of subcultures that can be associated with the particular sets of practices that they engage in. These practices are determined by the members of the particular subculture and are recognized (legitimated) by both the members of the subcultural group and the larger community of scientists. This conceptualization is consistent with the notion that science is best understood as arising out of the intersubjective accomplishment of those who identify themselves as scientists, more generally, and, in a more limited sense, as members of specific subcultures within science (disciplines, sub-disciplines, research groups). Similarly, the use of the generic social process scheme allows us to recognize and describe the idiosyncrasies associated with any particular group, while at the same time drawing attention to some of the transsituational and transcontextual elements of human group life.

The sections that follow should be read within the context of the three objectives of this project. First, does the coverage offer a clear demonstration of a symbolic interactionist approach to the social study of science? Second, does the material presented here demonstrate the value of this approach in helping us to understand the nature of the scientific enterprise?

Third, do we learn things about aspects of the scientific enterprise that have not been forthcoming in alternate approaches to the study of science? The generic social process scheme provides the framework within which all three of these questions are addressed and assessed. While a certain order of presentation has been imposed on this material, the sections in many ways could stand alone. They each offer glimpses into specific aspects of the scientific enterprise, while at the same time forming part of a much more sustained exploration of this complex area of human group life.

The data presented here are organized around the generic social process of "performing activity," which is comprised of a number of subprocesses, in particular:

Getting Ideas

Assessing Feasibility

Getting Prepared

Pursuing Funding (see Chapter 7)

Implementing Plans

Obtaining Results

Disseminating Results

Through an examination of these processes, and the various activities that each subsumes, researchers can explore in a more intimate fashion the streams of action that comprise the practical accomplishment of science. The process of elaboration, by which these processes are broken down, represents an attempt to sort the complexities of this aspect of human group life into more manageable and communicable pieces. As was pointed out in Chapter 5, the generic social process scheme is a heuristic device. There is no intention to "force fit" the actions of scientists into predetermined categories or typologies. These notions represent "sensitizing concepts" (Blumer, 1969), reference points that foster the open exploration and inspection of everyday life. Thus, the objective of this kind of "naturalistic inquiry" (Blumer, 1969) is to broaden our understanding of social dynamics, rather than to test hypotheses or reify concepts.

Getting Ideas

Like many other aspects of human group life, science is an ongoing set of activities that does not present us with a definitive starting or end point. The process of "getting ideas," however, does provide us with an intuitive launching point from which to begin a presentation of the activities of scientists. Thus we may ask: "How do scientists get ideas for the research in which they engage?" "How do researchers get started on particular projects?"

In collecting the data for this study, it became evident that all of the processes delineated here share a high degree of overlap and interrelatedness. This accentuates not only the complexity of the area under study, but also the complexity of the qualitative research process itself.

It's been my experience, that every time I've done an observing project, and come back with data and analyzed it, it's answered certain questions, but it's also produced more questions, and it's also suggested directions in which one might go. So, I look at that, and I talk to other people - toss out my ideas - find out what other people are doing, and normally just go with my instincts as to what I want to do. And, that instinct is based on my own personal preferences and my training, and then I add on to that what I know is being done in other areas in the literature. (P1)

As this excerpt suggests, the process of "getting ideas" that can be translated into scientific research projects consists of a variety of subprocesses that includes, but is not limited to: reflecting on one's own work, monitoring other research, discussing ideas with others, attending conferences and seminars, reviewing the literature, and conceptualizing projects. New ideas may emerge from activities seemingly as mundane as walking into the laboratory of the colleague next door and asking them how their research is progressing, or out of a comment overheard around the coffee machine while attending a conference or seminar. Also, those attending to the literature are apt to find that the vast array of professional journals provides extended records of other scientific activities upon which new projects can be built. On the other hand, a few of the scientists interviewed indicate that sometimes the best ideas come seemingly "out of the blue" in the middle of the night.

One day you may wake up with an idea that is completely unrelated to anything that you've ever done in the lab, and you try it out and sure enough it works very well. (C3)

So, the possible sources for research ideas, and the ways in which these ideas may arise, reflect a broad range of intersubjectivity, from personal reflection to wide consultation with others, and from following closely on past accomplishments to striking out in new directions.

Many scientists that I spoke with made a special point of emphasizing that scientific research does not follow some kind of linear or deterministic path. Nor, they say, can we look at what might be perceived of as some end product of science, and then try to work backwards from that to discover where the initial idea may have come from. The process of getting ideas follows a rather different path:

Whenever you use the telephone you're using lasers, and where did they come from? They sure didn't come because some government said, "We've got to develop a laser industry." They came because someone doing basic science was trying to understand something or trying to find a way to measure something, and inventing a new device that would help him to do some experiments in the laboratory. And, they happened to have some application later on. (C6)

The specifics of telephones, lasers, or any other products of science, then, do not provide the point from which to embark on an exploration of how scientists determine what research they will engage in. Rather, the process of getting ideas for projects can only be understood as never-ending and emergent. At the same time, the genesis of research projects is "occasioned" (see Knorr-Cetina, 1981) in a way that reflects the situations and contexts within which projects emerge. People may develop, and embark upon, specific research agendas, but this should not be seen in deterministic or exclusively rational ways.

The implication of this observation is not to suggest that scientific activity is totally random or ad hoc. As in other realms of human activity, actions are shaped by considerations of people's "stocks of knowledge" and their tendency to take the concerns and actions of others into account. Scientists indicate that research can arise, for example, in response to developments in theoretical and/or experimental aspects of their field:

The research I do is a combination of theory driven and experiment driven. People in the field will be doing forefront experiments, but they can't interpret them quantitatively without very complicated theoretical methods and computational techniques. So I work on developing theoretical methods and efficient means of applying them. The physical problem or phenomenon I'm interested in comes from the discipline. (C4)

In this case, the scientist is aware of ongoing problems among experimentalists and, for whatever reason, decides to engage in a series of activities that will provide material that addresses issues pertinent to a broader set of scientists. In this way, the researcher is not only responding to a backlog of existing problematic situations, but to some degree is anticipating problems that have yet to arise. Regardless of the motivation for following this research agenda, the selection of this particular course of action is contextually and situationally meaningful to the scientist and therefore of interest to us.

At other times, the scientist's awareness of the activities of others within their field is an important element in shaping their level of involvement and in shaping their identity within that field. Getting ideas for projects is integrally linked with other aspects of the research process, and with the broader range of activities taking place in the scientific community.

This goes back to knowing the field that you're working in well enough to recognize that something is potentially productive, and then pursue it. There is now so few people working in the area that I do, that I can continually live off the cream of the crop, and I can skim off the cream of projects all of the time, and I don't have to go down to the nth degree of understanding. I can just think, "Well, I can work on this." And, I have a broad enough understanding to know that this aspect could be publishable, and that I can keep working away like that. So, with a shrinking pool of people working with a particular focus, there's more potential for one to do that. (B1)

Aside from those ideas that are generated from within the scientific enterprise, or by scientists directly, there are occasions when members of the general public provide an opportunity for scientists to initiate research projects:

A farmer contacted a colleague of mine, because he had dug a pond in the bottom of an old railroad pit, and in the process of digging down, he found a layer of plant material under the clay. I went up and took a look at it, and what we hope to obtain out of this is an assessment of the time in which these plants lived. (G2)

While we cannot speculate on what might have motivated the farmer to contact a geologist about his discovery, we can draw attention to the fact that the scientist could in no way anticipate that this event would occur. Here, a chance situation initiates a chain of decisions that are made within the context of the other activities that the scientist is engaged in. Similarly, interaction

with individuals involved in business and industry can lead to specific courses of action being undertaken in scientific research that may benefit both parties.

I started looking at the bedrock geology in Southern Ontario – a number of projects that were just essentially probing around seeing what is really interesting. I had a bit of guidance for some of them, and essentially I phoned up people who are in the industry, or closely affiliated to the industry, and said, “Look, I eventually would like to get money from companies, or at least be able to get the attention of people reading literature in an applied way, to take notice of my research.” I could do something now, but ten years from now, you cash in on it. Okay, that happens. So, I was pointed in a few research directions that they thought were worthwhile and I looked into them myself, and I thought, “Well, what are these things? Which of these things really interests me?” And there were quite a few of them, and that’s how these projects got started. (G3)

Getting ideas then is not an isolated activity, and, contrary to Popper’s claim that the source of ideas is unimportant, the process is linked to a number of other activities and processes that together constitute the everyday work of research scientists. To treat this activity as if it is somehow special, or self-contained, is to misrepresent the nature of the scientific enterprise. In the same way, to demonstrate that the process of getting ideas is wrapped up in the social interaction of scientists neither degrades nor trivializes this activity.

Scientists engage in a process of transforming the meaning of everyday objects, as they establish new courses of action in their research. Parallel processes can be found in the activities of magicians (Prus and Sharper, 1991) or comedians (Stebbins, 1990) putting together a new act, or among members of a new religious movement as they determine how to present their worldview to others (Barker, 1984). For example, Stebbins (1990:46) indicates that a comedian finds new material

as he or she shops, watches television, reads the newspaper, walks around town, attends a concert, drives somewhere, takes an airplane flight, and so on.

Thus, the process of “getting ideas” can be viewed as a process of interpreting the situation at hand in such a way as to “make it their own.” In other words, scientists and others respond to the information they receive through interaction, at many levels, so that it becomes part of their own perspective and their own identity.

With respect to the process of getting ideas, more generally, the brief coverage presented here suggests that the positivistic and Mertonian differentiation between internal and

external aspects of the scientific enterprise is misleading, as it exaggerates the independence of science. There is no way to specifically delineate the single, or finite multiple, sources of ideas for scientific research. Rather, ideas emerge out of the intersubjective accomplishment of everyday life, as scientists interpret what is going on around them and selectively integrate these interpretations into their day-to-day work. Future research on specific instances of getting ideas could provide more details on the diversity of elements taken into account by scientists as they select directions for their work and determine goals.

Assessing Feasibility

Ideas for projects are not enough in themselves. Scientists normally contextualize these in terms of long-range career strategies, and they are assessed for goodness-of-fit with available resources and the larger research goals of the department or institution within which the scientists work. These kinds of considerations are an important element of the scientist's everyday life, and are generally taken into account when people try to determine whether any particular project can actually be carried out.

As is the case with all of the processes and activities discussed throughout this project, assessing the feasibility of some particular course of action typically involves some consideration of the activities and receptivity of others. The practical accomplishment of science, as with any other realm of human group life, will take place only through cooperation, the forming of alliances, and the sharing of perspectives. The kinds of activities involved in assessing project feasibility can include: evaluating individual projects within larger contexts, estimating costs, allocating material resources, determining levels of risk, encountering ambiguities, determining the need to shift theoretical directions, and determining the need to adopt new techniques or methods.

In some cases, the feasibility of a project is contingent on whether potential researchers think the phenomena of interest can be observed (i.e., accessed, measured, and transformed into data). For example, Jed Buchwald (1995b) describes how Heinrich Hertz, in an attempt to establish the existence of a particular phenomenon (cathode rays), had to borrow and develop a

number of technological advances in order not only to produce purer cathode rays, but to detect them with greater certainty. The scientists interviewed for the present study indicated quite a range of considerations:

Could you find a nesting aggregation of the wasps? Could you mark the wasps? And, could you sit down and actually see what they were doing? Basically what I did was, I went out and tried it in a couple of small nesting aggregations right near the university, and then based on what I did there, said, "Yes, it is feasible." So, I got my model airplane paint, my toothpicks, and my net; caught some wasps, marked them, and then... Could I actually sit and see the marks? What colors worked? What colors didn't? You don't use orange and red, because, I mean, by and large, you can't make that distinction. You can tell blue and yellow apart, so lots of details like that. What sort of markers would fit in beside the nest? What sort of numbering did you put on them, so the numbers were still there next year when you came back? (B2)

I've always been interested in star clusters, and now I study the globular clusters which are in other galaxies. Until about 20 years ago, we didn't have the telescope capability to do that. When the four-meter telescopes came in around the world, we could then observe these things. These things are naked-eye objects in our own galaxy, practically, but in other galaxies, they are dots that are near the limit of detection in the biggest telescopes. (P1)

While both of these examples point to human limitations, particularly with respect to human visual acuity, the way in which the problem is handled in each case is very different. In the first case, the researcher can directly alter the object being observed through some kind of physical intervention. In the second case, the researcher is dependent upon advances in technology, over which he or she may have little or no control, that provide an extension of human capabilities, rather than altering the object to be observed in some way. At the core of both of these instances, however, is the construction of a solution to the problem of convincing others of one's ability to observe a particular phenomenon. In other cases, it is exactly because some particular aspect of "nature" is directly observable that research can proceed.

I had a series of questions in developmental biology and evolution and I was looking around for a system in which to test some ideas, and really the best system was these blinking tadpoles: (1) they're clear, so you can see their teeth developing; (2) you can put them to sleep; (3) their teeth grow on their sides, so you can actually observe them growing. (B6)

The criteria for evaluating research projects will differ from case to case, and may arise out of a situation where the scientist balances his or her personal research interests with commitments to teaching and/or administrative duties, as well as the activities of colleagues.

When I'm teaching, I go to the lab first thing in the morning and make contact with the group, and then my teaching is usually in the morning. So, I will do the lecturing, and then in the afternoon, I try to spend the afternoons in my office, I will have lots of interruptions from undergraduates. (P4)

I would say, of the time available to me, that I spend fifty percent teaching and fifty percent doing research, and I don't mind that split. It is the other time writing grants and attending meetings that is really bothersome. (B7)

Similarly, the cost of undertaking a new project may be measured in terms of dollars of grant money, time taken away from other projects, or time that must be invested in learning new techniques.

It's something that I sat down and thought very long and hard about, because I knew it was going to be massive effort, and so I sat down and carefully thought, "Can I really afford to spend this much of my life, and a lot of it not getting any results?" (P4)

In some cases, a balance between a number of different elements is negotiated:

We've had to figure out a list of plants that an undergraduate student can look for and accurately identify, because if they are not identified accurately the data is completely worthless, and how much can you get out of this student in the summer? So... balancing desires, wants, and expectations of somebody who's a botanist, somebody who's a park manager, and a student who might like to have a couple of hours a day off. (B9)

Here, a scientist is collaborating with others who, while sharing an interest in this common project, are each looking for their own specific outcomes. At the same time, a student is being employed to carry out the fieldwork and this imposes certain constraints on what one (reasonably) can and cannot be expected to accomplish.

In another case, the allocation of resources, material and otherwise, depends upon a knowledge of: present limitations within one aspect of a scientific specialty, the advances that are taking place in another aspect, and what kind of investment is practical in order to address this particular set of circumstances.

The computational methods have lagged behind, so while people are doing these really neat experiments, the analysis of them at the fundamental level has been lacking. If it takes one hundred hours of CPU time then that run is feasible, but if it takes one thousand hours, one-eighth of a year, then that is not feasible. So the kinds of systems that we can study are limited by the speed of the computer. (C6)

In other situations, the feasibility of engaging in a certain research project may be dependent upon constraints or circumstances that are nearly impossible for the scientist to

measure with any level of accuracy or confidence. One example of this may be the cognitive ability or problem-solving skill of that scientist:

We're worried about developing methods and being clever about understanding the physical phenomenon, so there are some problems for which the technology is no limit at all; our own cleverness is the limit. (C6)

When those doing science are presented with a diversity of objects and a variety of possible paths to follow, the interpretation of the particular circumstances frequently becomes a limiting process. Star (1983) refers to the "simplification" process that scientists engage in to arrive at a manageable research project, and Fujimura (1987) similarly describes this process as constructing "doable" problems. A particular course of action is only doable when commitments and resources have been aligned in support of the work that the project represents.

Scientists may also consider the element of risk relative to decisions about what can and cannot be done. The determination of a course of action in some cases may rely as much on a scientist's assessment of themselves and others, as it does on material considerations.

Scientists deal with a number of ambiguities and obstacles, and we might assume that they would make every effort to maximize their potential for success in the selection of research projects (see Knorr-Cetina, 1981:74-78). The situation, however, is not that simple:

It is incumbent on people to try risky things, and to a certain extent, the riskier the better, because often times if it's riskier, it's also newer, and there's a correlation there. Your star rises ever higher if you pull off one after another of these very risky, and complicated, and ingenious experiments, and make them work. (G3)

Scientists balance a complex array of objectives, and make decisions that often reflect personal assessments of the situational and contextual contingencies that present themselves. The determination of risk, however, extends beyond the particulars of the specific research project to encompass an assessment of where the project fits into longer-term plans or goals. At this point, specific research projects may be undertaken, postponed until some circumstances have changed, or they may be rejected outright.

Much of what takes place at this stage of scientific research can be viewed as influence work (Prus, 1989a). Just as sales people attempt to build trust and gain the confidence of

potential customers, scientists present their ideas in such a way as to neutralize resistances and obtain commitments from others. For example, Traweek (1988:100) indicates that

[p]hysicists tell stories about the prowess of their own group and use their highly developed rhetorical skills to persuade others to support their work.

Thus, to say that a certain course of action is feasible is to say that a collection of individuals, each with their own interests and agendas, has come to accept some particular definition of the situation, such that some particular course of action may proceed. The process, however, does not end there, and much more work may have to be done as new opportunities for social interaction and interpretation emerge along with the practical accomplishment of any given project.

Getting Prepared

Once the decision to undertake a specific program of research is made, scientists try to draw together the elements that will allow them to proceed. In the process of "getting prepared" some of the following activities may be involved: learning new technologies and/or techniques, making commitments, acquiring supplies, designing experiments, promoting projects, discarding projects, revitalizing projects, and hedging bets.

While some research projects may fit nicely into the ongoing activities of a particular scientist, others may require significant changes in the scientist's work environment. This may involve the establishment of new associations with scientists engaged in similar research, or it may require large-scale changes in the equipment and procedures that the scientist has used in the past.

I had done a master's on wasp behavior, so I understood how to go out and do sampling, how to identify wasps, and what sorts of things other people had sampled, and what they had looked for. It doesn't sound like a lot now, but I had spent two complete summers outside specifically doing observations on wasps. (B2)

So, while there is a level of accomplishment associated with a set of activities (in this case a master's degree), this same set of activities served as a kind of dry run or rehearsal for a more sustained or complex application. When dealing with others, particularly those that are less

familiar with the kind of work to be done, this process of practicing or rehearsing can be quite complex.

The student is largely going to be doing the sampling, but it's my responsibility to make sure that the sampling protocol is doable, and that the data we get out of it is realistic. So, I sat down with the park ecologist and the botanist, and we've come up with a sampling protocol, and in two weeks the student is going to be going out and giving it a dry run. I'll be there part of the time, and then the student and I will sit down at the end of that and modify the sampling protocol based on what is actually doable. Can all the plants be identified? Do we have enough money in the budget to cover all the travel that the sampling protocol requires? And, I also have to go back and relearn some plants that I might have known 20 years ago. (B9)

This situation demonstrates the close linkage between the processes of assessing the feasibility of a specific project and getting prepared to actually carry out that project. While there is still an air of uncertainty over whether the goals can be attained, a certain level of commitment and investment in the project is often required.

In some cases, this investment may involve learning or developing new techniques, or it may require the designing and building of new pieces of equipment in order to perform the kind of measurement that the scientist wishes to make.

I developed a method, it was really very simple. No one had ever thought of putting these animals to sleep, opening their mouths, and going in with a microscope and measuring these teeth as they came up. Everything else had been cross-sectional. You take five at day one, five at day two, five at day three, and so on, then you try serially to reconstruct them. I did it longitudinally. This was completely new. (B6)

The majority of the work - you know, 60 or 70 percent of the work - is building the instrument, because it doesn't exist. Once you build the research equipment, then you use it to measure something. The point is, you want it to measure something for which there is no commercially available piece of equipment. (C7)

Scientific work is not just about making measurements of the phenomenon under study. All of the preparatory stages, such as, conceptualizing, designing, building, and using instruments, are an integral part of the process of "doing science," and thus cannot be ignored in any examination of the scientific enterprise that attempts to respect the nature of the scientist's life-world. Harry Collins (1992:79-111) presents an interesting analysis, regarding the case of gravitational waves, of the relationship between the claims that scientists make and the design and application of the equipment that they construct to support those claims. Joseph Weber attempted to measure

gravitational waves using a massive aluminum alloy bar weighing several tons. In order to eliminate any possible interference, the bar was suspended in a vacuum chamber on a thin wire, and the chamber was insulated from the ground by a series of lead and rubber sheets. Even with these precautions, the fact that the bar was at a temperature above absolute zero meant that random vibrations of the bar's atoms could give the impression that gravity waves had been detected. Thus, many of Weber's findings were dismissed as noise. Support for his claims grew when simultaneous readings were detected on two or more detectors separated by a thousand miles. As this case illustrates, it would be extremely difficult to state with any certainty, which of these activities should be considered scientific and which not.

Preparing for a new research project not only involves dealing with material resources, but often also includes probing the community of scientists to determine what others seem to know about the phenomenon in question:

I was totally ignorant, like most of the people are in my field, regarding these things (freshwater lakes), and so I thought, "Well, what do we know about them in terms of sedimentology?" I contacted a few people, and it turns out, we know about their biology, we know something about the water chemistry, something about paleontology, but in terms of their sedimentology, it was still virtually unknown. (G3)

I was around as these programs were being developed. I know some of the people who wrote these programs. I know other people who are using these programs. So, I get in there and I start trying to do it myself, and when I find out I have problems, I either get help from somebody, or I try to talk to the people writing the programs and find out what their motivation is. So, you have to do some... there's quite a learning curve in it, and I'm by no means the most skilled person at using these things. So, I find out what I can do, what the limitations are... So, it's a kind of a learning process all the time. You have to... you either are developing the programs, or you're getting them from somebody else, and finding out how they did them, finding out what other people's problems are before you start doing your own analysis. And, for instance, there are some programs... we now have enough of these things around that there are people who say, "Oh no, you don't want to use this for that, you want to use this." (P1)

Scientific accomplishment, then, is very much an intersubjective process that arises out of a collective understanding of what has been examined in the past, and what remains to be examined. Scientists check things out with their colleagues, but they may also gain some idea of what is known by searching the literature:

When you find a new fossil, you've still got to go back to the old literature and say, "Is this like what so-and-so described in 1930?" So, you need the literature. (G1)

Another case sounds similar to what Harry Collins (1992) discovered when he visited the laboratories of some scientists trying to build a TEA-laser from the written record.

Most scientific techniques are described in the literature, and, in theory, you're supposed to be able to reproduce the method from that, but there are always little details that don't make it into the written procedure. So, you talk to people who are doing that, and with a little practice you master the technique. (B3)

This can also involve examining pre-prints of articles that have just been completed.

The reason why this is so important is the immediacy of scientific information. From the time of acceptance to the time of publication in a journal, you can wait up to a year. Well, I mean that's old news. You want to know now what's going on. Often somebody will go to a conference, and come back and say, "Guess what? There's this new idea by so and so." Well, you could wait three months or more before seeing the paper. By the time you got it, of course, and read it, they're already on to the next thing. (P2)

The process of getting prepared involves more than just the situational imperatives that confront the scientist in the here and now. As each new project is embarked upon, the work to be done is contextualized within the broader sphere of research interests and accomplishments within the scientific specialty.

Implementing Plans

The actual implementation of a research project may appear to consist of activities as diverse as travelling half way around the world to view a solar eclipse lasting a matter of a few hours, or the meticulous microscopic examination of thousands of thin slices of specimens that have been extracted from specially treated animals, rocks, or plants. These are examples of the kinds of activities that may be consistent with a popular perception of the "work" of science, but in neither case does this kind of description begin to scratch the surface of what actually goes on in practice. The evidence indicates that the variety of activities that scientists engage in is enormous, with the "drama of discovery" often representing a relatively small proportion of the time and effort that research typically entails. Many of these activities can be explored through the processes of: administering budgets, performing experiments, juggling multiple projects, gathering data, collecting samples, performing tests, managing students, and managing

equipment. A selection of these activities is illustrated here, but a separate chapter is devoted to the activities that constitute “managing equipment” (Chapter 8).

Looking now at some particular aspects of implementing plans, as in other areas of human group life, scientists establish daily routines and engage in a variety of mundane activities, such as collecting samples, that provide a level of stability and consistency to their lives:

I work with fossils that are extremely small. I'm a palynologist, which means I work on stuff smaller than a paleontologist would. I do research on fossil spores and extinct one-celled algae, so I spend some time in the lab just dissolving rocks. I've got to dissolve the rock away; what's left over after the rock dissolves away might be fossils, so I do quite a bit of lab work. I don't have that big of an operation, and sometimes it's nice to just do the processing of material. When you are tired of thinky stuff, I don't mind going in the lab for half an hour and rattling beakers, and so on. (G1)

I spent three summers - it was more than summers - it was six months out of the year, out at my field sites where the wasps were – sitting in my lawn chair, with my binoculars, my clipboard, and my paints. And, for each session of two or three months, I'd select a place where the wasps were nesting, mark all the nests, mark all the wasps, and sit and record – from what I had done in my feasibility studies - what sorts of behavior it was feasible to record. I sat and did that for many hours of the day, and spent the rest of the day putting the data into a computer. For the most part, the actual collecting of the data - doing the science – was repetitive, and it was just a matter of doing the same thing again and again and again. (B2)

This is what I did, day in and day out, seven days a week, usually for about a month at a time. I put an animal to sleep, opened its mouth, looked in through a microscope, measured the distance between teeth, and recorded the stage they're at, ten to twelve hours a day. (B6)

These elements of scientists' daily lives are tied into the processes of achieving identity and being involved in as much as scientists are constantly interpreting and reinterpreting their actions. As such they come to identify themselves, and to be identified by others, as linked with the kinds of activities that they carry out.

Even these seemingly solitary actions arise out of an intersubjective context, and others may be more directly involved from time to time.

I've done a lot of my own collecting, in particular with birds. I had to get a collection from the Wildlife Service, so I had to go out and shoot them or trap them or whatever... I also get a lot of specimens brought to me by people who know that I'm interested in having things scraped off the roadsides. (B1)

Scientists rely on the cooperation of others in order to accomplish their research goals, and so a certain amount of effort must be expended to establish and maintain “working” relationships with a number of individuals and groups.

I'm working with a young molecular biologist, and basically he and I are working together more and more. I'm not about to pretend that I'm a molecular biologist at my age, so I teamed up with one. (B7)

We're doing the DNA work here. We're growing plants under sterile conditions, so we're sterilizing seeds and germinating them under sterile conditions, and then we isolate the DNA. And, we're going to use the existing c-DNA clones that we have to screen DNA from a variety of different plants, just to see how widely distributed this gene is. But, we have to rely on (another scientist) to do the protein work, because he has the fast liquid chromatography equipment that you need, and so the division of labor is working that way. It's more or less who is capable of doing what. (B3)

Also, the scientist may feel obliged to take the “generalized other” into account.

Scientific activity is not carried out in isolation from members of the public, or from other groups whose interests and activities may overlap in positive or negative (and possibly diverse) ways.

The act of going out and collecting specimens of course in this day and age of emerging animal rights, and all that sort of thing, is becoming more and more frowned upon. But, you do it discreetly, and you collect small samples. There's no need to go out and slaughter a whole population of anything. A sample size of ten or twenty animals is ample. (B1)

In cases like this, scientists are aware of broader perceptions of the kind of work they do and, in order to avoid confrontation and possible disruptions to their plans, they moderate their actions accordingly.

Within academic settings, particularly, scientists tend to be engaged in multiple projects.

Here, graduate students often carry out the bulk of the work, while the professor supervises or troubleshoots:

We're working on many different projects. I have six graduate students in my lab group, so all of them are dealing with something different. I wouldn't want them all doing the same thing. Many of them fall into one sort of category though. In other words, they're all working on different types of materials, but they all fall into the same classification, what we call polymer-oxide nanocomposites. After the students have their material made, then it comes down to studying the properties. (C5)

So, the process of implementing plans within the scientific enterprise can involve not just balancing multiple projects, but also monitoring the activities of several individuals whose

actions can greatly impact the success or failure of a particular course of action. Similarly, not all of the projects will progress at the same rate, or consist of precisely the same set of activities. Consequently, the complex job of managing scientific practice requires ongoing evaluation and assessment that may lead to a variety of adjustments being made on a day-to-day basis.

Whatever the circumstances may be, whether in science or any other realm of human endeavor, the objective in implementing a certain course of action is the accomplishment of a goal. In some instances, the goal may be the successful completion of a business transaction (exchange of goods or services for money), say, between a sales clerk and a customer (Prus, 1989a), or a hooker and a "john" (Prus and Irini, 1988). Alternately, the goal may be the somewhat less tangible "deception" of the audience during a magic act (Prus and Sharper, 1991). The goal may also entail the carrying out of a social event, such as the marriage of one couple, or thousands (see Barker, 1984), or a weekend-long "club run" involving a few like-minded but potentially hostile (towards each other) outlaw biker gangs (see Wolf, 1991). The interesting thing for us to observe in any of these situations is how the participants manage the practical accomplishment of their goals through ongoing processes of interaction and interpretation.

Obtaining Results

At various points in their work, scientists will determine that some aspects of their goals have been achieved. This may refer to some interim "benchmark," or it may apply to some "milestone" with broader ranging significance. In either case, scientists make decisions about when and how to consider something to be finished or complete. It may appear to some that a scientific research project is complete when the experiment has ended, or the data have been gathered, but this is certainly not the case, especially with respect to knowledge production (see Knorr-Cetina, 1981; Latour and Woolgar, 1986; Collins, 1992). More likely, researchers move on to new phases that may involve analyzing data, assessing results, drawing conclusions, and determining the next step.

Pickering (1995) emphasizes the temporal aspect of scientific activity, and unlike many of the initiatives discussed in earlier chapters, the focus here is on the process rather than the product of scientific activity. When and how scientists obtain results will depend on the particular circumstances surrounding their work.

I can probably be productive within five or ten minutes of getting going, simply because I don't do "Betty Crocker" science, where I have to mix A and B and wait three hours to give me product C, which then you add to... and all of this takes place over the space of five days or whatever to get a product. My stuff; I simply have to set up a microscope and start examining things, or sit down at the dissection table and start dissecting things. So I can be productive, in terms of actual hands-on activity, within five minutes of coming into the lab. (B1)

This statement draws attention to the contrast between research procedures that require vastly different investments in time. There is no implication that faster is better, or even desirable.

The important point is that the amount of time allotted to this aspect of the research process may differ with each situation and context.

I'd end up at the end of the day with well over fifteen sheets of my clipboard filled with little coded notes - wg provision - that meant that the wasp painted white and green brought a beetle back to the nest, which would then be fed to the young. And so all the observations were done, and the next step was to put those observations into some sort of - well, I put them into a computer file - and then sorted them. So then at the end of it, I could go back and look at what this wasp, white-green, had done, and that's what I would consider my results. (B2)

My general field of interest is observational astronomy, and that involves collecting data bases at telescopes, bringing them home, and then analyzing the data bases. And right now, I have a data base that has gone through preliminary analysis, so I have the numbers and I've got the numbers in a variety of computer files, and I need to rearrange the numbers so I can examine them. (P1)

So, just as there is no sure way to determine how much time it will take to obtain a result, there is equally great variance with respect to what exactly constitutes a result. The diversity of paradigms and sets of practices that emerge within the numerous distinct communities of scientists include criteria for determining what members will look for to acknowledge a result.

It is very clear cut. We have purified the protein before, so we have some idea of the actual amino acid sequence in the protein, and we have the c-DNA, and so we've got the nucleotide sequence in that. From there you can convert the amino acid sequence into genetic code. It's as simple as that. If the sequence is the same then the compounds are the same. Whether it actually has the properties we think it might, that's another step altogether, but again there are specific assays that will tell us that. Either it has the property or it doesn't. (B3)

There's an experiment at Los Alamos, that we call the LSND experiment, for liquid scintillatory neutrino detector, and the experimenters claim to have seen at a statistically significant level, an oscillation of one type of neutrino, into another type of neutrino, or rather, one type of anti-neutrino into another type of anti-neutrino. (P2)

In other cases, scientists may not be able to determine whether in fact they have achieved something, because there may be a mismatch between the paradigm and what the scientist has produced, or there may be no recognized standard there to begin with.

When I was putting the data together, I really didn't know what I was looking for, and I spent one year with an erroneous hypothesis, until eventually I realized that it was all very simple. It was a critical mass phenomenon. I could almost give you a simple equation for it. I can't believe that I spent all of that time, but there was no precedent for this. (B6)

In some instances, obtaining results involves balancing diverse and complex theoretical and methodological considerations in an attempt to invoke some level of "closure" to the research process (see Pickering, 1992; Collins, 1992; Collins and Pinch, 1993). That is, even though certain agreed upon criteria may apply, it may be difficult to determine when and if these criteria, or some interpretation of these criteria, have been achieved in practice. In these circumstances, scientists judge when to declare that a certain activity is finished or complete. This judgement may be based on the acceptance of certain operational parameters:

So much of what I do involves calculations of physical properties of dilute gases and then going back to see whether we have a good description of its physical properties. And we typically look to get agreement within experimental error. We're looking at shear viscosity measurements, for example, and if those measurements are made to within plus or minus half a percent accuracy then we look for calculations that agree within those error bars. And we go back and adjust parameters in the direction potential to try to get that agreement as best we can. (C4)

In other instances, it may be based on an assessment of how other constraints, financial and otherwise, will be taken into account.

Because this experiment costs about a thousand dollars a month to run, we're just going to have to stop and publish what we have at some point. I will have to make that decision and it will be on my conscience. We just don't have the luxury of researching things in a thorough manner any more. (B7)

In both of these cases, as in others, the judgments that scientists make are based on their experience and understanding of the community within which they work. The "tacit knowledge" (see Polanyi, 1962, 1967; Collins, 1992) that the scientists accumulate is balanced against the

circumstances of the particular situation, and decisions are then made on how to proceed (e.g., continue, expand, shift, or drop a line of pursuit all together). In spite of all the apparently objective criteria that may be invoked, there is no substitute for direct hands-on involvement.

You know Polanyi – you know more than you can tell. If you've produced the data, you've got a ton of stuff going on in your head that you are not even aware of. So, when it comes to doing the theoretical stuff, you are better prepared than someone who is looking at someone else's data. (B6)

A large part of this "knowledge" is being able to anticipate the responses of the members of the particular group of which the scientist is a part, as well as potential responses from the broader community of scientists and outsiders. In this way, the intersubjectivity of the scientific enterprise extends beyond the micro-environment of particular situations and contexts, to include more macro-level interaction and interpretation.

In some more dramatic cases, moving on to the next step may involve moving into a new realm of scientific activity, with new challenges and new opportunities. Scientists' perspectives change over time, and with this, so may their level or type of involvement and their identity within the scientific enterprise. The way in which scientists perceive themselves, and are perceived by others, is tied into the kinds of activities that they engage in. Some may find stability and security in highly predictable surroundings, in which they can engage in familiar courses of action, while others may seek out uncertainty, in an effort to progress, or grow, or satisfy their curiosity.

Some people make their careers on staying in one very narrow field, and then they very quickly become world-renowned experts in this field, and if that's what they want to do, that's great, because we have to have people doing that. But for me, personally, after I've explored something and I've basically tasted what I needed to satisfy my curiosity, I want to move on to something else. (G3)

A sustained examination of this "career" aspect of scientific activity may provide significant insight into the processes of scientific change and progress.

Activities take place in time, and the process of obtaining results is tied in with evaluations of how time is spent. So, for example, the amount of time that students will spend studying is linked to the marks they hope to attain (Albas and Albas, 1984), or the amount of time a sales clerk will spend with a customer is linked to the actual selling of goods and services

(Prus, 1989a). Scientists, like others, are constantly evaluating and re-evaluating circumstances, and one of the key aspects of achieving scientific goals is determining how the available time will be divided up among the numerous activities and courses of action that present themselves. The scientists' "careers" will be constructed out of the decisions they make with respect to how they spend their time, and out of an assessment of the criteria that they use for determining when a particular activity is complete.

Disseminating Results

Just as the process of "getting ideas" was selected as the starting point for this examination of the activities of scientists, it may seem logical to see the process of "disseminating results" as the final step in scientific research. However, it is critical to emphasize that while this notion may be analytically convenient for the sociologist, it should not be assumed that scientists view their work this way.

The kinds of activities involved in the dissemination phase of the research cycle include: preparing articles for publication, presenting results at conferences, releasing information to limited audiences, releasing information informally to co-workers, colleagues, or other interested parties, and presenting seminars. Some researchers such as Latour and Woolgar (1986) may place a great deal of emphasis on the centrality of the "paper writing" process to the scientific enterprise, but the present project views it as just one among many activities that constitute scientific work.

Writing papers works out to be a pretty much a straightforward process. You sit down and the material and methods section is the easiest part – you know, the techniques you used and so on. Then, the introduction which consists of a literature review and of course a statement of the problem, and it's all straight-forward from there. (B3)

I'm working on a paper right now, and it seems to me it's just endless revision, until I either get sick of it, or I am pleased with it enough to send it off. (B1)

I guess to a certain extent, I'm a perfectionist when it comes to writing manuscripts, and it can sometimes take me an awfully long time to get them to the point where I'm happy with them. But then, it has been my experience that I don't have any trouble getting them published. (B8)

As with the process of obtaining results, the process of determining when the work of preparing a manuscript is “finished” is somewhat tentative. One of the most important elements here is determining what to do with the manuscript, in terms of selecting the appropriate audience for their work, not just with respect to the eventual end reader(s), but also anticipating the responses of the editors and reviewers that act as “gatekeepers” in the academic publishing arena (see Latour and Woolgar, 1986; Hull, 1988).

Well, I guess I use a diversity of scientific journals, from broad zoology journals to more specialized journals, like disease journals, depending upon the nature of the result. (B1)

The stumbling blocks are never the actual writing. The stumbling blocks are in having to negotiate with the co-authors as to which journal you are going to submit the paper to, and then what information you are going to include and what information you are not going to include. In one recent instance, my collaborator and I could not agree on which journal to submit to. He is more established than I am and so he wanted to publish the results in a more prestigious journal. I am a little more concerned with number of publications at this point, so I selected a less prestigious and more narrowly focused journal. Unfortunately, the article was rejected, and so we had to submit it again, this time to the journal I had selected. That process probably delayed publication by up to eighteen months. (B3)

In some cases, the activities centered around performing the research and those dealing with the presentation of the results of that research to the community of scientists may be quite removed from each other. For example, it may be a question of finding an appropriate opportunity to publish:

There's the project on younger rocks that I did in the 70s, which didn't get published because I lost the momentum and there really wasn't much interest in it at the time. Now the government's got a lot of interest and has restudied the same area, and we will probably be publishing... so it's not lost, it was just... well, if it has waited sixty-five million years, it can wait another two or three. (G1)

Well, I think most people would think that a year is a reasonable goal, certainly by two years. But, I have data sets that are four or five years old that are in some intermediate stage, and I haven't been able to get around to them. (P1)

Considerations of broader situational and contextual elements, to some degree, will shape the actions that scientists take regarding some particular aspect of their work. Similarly, there are events about which the scientist can have no prior knowledge that may alter, compound, or

negate some previous course of action. For example, a scientist may fail to acquire adequate funding to continue a line of research, or the interests of a graduate student or colleague may require a scientist to shift their focus, at least temporarily.

In addition to the professional journals, elements of the scientists' work may appear in textbooks that are designed to reach a broader audience that will include non-specialists and students:

I did my Ph.D. on deep water carbonate sediments, and I published a half a dozen articles on various aspects of that in the last few years, and then I was asked by my former thesis supervisor and another colleague, who were putting together a book, to write a chapter on these things as an overview. (G3)

This aspect of communicating about science, whether to the general public, to colleagues with similar interests, or to potential new members of the scientific community, is particularly important for demonstrating the value of the generic social process scheme. All communities and human groups communicate in order to establish and maintain membership, and GSPs provide a way to examine these activities in a transsituational and transcontextual manner.

In other situations, the audience for the results may be very narrow and selective. In some cases, the requirements of more specialized audiences may in some ways shape the direction of the research.

Over the last couple of years, I have been doing more diagnostic work for the government, so that's a slightly different type of dissemination of results. They want to know: What is this? Might the birds have died from that? Or, they want specific questions answered, as opposed to the broader picture that would appeal to a scientific journal. (B1)

Under certain circumstances, scientists may also present interim results of ongoing research projects. These situations tend to be characterized by a "friendly" audience, made up of other scientists who either share or are sympathetic toward a particular worldview:

At the smaller (specialists only) meetings, people do tend to present results that they have just collected in the lab, which are really "hot off the press," and they're often even sometimes results that they will never publish. So, it's more a forum for kind of an informal discussion, where people will present things that are a little unusual, but are not completely tied down. (C5)

I presented some of the interim steps -- like where are these cells and what are they doing -- at the Canadian zoology conference, and I talked with a few people, and I ended going to one guy's lab to do some work directly related to the project. (B5)

From this experience, scientists may collect new ideas or gain new interpretations, and may even glean a particular piece of information that will propel their scientific careers in new directions.

Thus, we can begin to observe how the entire process of “doing science” loops back on its self and becomes a continuous series of activities that overlap and interrelate in a variety of ways.

Just as one paper is nearing completion, they often cause more questions to be asked. So, it's obvious what my next project is going to be, and they seem to feed off each other like that, and it's self-sustaining. (B8)

As mentioned earlier, the activities that take place in the accomplishment of science do not necessarily follow a linear path, nor are they discrete events. Rather, at any point in time, scientists are engaged in a matrix of activities that reflect a multitude of objectives, commitments, and alliances.

Summary

The data presented in this chapter describe the activities performed by academic research scientists through a “natural history” or cycle of: getting ideas, assessing feasibility, getting prepared, implementing plans, obtaining results, and disseminating results. As mentioned, this cycle of activity represents a heuristic device that allows us to get a handle on the continuous stream of action that constitutes the scientific enterprise. Similarly, the presentation begins to show that the activities of gathering, interpreting, and presenting data are intricately interrelated in such a way as to highlight the processual and ongoing nature of every aspect of human group life. In other words, there is continuity between the process of study and the processes being studied.

This chapter is central to the whole project in that it provides an illustration of all three of the stated objectives. Taking the various sections individually, or as a whole, it is clear what a symbolic interactionist approach to science looks like. Scientists are allowed to speak for themselves, as they describe the kinds of things they take into account on a daily basis, and how they respond to those things. Similarly, the evidence demonstrates the value of this approach to

expanding our understanding of science. We gain a direct appreciation of how scientists spend their time (e.g., building equipment, collecting specimens, writing articles) as they work towards the goals they have set for themselves, and as they respond to the various challenges and contingencies that arise. Finally, new details about what takes place in the practical accomplishment of science emerge. For example, we learn about where the ideas for scientific research come from (e.g., out of the blue, from farmers, from past accomplishments).

In contrast to other approaches to science which emphasize the products (e.g., knowledge) of science or the structures (e.g., norms, institutions) within which science takes place, the interactionist approach is naturalistic in that it deals with the day-to-day practical accomplishment of goals by human agents as they interact with others and interpret the world around them. This emphasis on *how* scientists deal with the "obdurate reality" (Blumer, 1969) of their life-worlds sets this approach apart from alternate approaches to the social study of science. For instance, Robert Merton's conclusion that the norms of science govern the behavior of scientists is based on data from the "highly select writings of the rare, great scientists" (Mitroff, 1974:15). No effort is made to indicate if, when, or how these norms are taken into account by the vast majority of scientists on a day-to-day basis as they carry out their research. Even Merton's critics (e.g., Mitroff, 1974; Mulkay, 1976) are more concerned with establishing the fact that Merton's norms are not unique, and that certain sets of what might be called "counternorms," that reflect the "ideological differentiation" (Robbins and Johnston, 1976) of scientific communities, are just as likely to characterize the behavior of scientists. We have yet to see how, or even if, these norms function in the daily activity of run-of-the-mill scientists.

One of the principal values of the interactionist approach to science, then, is that it does not attempt to impose some method of study that supports some *a priori* theoretical conception of what science is. Similarly, it does not try to fit theory to the kinds of results produced through the application of some orthodox method. Rather, through the use of concepts and the open exploration and inspection of what scientists are doing, it allows the researcher to respect more fully the nature of the life-world being studied. Here, the generic social process scheme is particularly important, in that it provides the conceptual framework within which the field research

takes place, the analysis is carried out, and the findings are presented. This continuity extends to allow researchers to study an individual, a small group, or a large number of subjects in a variety of contexts and situations, without having to alter theoretical and methodological frameworks. This overall approach also allows the researcher to draw attention to the idiosyncrasies of some particular group, while at the same time providing data for a more general appreciation of the nature of human group life.

Perhaps one of the most important things that we learn about science, by taking this approach, is the value of concentrating on what scientists do. To view the scientific enterprise as emerging out of the intersubjective accomplishment of a group of practitioners allows us to examine many aspects of the day-to-day activities of scientists, that may be glossed over or considered too mundane to be mentioned, let alone investigated, in other approaches to the social study of science (e.g., those that emphasize social interests, or an organizational view of science). By interviewing a number of scientists engaged in separate research programs and different disciplines, we gain insight into the "multiplicity and heterogeneity" (Pickering, 1995) of the subcultures that constitute the scientific enterprise. The kinds of processes and activities mentioned here provide the basis not only for the sustained examination of any one of these in a single situation or context, but form the basis for a more coherent examination of all aspects of the scientific enterprise, and any other aspect of human group life. The following chapters provide more evidence in support of the concept of a social study of science as expressed here, by examining a selection of specific activities carried out by scientists.

Chapter Seven

PURSUING FUNDING

Following a brief review of some of the literature in the social study of science dealing with issues around providing financial support for the scientific enterprise, this chapter presents data on activities engaged in by scientists as they pursue funding for their work. Parallels are drawn throughout to findings from other qualitative studies of science and to themes that arise in the literature with respect to the relationship between the availability of resources and what science gets done, and how it gets done. Parallels are also drawn to the activities engaged in by members of other groups, particularly outlaw biker gangs. The data presented here also provide the basis for an alternate conceptualization of what it means to say that science is socially constructed.

Context

One of the objectives of the present project is to draw attention to some aspects of the scientific enterprise that have not received adequate coverage to date in social studies of science. The issue of financial support for science, whether through government agencies or private industry, is something that nearly all of the scientists interviewed for this study indicated is crucial for their work. Also, they indicated that the activities centered around financing their research take, what they considered to be, a disproportionately large amount of their time.¹ Part of the common perception of the scientific enterprise is to view scientists somehow as passive recipients of public and private money that will be used to further knowledge and benefit humankind.² However, scientists are now, and possibly always have been, in the situation of having to pursue actively support for their research in highly competitive environments. My

¹ Martin Kenney (1986:18) indicates that as much as 30 percent of a scientist's time can be devoted to the grant application process. The scientists I interviewed indicated that they spent from 5 to 25 percent of their time on funding-related activities, with the average amount being about 10 percent.

² A recent report from the Natural Science and Engineering Research Council of Canada (1997:68) indicates that just over ten billion dollars was spent on natural science and engineering research in Canada in 1996.

study of the scientific enterprise should give us some insight into how scientists deal with this situation and how the actions they take in response to this situation affect the way they do science. The remainder of this introductory section examines some of the research that has been done on the financing of the scientific enterprise, and outlines the kinds of processes that scientists engage in as they pursue funding for their own research activities.

Leslie Stevenson and Henry Byerly (1995) offer some historical and contemporary examples of the correlation between scientific activity and the availability of funding. While their research deals primarily with exceptional cases (whether based on fame or infamy³), they describe a sequence of changes in the relationship between scientists and their benefactors that is instructive for the present study. They observe that, from the birth of "modern science" up to this century, many scientists relied on their own wealth or the support of wealthy patrons in order to carry out their research. For example, Cosimo de Medici appointed Galileo to the post of "philosopher and chief mathematician" to the city of Florence, with a generous stipend and no official duties. The rise of the universities led to a situation where a more consistent system of academic support opened up science to those with desire and ability, who may not otherwise have been able to gain support for their work. One well-known benefactor of this system was Sir Isaac Newton who, while in his early twenties, had the good fortune to be elected to a lifetime professorship in mathematics at Cambridge University. However, this example should not be taken to imply that universities have a long history as research institutions. Yves Gingras (1991) provides an account of the institutionalization of scientific, particularly physics, research in Canada that took place for the most part in the early years of this century, as universities expanded their role beyond teaching. More recently, however, broader economic pressures have created an environment where even within the academy there is a thrust towards applied research and the establishment of links with commercial interests.

³ For example, the authors describe how the University of Utah decided to invest five million dollars to establish a cold fusion research center based on the work of Fleischmann and Pons. The center closed two years after its inception, and the university president took early retirement, rather than face the political turmoil associated with this risky venture.

This development has meant a move away from an emphasis on the fundamental production of knowledge, towards a notion of profiting from science, in terms of more business-related measures. Historically, scientists and institutions have responded cautiously to this kind of narrow, practical, and profit-oriented rationale for scientific activity. Stevenson and Byerly (1995:143) cite a letter from Marie Curie, wherein she indicates that even if the discovery of radium "has a commercial future, that it is an accident by which we must not profit." Similarly, when Ernst Chain recommended to Oxford University that they patent their method for the large-scale production of penicillin, officials were appalled to think that anyone would dare to bridge the gap between pure academic science on the one hand, and applied industrial science on the other. A radically different view can be seen in the case of Alfred Nobel who, through his role as an "armament producer without national allegiance," and by virtue of his holding no less than 355 patents, was able to amass the fortune that allowed him to establish the prizes that bear his name.⁴ It is a curious paradox that an individual who profited so greatly from scientific research would turn those profits back into an effort to maintain the "purity" of pure science, as if in an effort to exonerate himself from his past.

In his study of the relationships between commercial and academic interests in the field of biotechnology, Martin Kenney (1986:17-18) states that postwar science bureaucracies allocated funding competitively, in the hope of advancing scientific knowledge.

The majority of the grant proposals were evaluated through the peer review process, a system that encouraged the most successful professors to recruit more graduate students and postdoctoral researchers to work on their projects, thereby expanding their empires. For these scientist-entrepreneurs to continue to receive funding they found it necessary to direct their research toward topics currently in vogue. Scientific achievement was judged on the basis of grants, prizes (Nobel, Lasker, Merck, Novo), and publications. Having a large research staff made it possible to produce more results and secure greater recognition. The key was to ensure that grant money continued to flow into the laboratory. A professor could no longer remain alone and aloof in his laboratory because large sums of capital for equipment purchases were required to remain in competition.

As this statement indicates, while it would be difficult to determine whether in fact scientific

⁴ The results of a recent survey of 2,052 academic scientists at 50 universities (see Blumenthal et al., 1996) indicate that high levels of industrial support for research result in less academic productivity and no significant increase in commercial productivity among scientists.

knowledge grew through this process, there can be no question that the scientific enterprise grew. Kenney (1986:29) goes on to argue that with the increased involvement of corporations in the funding of science the universities came to be viewed as: (1) the source of a trained labor force, (2) the site of research activities that could be turned into profitable ventures, and (3) legitimators of the social and economic structure of, in particular, the United States.

Support for all three of these points can be found in Chandra Mukerji's study of the Scripps Institution of Oceanography. She (1989:6) argues that:

Soft-money scientists constitute a reserve labor force in the sense that they are supported by governments and industries so their honed skills will be available when they are needed (by, for instance, the military in case of war, by industry in case there are major changes in the direction of the economy, or by the medical community if there is an outbreak of some new and threatening illness).

Similarly, in their analysis of the exceptional nature of the scientific enterprise, Bimber and Guston (1995:558) state that "science is a uniquely productive investment of current resources for future gain." All of these comments are consistent with an instrumental view of science, and a teleological view of the evolution of society. In other words, spending money on science today will ensure a better future.⁵ The underlying assumption here is that there is a positive correlation, if not a direct causal relationship, between the level of funding and the amount of scientific knowledge produced.

In a special issue of *Social Studies of Science*, Susan Cozzens (1986) brings together a series of four papers that examine the relationship between government funding and the growth of scientific knowledge. Steven Cohn (1986) attempts to construct and test a model for determining the impact of changes in funding levels on progress in a particular branch of mathematics (algebraic and differential topology). Contrary to his expectations, and yet consistent with some other studies (see Menard, 1971), Cohn finds little evidence to support the notion that knowledge growth is in any way dependent on specific levels of support. Further, Cohn provides support for the conclusion reached by Cole and Cole (1973), that major cuts to

⁵ A recent joint statement to Congress by the presidents of 23 major U.S. scientific organizations (see Jones, 1997) calls for a seven percent increase in the federal research budget for fiscal year 1998. The rationale includes the four national goals of economic competitiveness, medical health, national security, and quality of life.

government research budgets would have little effect on the progress of science. In a study of research on sudden infant death syndrome (SIDS), Karl Hufbauer (1986) finds that, even though there was a surplus of targeted funding for research on this problem in the postwar period, there is no way to link the one major breakthrough in our understanding of this phenomenon to the resources available for studying it. In fact, it appears that funding levels increased only after the research activity was well underway. Hufbauer's findings are consistent with the Matthew Effect (Merton, 1973:439-459), in that money tends to be a reward for past performance, rather than a contributing factor to new ventures and initial success. In a third study, Joseph Tatarewicz (1986) examines the resurgence of planetary astronomy in the United States after Sputnik, and discovers that scientific advances are more directly linked to the availability of instruments (telescopes and so on), than to the direct funding of research initiatives.

Finally, in his examination of ionospheric physics, a branch of science that received support from a wide variety of sources including twenty-six different offices within the military alone, Stewart Gillmor (1986) finds that no reliable conclusion can be drawn about the relationship between funding and the growth of science. As part of his study, Gillmor interviewed twenty experienced scientists, who had been involved in science/government relations. Consistent with the findings of Tatarewicz, Gillmor's informants all stressed the importance of access to adequate facilities for knowledge growth to occur. They also made three other points, more directly related to pursuing funding. First, they spoke of "funding pluralism" (1986:121), which means that the most successful fundraisers, among scientists, are generally those that tap into multiple sources. Second, they noted that funding sources tended to differ with respect to whether they funded particular people (e.g., prize-winning scientists), projects (e.g., Human Genome Project), or problems (e.g., SIDS). Finally, they indicated that the funding process displays a certain "bi-directional" character, whereby scientific knowledge flows up from the scientists, while bureaucratic knowledge flows down from the agencies. In each funding situation, there is uncertainty over the extent of the "knowledge" gap, or overlap, between those allocating funds and the potential recipients. One possible outcome of this situation is that

scientists who are very successful at acquiring funds may be so because of their bureaucratic skills and not their scientific ones (narrowly defined).

Turning to the laboratory studies that were examined in Chapter 3, findings appear to support the notion that funding serves to maintain the scientific enterprise, irrespective of whether it contributes to the growth of knowledge. In examining the careers of scientists, Latour and Woolgar (1986:198) talk about "cycles of credit" that can be compared to a "cycle of capital investment." In other words, they argue that scientists write papers and construct objects in order to establish credibility among their peers that can then be reinvested to engage in the further pursuit of credibility. Knorr-Cetina (1981:87) relates that the scientists she studied emphasized the importance of the "art of writing a grant proposal." This "art" involves balancing the need to be concrete and specific about the proposed research project, while at the same time not giving away enough information for others to "steal" the ideas contained therein. Sharon Traweek (1988:3) relates how E.O. Lawrence was able to acquire the vast amounts of money needed to construct the first major particle accelerator through his "public appeal that physics should be funded because it ultimately enhances the public good." She (1988:101) goes on to argue that some scientists will reach the point in their careers where they become statesmen, who "no longer actually do physics; they recruit students. They get money for the lab; they get money for science, they attend to the public understanding of science."

As was mentioned in the first chapter, it is surprising that more explicit details of how scientists deal with issues related to funding on a day-to-day basis are not forthcoming in these studies. Part of the reason for this, is that these studies still maintain a broader institutional perspective on issues such as funding, rather than carrying through with the more internally-consistent microsociological approach to studying science that these studies advocate. So, for example, Latour and Woolgar (1986:73) indicate that, by dividing the annual budget of the laboratory by the number of papers produced, they were able to determine that the cost of producing a paper went from \$60,000 in 1975 to \$30,000 in 1976. This fact may provide us with a measure of the cost of knowledge production, and it certainly points to changes that must have occurred in what took place in the laboratory in these two years. What we do not learn, however,

is how this situation came about, or how the scientists engaged in and interpreted the events tied in with these changes. For example, did the scientists increase the amount of time they spent on filling out grant applications, or did they perhaps find a new source of money? Latour, who was in the lab for nearly two years, was ideally situated to observe and inquire about such matters, but instead he and Woolgar draw upon citation counts to analyze the broader scientific (institutional) impact of the papers produced and to examine the economic efficiency of this particular laboratory.

The present project is not concerned with establishing people's motives for funding the scientific enterprise, nor is it concerned with understanding the content and application of various government or industrial policies regarding science. Rather, the focus is on the activities that scientists engage in as they attempt to acquire financial support for their research and the ways in which they deal with the perspectives and actions of those in a position to provide money for science. Through talking with scientists, a number of processes emerge to facilitate the exploration and communication of how the activity of pursuing funding is carried out on a day-to-day basis. These processes include:

Identifying Sources of Funding

Preparing Grant Applications

Entering Partnerships

Marketing Services, Equipment and Materials

Making Adjustments

An examination of these activities provides an opportunity to gain an appreciation of some of the external constraints on the activities of scientists, while still concentrating on the specific details of the everyday accomplishment of science. Without implying that those who control specific sources of funding are in a position to dictate the activities of specific groups of scientists, we want to see how scientists deal with the matter of financing research projects. There is no question that funding agencies play an important part in the accomplishment of science, and there is no question that science "gets done." What we would like to have a better handle on is what it is exactly that is getting done and how, and a crucial part of this is the pursuing of

funding. By examining the activities that scientists engage in doing this, attention can be focused on the dynamic and processual nature of funding scientific activity, thus improving our level of understanding of the scientific enterprise in general. At this point it is important to stress that, to date, no one has ever attempted to delineate the various activities undertaken by scientists, and the options they really face, in securing adequate funding for their research and their careers. The first task, in other words, is simply to advance the descriptive adequacy of our understanding of this vital, yet under-researched, sphere of scientific "work." As will become clear, the detailed and unprecedented delineation of the social processes involved in being a successfully-funded scientist points to the need for even more exacting qualitative research into the micro-structural conditions and dynamics of how funding in "regular" (and not just extraordinary) science actually occurs.

Identifying Sources of Funding

The days when you could start with five or ten thousand dollars are gone. Big science demands big money, and big money is hard to find. (C2)

While not all scientific activity, whether inside or outside of the academic setting, is "big science" (see Price, 1986; Galison and Hevly, 1992), there appears to be a general perception among scientists that research is costly, and that it is becoming more expensive all the time. As such, scientists often find themselves devoting considerable parts of their working time to acquiring the necessary funding to carry out the kinds of activities that they think are appropriate. This seems particularly relevant in the university setting, if they hope to recruit and retain graduate students.

There are a lot of subtleties in the university life. Having graduate students has advantages politically, having graduate students helps you to get money for graduate students. (G2)

The environment for graduate students is very good, but you've got to spend huge amounts of donkey-time trying to get research grant money. (C1)

While attracting students may be only one of a number of activities linked to the process of pursuing funding, it is an integral part of the scientific enterprise.⁶

⁶ It is important to note that funding agencies also fund students directly. So, for example, in 1996 NSERC provided 40 million dollars in scholarships to graduate students in the natural sciences and engineering (1997:47).

Simply understanding or accepting the fact that funding is an important aspect of accomplishing science, however, does not help us appreciate the work of identifying potential sources, or carrying out an array of related activities on a day-to-day basis. What is required is a mechanism that both draws attention to the peculiarities of the funding process, and demonstrates how funding is linked with other elements of the scientific enterprise. In order to communicate effectively the complexity of what takes place here, it will be helpful to examine this initial phase of the funding process through these activities:

- Dealing with Government Agencies
 - Financing a Career
 - Satisfying Mixed Agendas
 - Responding to Innovation
- Identifying Industrial Niches
- Looking for Alternatives
 - Tapping in to the Broader Community
 - Looking within the Institution
 - Settling for Less
 - Bowing Out

Dealing with Government

In many nations, government support for scientific research is part of the broader social support system for education, as well as industrial and military development. In the academic setting, identifying sources of funding can be viewed as a foundational step in “financing a career.” Scientists are discovering more and more that they have to “satisfy mixed agendas,” as the administrators of granting agencies alter their perspectives on how money is to be divided up. In a similar fashion, new alliances between government, industry, and educational institutions place scientists in the position of having to “respond to innovation” in the expectations and worldviews of these new groups. A large part of identifying sources of funding, then, is identifying the goals and objectives of the people that manage those sources.

Financing a Career

As new members enter academic science departments, part of the perspective that they acquire accentuates the critical role of pursuing funding in order to take advantage of opportunities to carry out chosen avenues of research, and to achieve success with respect to

promotion and tenure. In Canada, NSERC represents one of the most centrally recognized sources of funding for academic research in the basic sciences.⁷

The Natural Science and Engineering Research Council of Canada (NSERC) is our main supplier of money in Canada, and they characterize a number of different kinds of grants. One is the individual research grant which we apply for in the autumn and which we get for three or five years, as the case might be. That's becoming harder for new faculty even... not every new faculty member gets one. Used to be that everybody got one, but even new faculty now are finding that they may be turned down, and if they are that's really serious because they can't get started, and hence they can't get tenure, and they can't get graduate students, and so it affects their careers quite markedly. (C1)⁸

Up until this year, I had not been successful in obtaining NSERC funding. The proposals were good and they received good evaluations, but because I was just starting out and I was at a small institution, the examiners weren't confident that the work would get done. (B3)

So, even though there exists a well established and recognized source for the funding of academic scientific research, it does not follow that all scientists will have access to this source.

Even among those scientists that are successful in obtaining this kind of funding, there are two limitations that not only help to emphasize the contextual and situational nature of science, but clearly demonstrate the continuous ambiguity with which scientists deal along the way. The first of these is a temporal constraint, which requires the scientist to repeat the funding application process at specified times in the future (see Knorr-Cetina, 1981:86). The second constraint is related to the match between the amount of money requested by the researcher and the amount actually given out by NSERC.

This past year I had my NSERC grant proposal come up for renewal, and I renewed it and it was granted and they gave it to me this time for five years, so I don't have to apply again for five years. They only gave me 58% of what I asked for, and that would be not too bad, but in a sense I can't run the operation I'm running on that amount of money. So, I need to find other sources of money. (C1)

When the proposal was submitted, we asked for enough money to hire two graduate students, and even though I thought that was a bit excessive, my collaborator wanted that in there. So, we only got half of what we asked for, and therefore we won't be hiring two graduate students – perhaps not even one. We are going to have to stretch the funds to accomplish what we said we were going to. (B3)

⁷ The Medical Research Council (MRC) is similar to NSERC, and while it primarily provides funds for medical research, there are a number of instances where research projects in the basic physical and life sciences receive MRC funding. Organizations similar to NSERC and MRC exist in the United States, Great Britain, and many other nations.

⁸ Slightly more than half of new applicants are successful in obtaining NSERC funding (1997:20).

In these cases, scientists face the challenge of altering some planned course of action in their research, or exerting additional effort to make up the shortfall in the funds.⁹

Since funding is closely related to the goals and interests of the people involved in administering that source, success in obtaining funds has something to do with the ability of the scientist to adapt to the changing nature of the environment in which science takes place. In other words, government agencies in particular will adapt their funding strategies to reflect their changing perception of the demands and expectations of their various constituents.

Back in the old days, it was easier to do research because there was more money and less competition, and you could explore new avenues. Now, even granting agencies that allegedly support fundamental research, want to see an applied component. (C6)

Well, it has changed a lot in my opinion. Many, many years ago, they funded only fundamental research – idea driven things. They have evolved into putting more and more money into practical aspects, if you like, in the sense of these “strategic grants,” which have siphoned off money from the fundamental pool. (P4)

Not only are scientists involved in the more day-to-day management of their commitments within a specific institution (teaching, administration, research), but an important part of their “careers” and identities is linked to their taking into account the broader perspectives of the groups that they rely on for financial (and other) support. To treat the process of pursuing funding as somehow non-scientific, then, is to misrepresent the “obdurate reality” (Blumer, 1969) of the ongoing and emergent situational complexity faced by the scientists.

Satisfying Mixed Agendas

Moving to another level of complexity, scientists are often in a situation of having to present their work in ways that satisfy the requirements of two or more different groups. The somewhat recent development of “strategic grants” represents initiatives on the part of government and industry to maximize their investments in research through consolidation of their goals.

NSERC, in their wisdom, has created so-called “strategic grants” which

⁹ The size of an average research grant from NSERC is \$25,000, a figure that has remained constant over the last ten years (1997:18).

are applied for at this time of year (spring), and you hear about the result at the end of October. They're for supposedly strategic areas of concern to the country, such things as: industrial materials, energy, food, agriculture, etc., and there's a booklet on that, and these grants require you to have an expression of interest or a statement of interest by potential users of the results. This is both good and bad. It brings you into contact with industrialists, but you have - in the bad sense - you have to go find it, generate it, and so on, so I've been talking on the telephone, and sending off courier bundles, and sending off faxes to about four different groups who I feel will be able to support this particular proposal. And in fact they're supporting it, but as usual, with deadlines and so on, and all of this creates a certain amount of pressure. (C1)

Within the context of these grants, the onus is on the scientists to attend to not only specific areas of national (and corporate) concern, but also to indicate whom the potential beneficiaries of their research might be. They may have to gain the support of particular external groups, and in the end convince yet another group (the funding agency) that the work proposed can be accomplished. This intricate array of activities presents the scientist with a number of challenges that sometimes can only be met with the cooperation of many different people. In these cases, a great deal of additional effort may be expended in trying to establish and coordinate these associations. As Traweek (1988) indicates with her notion of a scientific "statesman," in pursuing strategic grants, scientists commonly take on multiple roles (salesperson, marketer, negotiator, lobbyist).

They have a category called "strategic grants," which is mission-oriented research - to use a catch-phrase. It has to have a specific purpose, that is in alignment with some listed criteria - some areas of research that the government would like to see undertaken. And there's been increasing pressure over the years in that direction, to explain your research in practical terms, to justify yourself to the public. There's been increasing pressure in that direction - there's less and less satisfaction with having ivory tower professors working away at what they want to do, and the pressure to do more and more applied work is increasing, because it is easier to explain to the public what people are doing. You witness this in the press over the years. Once in a while somebody gets on a bandwagon about money being thrown away for researchers, and if you're not in that field of research, it does often look ridiculous. (C6)

Responding to Innovation

Another variation on government funding schemes that links scientific research with industrial concerns brings together separate research sites and potential users to form a complex

network within which funds are distributed. These kinds of innovative initiatives provide both opportunities and challenges for scientists.

The government has recently come up with these new "centers of excellence," which are a much poorer way to selectively hand out money. I am connected to one of these centers, and it has given me two or three times the money that I've been able to get out of NSERC, but it is a very painful process because of the amount of paper shuffling and proposal writing involved. It destroys a lot of working time. Those of us who are associated with these centers have gotten lots of money out of it, and yes, probably we've been able to accomplish more research, and been able to do more challenging things because it gives us better facilities. But, one wonders whether it is worth it because of the extra time writing and rewriting proposals, and the infighting that takes place. The money comes from the government, and these centers are run of necessity by little cabals, and a group of people who are going to be major recipients of this money then get to decide who amongst themselves, and the other people in the network, are going to get the money. So, political infighting gets unpleasant. (C6)

The ambivalence expressed towards this process partially reflects the amount of work that must be expended in establishing and maintaining the network of associations among the various groups that constitute the centers. Frustration may arise over the uncertainty of outcomes from this kind of arrangement, and this may in turn directly impact research programs and careers.

One of the problems is that we may get our throats cut because, as far as I'm concerned, our major product is people, and we will give the industry the kind of people who can do the best current science, and then they'll go to these companies and help them to find the best ways to produce things. Sometimes, we produce tools or techniques in the laboratory that can be marketed, but if a student gets hired by a company that then brings out the product, we could lose. (C6)

Identifying Industrial Niches

As an alternate to government agencies, scientists often try to secure funding from various industries that may have some interest in the results of their research. Even within this seemingly more limited scope, scientists who seek support from a particular industry may take into account information on issues that concern a variety of groups within society.

It used to be in the '70s that funding was available from (company name) and from various oil companies, who were really into great exploration programs, and needed this technical work done. But, when the bottom fell out of the oil industry that funding dried up. (G2)

Determining what exactly to take into account when seeking funding from industry, though, is not an automatic or predetermined process. A great deal of (scientific) research is involved in the process of deciding what kind of (scientific) research to carry out and to pursue funding for.

Nobody will come up to me and say, "Look, these are the directions that the whole system is taking." You have to read the trends, and you read the newspaper just to see what is hot and what's not – what people are interested in. And, if you decided to apply to look for the cure for cancer, and I am sure nobody needs to tell you that that is going to be a hot topic, and you may find somebody willing to give you some money, especially if you have a good research record. On the other hand, if you were trying to do something that was really interesting – really interesting from the scientific point of view – and if there was nobody out there interested in it, no matter what it was, you would be hard pressed to get support. (G3)

On one hand, then, the scientist can try to jump on some particular "bandwagon," such as the one identified by Joan Fujimura (1988, 1996) around oncogene research, where commercial ventures or government agencies have taken popular public interests (regarding health, for example) into account.¹⁰ On the other hand, the decision to engage in a program of research solely determined by individual preferences or concerns may jeopardize the scientist's chances for long-term success (see Knorr-Cetina, 1981:74-78). In other words, there is evidence to support the notion of the "Matthew Effect" (Merton, 1973), whereby those scientists that regularly obtain funding will continue to do so, while those that never have, likely never will. Linked to previous success in acquiring funding is what is perhaps the most important measure of research activity, the journal article.

I was interested in this process, and because it involved learning about things that I hadn't learned about before, I surely lost funding because I wasn't productive. The community doesn't particularly care why you don't publish, they only care whether or not you publish. (P4)

The decision to take advantage of an opportunity and align your research interests with the economic or political interests of some other group, however, is not the same as sacrificing or abandoning personal perspectives or sets of goals within that framework. For example, research funded by the military is not by definition military research (see Smit, 1995).

In the past I've had some support from the Office of Naval Research, but that's dried up, and the US Army, but it is not funding anything. It's

¹⁰ Paul Rabinow (1996) provides an interesting account of the mingling of science and business, with respect to the discovery of the polymerase chain reaction (PCR) method for synthesizing DNA.

not that this is anything military. It's not a military project. It's a project to develop better batteries, and the military have an interest in that as well, but everybody who goes out at night has an interest in this, and wants to see by their flashlight. (C1)

So, while the interests of others are taken into account in determining a particular course of action, this is not the same as saying that scientific research ideas or programs are "determined" by those interests. Rather, the intersubjective accomplishment of science emerges out of the interpretive and interaction processes that take place in the context and situation within which the scientists find themselves. Scientists, and others, respond to objects on the basis of the meanings that those objects have for them. This is true of all social life, but that is precisely the point I am trying to make here. Contrary to the approaches taken by some philosophers and early sociologists of science, however, we need to "show" (and not just assume) that science is not very "exceptional," and this means that we need to study science in the same manner (i.e., with the same sociological methods) as we study the rest of social life.

Looking for Alternatives

Apart from government agencies and industrial sources, there are a number of alternate solutions that scientists can pursue to deal with the problem of financing their work. Upcoming sections provide some details of how a variety of these alternate arrangements (entering partnerships, marketing services, equipment and materials, and making adjustments, respectively) are carried out in practice. The situations described below, however, draw attention to what might be considered exceptional cases, in that these solutions may only apply to a limited number of scientists, in special circumstances. Whether this is in fact the case is an open empirical question that can be clarified only through more extended research into this area. At the same time, however, it may be the case that the activities described here are similar to those carried out by members of other human groups.

Tapping in to the Broader Community

Members of certain scientific subcultures carry out research projects that involve elaborate facilities that may be financed by several institutions or even several nations. In these

cases, financial resources are allocated to the facility itself, rather than directly to scientists. In this way, the scientist is tapping into the resources of the broader scientific community, thus reflecting to some degree shared worldviews and similar goals.

Well, an astronomer gets indirect grant money that other physicists don't see, because the James Clerk Maxwell telescope and the Canada-France-Hawaii telescope have capital costs of, well, CFHT cost about \$30 million to build, that was the Canadian contribution, and JCMT is probably in a similar ballpark, no probably less for that one, but then there are ongoing costs every year to run those telescopes. That's another hidden grant that astronomers have. Astronomers all understand that they're being supported through this grant level. (P1)

In cases like this, it may be difficult to determine on a per scientist basis how much money each one is receiving in support of their own research projects. What is important is that there is a sufficient level of support for a broader range of potential research using equipment like this, to ensure that these facilities are built and maintained (see Traweek, 1988; Galison and Hevly, 1992; Pickering, 1995).

Looking within the Institution

The emphasis thus far has been on identifying sources of funding for academic research that are largely external to the university environment, but research funds are available through many internal funding programs as well.

The university has a limited amount of money that it uses to encourage research projects. People can apply for this in the fall or the spring, and a university-wide committee decides who will get what. We usually try to divide up the money fairly evenly to help people get started on something, or finish something off for publication. It usually amounts to about a thousand dollars. That's all. (director of research)

I received one of these internal grants, and I got \$3500, which isn't very much, and you have to ration very carefully how you spend that. I mean the most you can get is five thousand dollars, and you're not going to get that because of the sheer number of people applying and the fact that the pool of available funds is shrinking. (B3)

In some of these cases, there are circumstances where research funds can be found through compromise and adjustment.

Well, because of my secondment (to administration), the department gets money to fund the teaching of one course a term for me, and I end up teaching these courses, and taking the money as research money. So, I'm getting that money channeled that way. So, this type of minor funding

comes through. (G1)

Here, consideration of the separate agendas of teaching, administration and research are all brought together to give rise to a solution that, while increasing the workload of the scientist involved, still satisfies the requirements of those concerned.

In one situation, the manner in which internal funds had been managed led to a situation where some quick action was required to maintain an existing relationship between a particular department and the institution as a whole.

The chairman of the department walked into the lab and said that we have \$30,000 left in the budget that we have to get rid of by tomorrow or it goes back to the university. And, he asked, "Can you do something about that?" (B5)

Settling for Less

Some scientists decide to modify their research agendas in such a way as to avoid spending too much time on money matters.

I had to do something that was relatively inexpensive, and the reason it had to be inexpensive is because research funding is very hard to come by, and if I elected to keep things fairly academic, my chances of getting industrial contracts to do them are essentially less. If I've got the option to carry out academic research, and that's what I want to do, I don't want to prostitute myself to do projects I'm only half interested in, just because the money is available. (G3)

In such cases, not only do scientists exclude themselves from consideration by a number of potential funding sources, but it also becomes obvious that the way in which these scientists conceive of their research is tied into conceptions of several elements that might be considered less important, or even non-scientific, by others.

The major grants I've received lately have all involved molecular biology, because that's what they're funding. To do whole animal work or systems work, I have to borrow money from other grants. (B7)

Bowing Out

One of the other options open to scientists is to stop pursuing funding at all. Scientists take stock of their position within the scientific community of which they are a part, and determine what courses of action they should take with respect to many aspects of their work.

I, myself, don't get funding from NSERC anymore. I've dropped out of that. I just don't publish as quickly and as prolifically, as they would like. So, I just at the moment am operating on a shoestring, and I don't spend one third of my time looking for money, which most everybody else does. (G1)

While the majority of scientists may not adopt this attitude of resignation, they commonly express a high level frustration at having to do this kind of work.

Now I'm pursuing... in fact you were asking earlier how much time I spend... I spend too much time doing that, because what I don't do is, you know... I don't do enough reading at the library and fine tuning my own projects. (C3)

The diversity of activities that scientists engage in take place in real-time (see Pickering, 1995).

In other words, they have a sense of immediacy, and time must be allocated in a way that reflects the scientists' perceptions of the situations and contexts within which they find themselves. The achievement of scientific goals emerges out of everyday action that contains disappointments and compromises, along with opportunities to excel.

Just before moving on to the next section, there is one additional case to be mentioned that highlights the endless diversity of responses to the situations facing scientists.

I find that I have not pursued external money other than NSERC, simply because I haven't got the time to create the projects to spend it. I'm fully occupied trying to spend the money that I've already got. (B1)

Some scientists, and people that study science, may consider this to be an uncommon case. While we should hesitate to make that kind of judgment, it is important for us to acknowledge that it demonstrates a particular line of action adopted by a practicing scientist. As is the case for this particular scientist, so it is for us. The situation is real, and thus it is worthy of our attention, in that it is real in its consequences. In other words, as researchers, we cannot ignore some line of activity that is taking place in the life-world we are studying, just because it does not fit into some preconceived notion of what does and does not constitute legitimate activity within that life-world. All activities that take place within the setting contribute in some way to the construction of the life-world.

Preparing Grant Applications

As in the case of selling goods and services (see Prus, 1989a), much of the success in acquiring funding is tied into how well the scientist presents his or her case to a specific set of

evaluators (see Gillmor, 1986). Those evaluating research projects may be other scientists, industry representatives, government bureaucrats, and/or members of the general public. For the most part, it does not involve face-to-face interaction, and therefore it relies heavily on the ability of scientists to make their case on paper and, as with the process of identifying sources of funding, this activity can be very time consuming.

I would say I spend a great deal of time writing proposals because the best way to set up is to bring the money in. No matter how much the university is doing to help you - the "higher ups" - if you want to call them that - they reach a limit too. Times are tough all around, so you have to go out and write the proposals, and I started with a simple theory. I don't know what batting averages that people have, but the more you try at least you're chances of succeeding get higher. And in my first year, I devoted it to sending out proposals, teaching, and writing more proposals. Now I am really aggressive with proposal writing – show people you are there, show them your ideas - some of them will be funded, and lots of them won't. (C2)

I'm the head of a lab, so funding has become a major job for me. The technicians and graduate students, from six to eight people, don't have to fund, so they're doing their work. My work would normally be writing and thinking, and these elements are greatly impaired by my duty to fund. I'm comfortable this year, but last year, I must have written up five or six proposals over the course of the year. (B7)

Writing research proposals is an uncertain venture. Since scientists cannot be sure that the efforts expended will bring positive results, they often send out proposals to a variety of funding sources. Each of these typically incorporates somewhat different elements that the applicant hopes will reflect the interests and concerns of the funding evaluators. Not only is this process a critical aspect of getting started on a scientific career, it is an integral component of continued involvement.

Every three to five years you have to write a five page proposal, and supposedly you think very carefully about it, and we know that it is going to be read by our peers. So, we don't have to fill pages with a lot of blather. (C1)

What it really does to me is that my best thinking and writing are never published. They are sitting around in a file somewhere in Ottawa, and I'm really quite angry about that. (B7)

To better understand this process, it may be helpful to divide the following discussion into two parts: meeting the requirements, and interpreting the requirements. Both of these activities incorporate interpretation, but the difference has to do with what is put on paper and what is actually carried out in practice.

Meeting the Requirements

Even though the application process may be considered routine by established scientists, some agencies are more demanding than others, and therefore the time required to fill out the necessary paperwork may vary greatly.

Since Christmas (twelve weeks ago), I have spent two full weeks doing nothing but writing proposals, and so on, all to do with this federal "center of excellence." (C6)

This comment not only reflects differences among agencies, it also reflects the perception that more and more groups are involved in evaluating the activities of scientists.

I would say that over the course of my career the granting process has become more complicated. It's become a much more sophisticated, high-profile sort of thing. (C1)

As more perspectives must be taken into account, the complexity of any social activity is likely to increase. The nature of that complexity will reflect the goals and attitudes of all those groups that feel they have a stake in the decisions being made.

They're more complicated in the sense that they ask for various things which perhaps are realistic, and would be realistic in industry, but for us in academia they present a certain amount of anguish, and I'm thinking of things like socio-economic significance, relationship to societal needs - rather strange word statements - which you have to use your imagination to complete. (C1)

In this sense, scientists often must venture outside their area of expertise and speculate about the perspectives of other groups, and of the general public. This situation creates a certain amount of anxiety, as the scientist tries to second-guess any and all eventualities.

It's not too bad to write up a scientific proposal and say that we propose doing the following, and this is the way we propose to do it, and we expect to get this kind of result out of it. It's quite a different thing to say what its socio-economic impact is going to be - it's like crystal ball gazing. It's also a bit of a crystal ball gazing to list milestones - you know - what you're going to have done by such and such a date, and you have to list all these milestones. I find all this kind of thing a bit frustrating. (C2)

Predicting the future in this way is more consistent with a view of science as an objective and deterministic machine that transforms known ingredients into predictable results. In other words, in the best of all possible worlds, the scientist would be able to control for any and all inputs and outcomes, and therefore be able to state in advance the consequences, intended and

unintended, of some proposed course of action. Scientists' own perceptions, however, seem more consistent with a symbolic interactionist appreciation of science that emphasizes the contingent and emergent nature of scientific practice.

Another limiting factor in the presentation of research proposals is space. In spite of all the information and speculation that the scientist is required to provide, this typically must be condensed to fit on regulation forms.

All of this gets reprinted on the pink forms, and it has to be in the space that they've allotted. So that's why it is put together the way it is, and it's going to be reprinted on the form, listing: new equipment, qualifications, references, contributions to training - this is the societal thing - who will use the information and how, how will it be distributed. All of that kind of stuff. It's time consuming. It's fussy work. (C1)

Interpreting the Requirements

One very interesting aspect of the application process is the match between the activities that the scientists apply for funding to carry out, and those activities that actually end up being carried out with that funding. In some cases, the funding covers a broad area of research within which a diversity of activities may take place. Both scientists and evaluators interpret research proposals, and the differences in those interpretations may show up in what course of action is actually carried out.

My grant is broadly based on purpose to give me a lot of flexibility, but there are certain themes within that broad umbrella that I primarily work on, that I'm primarily interested in, and this particular discovery fell into one of those baskets. (G2)

The flexibility in the implementation stage of research has a direct parallel in the way in which the grant evaluation process takes place.

In the case of an NSERC grant, at least mine, and there are elements similar to other people's, the objectives I stated in the grant are not - they're not followed up on specifically. In other words, they look at your publication record, your record of research achievement, and there is no accounting as to stated objectives of your application. You're evaluated in general terms. (P4)

This statement does not seem to be consistent with the insistence on providing specific milestones, and predicting the social significance of particular research projects. Perhaps what actually takes place emerges out of the mutual consideration, by scientists and funding agencies,

of the goals and interests of the other. In some cases, however, agencies are stricter when it comes to following a research plan.

Now, other funding agencies that I have applied to involve much more specific objectives. You have to spell out in detail exactly what you're going to do, and your success is measured in terms of these objectives. (C3)

The extensive variation in the ways in which grant proposals can be completed and interpreted creates a situation where the scientist continuously deals with an element of uncertainty. In spite of the standards associated with granting sources, there is no sure way for the scientist to predict the outcome from any particular proposal.

I got a project funded - this was an individual project - last year, that I thought my chances were next to nothing, and I didn't get something this year, where I made a proposal which I thought was iron-clad. (C2)

Sometimes, this uncertainty can be mediated by enlisting the cooperation of others who may be interested in the project. In some cases, this involvement may be initiated by someone from outside.

In this case, one of the parties approached me, and knew something of our expertise, and said - you know - "Will you help us?" So, they're kind of supporting the application, but I'd say that's lots more unusual than usual. Usually you have to go out and find somebody to support you. This requires, oh let's say, a week of work at least to produce a decent proposal. (C3)

While this kind of involvement is no more predictable than the grant application process itself, it leads to a consideration of how scientists can enter partnerships in order to secure funds.

Entering Partnerships

Another mechanism for securing research funding is to enter into a partnership with another party that can benefit from the association. These can take a number of forms: scientists working together with colleagues at the same university or elsewhere, scientists working with researchers in government laboratories, or scientists linking with their industry counterparts. In any of these situations the course of events that emerges will reflect a negotiation process wherein a diversity of goals and perspectives will be balanced. Similarly, the kind of benefit that each party accrues from such an arrangement may be quite different.

Working with colleagues can take on a variety of forms that may involve individuals from the same university, different universities, or even different countries. The composition of a particular research group or thought collective may represent a conception of community, that is not based on nationality, or specific institutional affiliation. In some cases, funding agencies may require this kind of collaboration.

I have had a letter from a professor in Japan, asking me to co-sponsor a symposium with him in Hawaii, a Pacific rim conference. Why did he write to me? Well, he knows that I am working in the field, and you have to have representatives from three countries: the U.S., Japan, and Canada. (C4)

In other cases, the attitude may be entirely opposite.

One of the things the NSERC funding system has not done well is reward collaborative work. They have looked on it rather suspiciously. If two groups are working together, then one must be weaker, so we'll give that group less money. In some cases, that is true. (C6)

Thus, working with certain colleagues may negatively impact how a particular scientist is viewed by others. These two instances illustrate how scientists achieve identity through interactions and involvement at a number of levels.

It may be possible for some scientists to raise their profile through association with a "big name" scientist, such as a Nobel prize winner (see Crane, 1972; Hagstrom, 1974), or a "hot" area of research, such as oncogene research (see Fujimura, 1988, 1996). In other cases, the cooperation may reflect a more informal working arrangement that the funding agency may or may not even be aware of.

In fact, she's (graduate student) funded by a colleague -- we co-supervise her, and it's his project so therefore he's funding her research assistantship and I do the supervision. This is a chap who used to work here, and in fact many years ago was a graduate student of mine. He is now at another university. (G1)

As with these collegial arrangements, working with the government has its own peculiarities. Some scientists may respond negatively to the kind of perspective on science they associate with representatives of political groups. In these instances, some particular department of the government may be acting as a business partner, with the opportunity to more directly influence the course of scientific research in some direction or another.

The agenda of the "centers of excellence" bothers me in that they may turn us into some Ottawa bureaucrat's idea of an applied science laboratory. Money has a way of finding people who will do what it wants. (C6)

All such arrangements will be based on some compromise with respect to goals and objectives. However, there may also be situations where it is extremely difficult to establish enough common ground to proceed.

Well, it's difficult to get government money for that kind of joint project, because it involves two different countries. It involves the United Kingdom and Canada, and NSERC of course will entertain a grant application from me to do something, but they generally would not give any support to what would be done on the other side. So, we'd have to find some mechanism that would bring NSERC and SRC in the UK together to look at our application jointly in order to get that funded. Whereas, if we were to put an application in jointly to someplace like (international corporation) for a contract, it's feasible that we would get money from them, and probably more money from them than NSERC or SRC would be willing to give at this point. (C4)

The perception of increased benefit from the global economy may encourage industrial groups to support projects that individual governments cannot justify within their worldview. Thus, scientists monitor activities and decisions made by these corporate bodies, and determine how to integrate this awareness into their own situations.

The ways in which scientists make contact with potential industrial partners reflect the diversity of activities and opportunities for interaction that both parties engage in.

As scientific people, we go to conferences, we publish in the literature, we get inquiries, we meet people at conferences, we hear of people through the grapevine. In this university, some of our students work in industries that are relevant. So, we get some feedback sometimes that way - it's largely I think just that informal. (G2)

Here, potential funding arrangements are seen to emerge out of activities that are primarily organized around other aspects of the scientific enterprise. In certain circumstances, reciprocal arrangements that are financially beneficial to scientists can arise out of a negotiated order.

Once you have some money, then you can negotiate with companies and I did that in fact when I got the last two synthesizers. I got two for the price of one type of thing provided I would help them out as a consultant. (C3)

As with many of the activities discussed in this project, variations in circumstances can lead to different outcomes arising out of seemingly similar situations. There is no one way to do things, and our depiction of what goes on in science, or any other aspect of human group life, should reflect this variation.

Some companies are good to work with... these guys have been very good. Some other companies have no sympathy whatsoever. (Company) says, "Well, what do we care whether you're making scientific progress or not, as long as we sell a machine somewhere, we don't care." (C3)

In some cases, scientists have found a way to combine their research activities with the provision of goods and services, in such a way as to largely support themselves through commercial means. In other words, instead of being drawn into associations with industry in response to particular circumstances, scientists may also integrate the direct marketing of goods and services into their overall plans.

Marketing Services, Equipment, and Materials

Latour and Woolgar (1986:70) offer the following observation about the laboratory they were studying.

Frequently, purified fractions and samples of synthetic substances are sent to investigators in other laboratories. Each analog is produced at an average cost of \$1,500, or \$10 per milligram, which is much lower than the market value of these peptides. Indeed, the market value of all peptides produced by the laboratory would amount to \$1.5 million, the same as the total budget of the laboratory. In other words, the laboratory could pay for its research by selling its analogs.

This report based on a convenient piece of accounting draws attention to the commercial potential of the scientific laboratory. At the same time, however, it obscures, among other things, the processes involved in initially producing and then mass-producing this peptide. The scientific enterprise emerges out of real time processes. Time spent doing one thing cannot be spent doing something else, unless more staff can be hired in the process.

Now once the instrument is developed - I mean the instrument that I spoke about earlier - the one that I have been refining for over ten years... it is at the point where I'm now going to commercialize it, because it's a big pain in the neck to build one of these things. So, I can build it and there are lots of people who, if they had that instrument, would then go ahead and take some measurements. (C7)

Here, the scientist is looking for a way to get some return on his investment, so that he can proceed over the next several years to make measurements rather than building equipment. We have no way of knowing whether this development process would have taken five or twenty-five years to complete, and neither did the scientist involved. He took a risk, and now he wants to

maximize his payback. Other scientists, who can acquire the machine, will be able to use it in their research. They neither have to take the same kind of risk (i.e., loss of short-term productivity), nor invest ten years of work.

In some cases, the cost of a piece of equipment can be offset by providing a service to others who also would like to have access to such a machine, if only on a limited basis.

There is an applications specialist coming, and what I'm going to ask, what I'm trying to do is... well, I need it, I really need it... I shouldn't even be doing the rest of the work without having access to this (piece of equipment). I'm trying to set up some kind of service so that maybe I could pay for the machine, at least part of the machine, out of the service. (C3)

This kind of service may involve making measurements or preparing samples for colleagues, for government, or for industry. The exact nature of the work to be done, and the amount of time required to carry it out, will vary with the specific requirements of prospective customers.

If it's industry, then you can generate a little bit more money, but that means taking a lot of my time. That means I've got to write letters, get on the phone... everything is very time consuming. (C3)

By striking a balance between a number of possible courses of action, scientists may be able to maximize their gain out the circumstances that present themselves. Thus, they may positively evaluate devoting a certain percentage of their time to commercial enterprise, rather than say spending time going through the funding agency process. Balancing these various elements may provide scientists with greater flexibility in determining what goals they can or cannot pursue.

Making Adjustments

One of the other activities that scientists engage in around funding is making adjustments to their plans as they proceed. Sometimes the budget that they presented on paper in a research proposal does not match the actual day-to-day operating costs that are incurred in the research setting. Obstacles (equipment breaks, people get ill, the weather does not cooperate) arise and must be dealt with. An important part of the intersubjective accomplishment of science is tied in to how effectively scientists deal with these obstacles and uncertainties as they arise (see Star, 1989:62-95).

Some funding agencies appear to recognize this contingent nature of science as practiced.

That's one of the joys of the Canadian NSERC system. They want to see productivity. They are not too fussy about following your original mandate, as long as you are productive, and you're recognized for that productivity and the contributions in that field. (P4)

In some cases, this flexibility can extend to encompass new and often unanticipated avenues of research.

My NSERC funding... what I did is pretty typical. I applied to get money to do a particular project or group of related projects, and right now my grad students are working on hydrocarbon reservoirs in Ontario, and are actually fulfilling the mandate of the NSERC proposal. And these little extra things came on as a fluke, and it turns out that sometimes these little things turn out to be a lot more fruitful than anticipated originally. So I get involved in little bits and pieces of research here and there that I just had not anticipated in any possible way. (G3)

Adjustments are made and resources are re-deployed in such a way as to reflect the new definition of the situation. These processes continue and, in retrospect, may appear to constitute a smooth transition from one research interest to another. However, our concern is to highlight the jumps and breaks in activity that help us to understand how ambiguities and uncertainties are dealt with on a day-to-day basis.

In other cases, there is a clear shortfall of funding, and these merit particular attention because they may put projects in jeopardy. Often, there are relationships within the academic setting that provide a mechanism for making up just such a shortfall.

The dean will help you out, and today we have a dean who is very sympathetic. If you have most of the money, but you need something to top it off, to make it, to get a really good deal, then he'll do it. (C3)

Alternately, the scientist may negotiate directly with the supplier of a piece of equipment to get what is required within a certain dollar envelope.

Or else you get some money and you try to extend the amount of money and you negotiate with a company and say, "Well, we'll buy this, but we want this also." And they come back saying, "Well, we can do it, but only for \$10,000 more dollars... (C3)

Adjustments can be made in such a way as to extend the research possibilities within a given amount of dollars, or to extend the dollars to allow a certain kind of research to take place or a

particular piece of equipment to be purchased. In either case, these negotiations and adjustments are very much a part of the practical accomplishment of science.

Discussion

The data presented in this chapter have illustrated the variety of activities that are related to pursuing funding for scientific research. The coverage cannot be considered to be comprehensive, nor can it be taken to imply either that these activities must take place, or that these are the only activities that can and do take place. Rather, the descriptions of activities documented here arose out of conversations with practicing scientists, who indicated that these activities comprise an integral part of the day-to-day accomplishment of scientific practice. However, consistent with a symbolic interactionist perspective, the process of pursuing funding is conceived of as integral to science not on some *a priori* basis, but because scientists engage in activities related to this process in the here and now. Furthermore, the meanings that scientists attach to these activities are seen to arise out the processes of interpretation and interaction that constitute all human group life.

As a means of determining what we can learn about science by exploring the process of pursuing funding from a symbolic interactionist perspective, it will be instructive to return to the three observations that scientists made when speaking with Stewart Gillmor (1986). First of all, the data presented here certainly support the idea of "funding pluralism," however the emphasis now is not on measuring success. Rather, the data illustrate the diversity of approaches that scientists take to pursuing funding, and the implications that these activities have on what science gets done and how it gets done. For example, one scientist indicated that a stipend to cover administrative duties was used to finance research, while another indicated that the decision to carry out unfashionable research meant that most avenues for financial support would be closed. Similarly with respect to Gillmor's second observation that different funding agencies tend to fund either people, projects, or problems, the data demonstrate that scientists take these differences into account in complex ways when applying for funding. Decisions to carry out specific research programs are not taken in advance of pursuing funding, nor are these

two activities carried out in isolation from one another. Rather, an intricate relationship exists between these activities, not to mention several others outlined throughout these pages, that together constitute the experience of science as practiced. Decision-making processes appear to reflect a negotiated order that is far more pragmatic than what some might perceive as being consistent with the disinterested or universal pursuit of truth. Scientists carry out their work in the day-to-day world, just like members of other groups, and the decisions they take reflect their interpretations of this world and their place in it. Some choose to pursue large grants, while others select to operate on a shoestring. Neither situation is more or less scientific than the other, but both are certainly socially constructed.

Thirdly, the scientists interviewed by Gillmor identified what he calls the "bi-directional" character of the funding process. That is, there is a knowledge gap between what scientists know about funding (i.e., policies, procedures, and political implications) and what bureaucrats know about science (i.e., disciplinary jargon, research programs, and experimental procedures). This "incommensurability" between the scientists and the bureaucrats can lead to frustrations on both sides, and it can certainly have consequences in terms of what science gets done and how. In fact, decisions to fund certain scientists and not others might be made on the basis of what people on both sides would consider irrational and irrelevant criteria. The data presented here indicate that scientists often see things this way, and some clearly indicate that they resent this. What we want to know, however, is how this state of affairs impacts scientists on a daily basis. Part of the answer has to do with what Karin Knorr-Cetina (1983a) refers to as "transepistemic" processes, that is, processes that involve taking into account multiple worldviews. Based on the data, the process of pursuing funding would certainly fall into this category. Scientists not only take others into account when carrying out their work, but they learn to view objects (e.g., their own research, government science policy, industry goals) from a variety of perspectives, through ongoing interaction with members of other groups and through interpretation of what takes place.

The equating of scientific activity with the growth of knowledge is a theme that has arisen several times throughout the preceding chapters, and as the discussion at the beginning of this chapter shows, the role of funding is an important aspect of these debates. Studies seem

to indicate that no clear link can be drawn between the level of funding attained and the amount of knowledge produced. From the perspective of a symbolic interactionist, part of the reason why this relationship is unclear is because the wrong questions are being asked. Not only is it incorrect to assume that the primary goal of the scientific enterprise is to produce knowledge, but it is equally fallacious to suppose that there is a direct causal relationship between money and facts. Some of the scientists interviewed here stressed the importance of acquiring funds, while others were less concerned with these matters. None of them stated their views in terms of what might be referred to as a "price of truth" argument. Rather, a decision to pursue funding or not, and to what level, is tied into what I would call the "freedom to pursue goals." Ballet companies and symphony orchestras, neither of which are generally able to meet their financial needs through ticket sales alone (see Chappell, 1991; Huntington, 1993; Throsby, 1995), seek out support from government arts councils and private sponsors, in order "to perform." Outlaw biker gangs hold field days and "public" dances (see Wolf, 1991) in order "to ride." It might be the case that the goal of science is simply, in processual terms and in the words of Francis Bacon, "to twist the lion's tail." In other words, scientists poke and prod the world around them to see what will happen as a result of their actions. This view of science is consistent with Ian Hacking's (1983) contention that science "as practiced" has more to do with "intervening" in the world, than it does with the more philosophical view that science is about "representing" the world. Knowledge growth and the building of research empires may be consequences of such a venture, whether intended or unintended, but our focus is on the processes through which all of this takes place, not the products. So, the focus of much philosophy, history, and sociology of science, in following an epistemological agenda, may really be off the mark in terms of getting at what really "drives" science, at least as a real human, social, and institutional activity. However, we have only discovered this and can only pursue it further through *more* (not less) genuinely sociological exploration of the scientific enterprise.

A related issue is whether funding-related activities are to be considered scientific, and, by extension, whether our exploration of these activities brings us any closer to determining what is and is not science. As a working definition, throughout this project, science has been equated

with what scientists do. The interview material clearly shows how deeply funding-related issues are embedded in the matrix of activities carried out by scientists on a daily basis. To attempt to tease out those elements that somehow could be classified as strictly funding-focused would be to alter the way that scientists conceive of and carry out their work. At the same time, however, the activities that constitute pursuing funding do not serve to differentiate science from non-science. Rather, consistent with the whole notion of generic social processes, they serve to link science to other realms of human endeavor. There may be something unique or exceptional about science, as with other human pursuits, but this "quality," whatever it may be, is unlikely to emerge from the examination of everyday activities.

By viewing science as a realm of practice, however, we are in a position to contribute to the ongoing controversy over what it means to view science as socially constructed. As discussed earlier, Latour and Woolgar (1986) argue that the facts produced by scientists are socially constructed in the laboratory, while Knorr-Cetina (1983b) refers to social constructionism (constructivism) more as a research program for those studying science. Like many contemporary philosophers of science, following an exploration of the different ways in which the term "social construction" is used in discussing science, Sergio Sismondo (1996:77) still concludes that social constructionists' views on science can be explained away in the end as ultimately epiphenomenal. That is, he argues, science only "appears" to be socially constructed.

Scientific knowledge looks underdetermined and contingent while it is being created because there are (often) competing institutions and other social objects, and scientists spend much of their time dealing with these objects. Thus we need a theory of science, or an approach to science, that is consistent with and helps us to understand the fact that science is not the purely rationalist and cognitive exercise that some positivists thought it was. Yet to understand science's successes and failures we have to have a theory of science that doesn't make successes and failures into miracles.

Sismondo is arguing that science is in some ways a product of both cognitive and social processes, but his reversion to the word "miracles" betrays that his acceptance of this state of affairs is crucially limited. When push comes to shove, he maintains a fundamental asymmetry between the cognitive and rational aspects of science and the social and irrational. He has simply accommodated a greater measure of the latter in his conception of science, on the basis

of the findings of the history and sociology of science. His position differs little, really, from the exceptionalist view of science held within the traditional sociology of knowledge, and it points to the fact that Sismondo is still evaluating social constructionism from the standard epistemological standpoint of the philosophy of science. In other words, he, like many advocates and critics of social constructionism, still views the production of certified knowledge as the definitive goal of all scientific activity, and he believes that decisions as to the success or failure of the products of science are based on largely rational criteria. The social side of science, while certainly evident, is still viewed as something to be taken into account to a point, but ultimately explained away. What I would argue is that when science is viewed "naturalistically" as a practice, and not "artificially" as a philosophical system, then whether a development in science is logical and rational is irrelevant to the "scientific" study of science. What matters, as symbolic interactionism would argue, is that our understanding of science reflects what actually takes place among practicing scientists, as they attempt to accomplish the goals they have set for themselves.

The advocates of the strong programme (see Bloor, 1976, 1991; Barnes and Bloor, 1982) suggest that the social and cognitive aspects of science should be treated symmetrically. That is, when examining the basis for the successes and failures of science "all beliefs are [to be treated] on a par with one another with respect to the causes of their credibility" (Barnes and Bloor, 1982:23). In other words, whether something is determined to be true or false, the basis for that truth or falsity must be sought symmetrically. True beliefs cannot be assumed to be solely cognitive and rational, any more than false beliefs can be assumed to be solely irrational and social. Following the standard view within the philosophy of science, most scholars studying science start out with the assumption that the cognitive and the social are separate, and yet the bulk of evidence gathered from studies of science does not appear to support this view. Like Barnes and Bloor, I have chosen to assume otherwise, and the material presented here, while certainly not conclusive, demonstrates that it might be worthwhile grounding the sociology of science on this alternate approach.

Taking this analysis a step further, social constructionism, as the term is used by Sison and others, implies that scientists are not dealing with the application of steadfast rules, but that these rules are dynamic (changes in thought styles, changes in paradigms, changes in government policies). Changes occur both with respect to the social side of science (funding, government policies, and so on), and the cognitive side of science (method, logic). Maintaining the dichotomy between these elements lends support to a perception that somehow the social aspects of science are negotiated separately from the negotiations of cognitive elements, and that the almost accidental result of these two separate processes of negotiation (cognitive negotiations having priority over social negotiations) give us what we call science. As stated above, I contend that, in practice, no distinction is maintained between the social and the cognitive, and that the negotiated order that manifests itself as the products of science is more accurately conceived of as "intersubjective accomplishment." Latour and Woolgar (1986:281) hint at this when they argue that the word social should be dropped from the phrase social construction because all construction is social. I would go further to suggest that the term "social construction" be abandoned within science studies altogether as ambiguous and politically loaded (see Gross and Levitt, 1994; Ross, 1996).¹¹ From a symbolic interactionist perspective, then, it is not enough to say, as others have done, that pursuing funding is important to, but distinct from, science. Rather, the evidence presented here supports the contention that funding-related activities are, in fact, integral to science as practiced, in ways that alternate approaches to the study of science are less well equipped to take into account.

A further benefit of the symbolic interactionist approach to science, and the use of the generic social process scheme, is to permit researchers to demonstrate just how similar science is to other group activity. With respect to the GSP scheme, more directly, the following list of activities was used to order the presentation of data in the chapter and provide a framework for discussion.

Identifying Sources of Funding
Dealing with Government Agencies
Financing a Career

¹¹ In Chapter 9, I discuss the so-called "science wars," in which social constructionists are accused of trying to undermine the scientific enterprise.

Satisfying Mixed Agendas
 Responding to Innovation
 Identifying Industrial Niches
 Looking for Alternatives
 Tapping into the Broader Community
 Looking within the Institution
 Settling for Less
 Bowling Out
 Preparing Grant Applications
 Meeting the Requirements
 Interpreting the Requirements
 Entering Partnerships
 Marketing Services, Equipment, and Materials
 Making Adjustments

The fact that some of these areas are explored in a more detailed fashion merely reflects the quality (implying characteristics and not value) of the data, rather than suggesting that some activities are more or less important than others. Any one these activities could form the basis for an extended research project, just as there may be other activities related to funding that have not been dealt with here, such as the ongoing administration of funds over the period that a grant is held. At the same time, these same processes should be evident in the activities carried out by members of other social groups.

In his study of an outlaw biker gang, Daniel Wolf (1991:250) indicates that the club had five major sources of revenue: membership dues and club fines, the sale of various commodities to members, the brokerage of club shares, sponsoring 'boogies,' and holding 'field days.'" Like members of other groups, bikers work towards achieving their goals, and for motorcycle clubs this means holding "runs," in other words, being able "to ride." While direct parallels with the activities carried out by scientists may not be obvious initially a closer examination of how these sources are employed by the bikers will help to draw out the more generic elements. For example, just as scientists apply for funding at regular intervals (every year, every three years, and so on), bikers pay annual dues to their club in order to continue to carry out their "work." The imposition of club fines is not unlike a sort of "peer review" process through which the actions of individual members are monitored and evaluated. Similarly, just as scientists market various compounds, equipment, and services to their colleagues, bikers provide beer, motorcycle parts, or even a room for the night, to club members at special prices, or through barter. Bikers can turn to the club or other members for a loan, just as scientists can look within their institutions for

funds to supplement or replace what can or cannot be obtained externally. In some cases, when adequate funds cannot be obtained, bikers will make adjustments by not riding as frequently, or they may bow out for a season if circumstances warrant such action.

On a larger scale, motorcycle clubs will often tap into the broader community by holding a 'boogie,' which is a "public" dance. That is, the usually closed world of the outlaw bikers will be open to certain members of the public who want a "taste" of the outlaw life, through a well-planned and carefully-executed event, in many ways parallel to preparing to carry out a scientific experiment. Just as scientists set up laboratories, and purchase equipment and raw materials, bikers rent a large hall or public place, hire a band, and bring in gallons of beer. Tickets for these events are sold through motorcycle shops and bars. In carrying out these boogies, bikers are balancing their desire to be left alone with the need to finance their lifestyle. Just like scientists dealing with government agencies, equipment suppliers, or their colleagues, the successful operation of these public dances involves taking into account different worldviews.

Bikers also hold "field days," to bring several clubs together for a series of competitions and some "serious partying." These events are closed to the public, and every effort is made to isolate the events from potential public contact and police surveillance. As Wolf (1991:253) indicates a successful field day is a "collective enterprise that demonstrates the strength of the brotherhood as a resource base of diverse skills and a wide network of both business and streetwise contacts." For example, several members worked together to construct a motorcycle out of spare parts that they were able to get their hands on, and this was raffled off as part of the festivities. Also, corporate sponsors, especially Harley-Davidson (the sole supplier of equipment to outlaw bikers) through their local sales and service outlets, will contribute prizes for some of the competitive events. Just as with scientists who enter partnerships or identify certain niches, bikers build on their skills and knowledge as a resource to help them attain funds. Through one such event, "with upwards of four hundred people attending the forty-four-hour-long party," the club realized a profit of about \$6000.

In terms of examining other aspects of human group life, the kinds of processes documented here might help us to increase our understanding of ballet troupes, theater

companies, symphony orchestras, and other groups that rely on government and corporate funding for their existence. Similarly, these activities might help us to examine the changing ways in which universities and private schools are appealing to broader audiences for support. It might also be interesting to take one of the many subprocesses or activities mentioned here and examine it in a number of diverse settings. For example, it might be instructive to compare how various groups faced with shortages in government funding look for alternatives by tapping into the broader community. A study of this kind could include a number of voluntary sports and recreational associations, community health services, day-care facilities, help lines, and so on, as well as the scientific community.

Chapter Eight

MANAGING EQUIPMENT

As with the previous chapter on funding issues, this chapter begins with a survey of some of the studies that have been carried out on the part that equipment plays in the scientific enterprise. Again, I draw links between issues raised in these studies and the material that was provided to me by the scientists I interviewed. Further, I compare the activities engaged in by these scientists with those engaged in by biker gangs and sex workers, as they take the equipment they use into account. The data also support alternate interpretations of the terms “black box” and “technoscience,” and allow for further clarification of the notion of non-human agency.

Context

Herbert Blumer (1969:2) indicates that one of the fundamental tenets of symbolic interactionism is that people respond to objects (animate or inanimate, conceptual or concrete) based on the meanings that these objects have for them. Regardless of the form of these objects, the two most important points to remember are that these objects constitute the “obdurate reality” which people confront on a day-to-day basis, and that people assign meanings to these objects through ongoing processes of social interaction and interpretation. The material presented in this chapter focuses on how scientists respond to one particular class of objects, namely, the equipment they use to carry out their scientific work. Consistent with symbolic interactionism and contrary to some versions of actor-network theory (see Callon, 1986, 1995; Latour, 1987, 1988; Pickering, 1995) the present project does not extend the notion of agency to encompass non-human objects. Rather, the action of science is to be found arising out of the intersubjective accomplishment of day-to-day activities by human actors that take this equipment and a range of other objects into account as they pursue their goals. Thus, for present purposes, we are interested in the equipment itself only to the extent that it is meaningful to the scientists that encounter it on a daily basis.

From test tubes to space telescopes, scientists regularly make use of a broad spectrum of equipment (devices, machines) to help them reach their goals. In spite of this, equipment has not figured prominently in social studies of science, and we know very little about how scientists deal with equipment-related issues on a daily basis. With the exception of a few studies examined below (Traweek, 1988; Jordan and Lynch, 1992; Barley and Bechky, 1994; Nutch, 1996), when machines have been mentioned, it has tended to be in the context of two ongoing philosophical debates. First, several sociologists of science have discussed equipment in the context of delineating the nature and relationship of science and technology. The boundary between the two has proven to be very fuzzy (see Knorr-Cetina, 1981; Latour and Woolgar, 1986), raising the philosophical problem of the demarcation of science once again. Second, the issue of how scientists interact with their equipment has been raised in the context of the sociological notion of the "black box" (see Latour and Woolgar, 1986; Collins, 1992). This notion centers on the way in which scientists treat certain objects (e.g., equipment, facts, methods) as taken for granted and unproblematic, as they go about their work, thus opening up discussions on the relationship between theory and evidence and the closure of scientific debates. A closer examination of these two issues will help to illustrate the impact they have had on the social study of science.

Pickering (1995:157) points out that people tend to associate science with the production of knowledge and technology with industrial settings and material production. This separation of science and technology is evident in the Mertonian tradition of the sociology of science, where no mention of equipment is ever made, because scientists are seen to be engaged in the more abstract pursuits of the development and testing of theory. Evidence that the boundary between science and technology was not so distinct in practice, however, led some sociologists of science to view things differently; arguing that there was in fact no significant difference between science and technology. For example, actor-network theorists (see Latour, 1987; Law, 1987) emphasized the instrumental character of what they called "technoscience." A group of researchers engaged in studying the "social construction of technological systems" (see Bijker, Hughes, and Pinch, 1987) advocated a more extreme variant of this position, in which

technology was to be viewed as being on a par with science, at least with respect to its level of abstraction. Both of these positions represent a return to philosophical arguments over how to determine what is or is not science. As has been argued throughout, I not only think that these debates are largely irresolvable, but, in the absence of much more empirical evidence of what actually takes place in scientific practice, sociological efforts to contribute to the resolution of these problems are misdirected and futile.

Sociologists of science use the concept of a “black box” to describe the way in which equipment is often discussed within the more positivistic and mechanistic framework of science textbooks and journal articles. This term refers to any apparatus (or procedure) that when given a specific input x will reliably produce output y . What goes on inside the black box is viewed as “unproblematic,” in that individual scientists can be confident that their peers will not question that a certain device produces output y from input x , and that any scientist using this device will obtain the same results. This element of control and consistency is seen to provide a sense of permanence and stability to some aspects of the scientific enterprise. As Latour (1987:3) argues,

no matter how controversial their history, how complex their inner workings, how large the commercial or academic networks that hold them in place, only their input and output count.

In support of the appearance of closure or stability, social studies of science have shown that scientists regularly forget (ignore) the details of the messiness and contingency of scientific practice associated with the construction of these black boxes in the first place. Latour (1987) attempts to account for this forgetfulness by arguing that scientists build strong resource networks in support of their positions. On the other hand, Collins (1992) argues that scientists rely heavily on tacit knowledge rather than on more formalized notions of theory and evidence. In either case, though, the primary concern is with using this information to support their debate with more realist sociologists and philosophers of science about the essential nature of science. Consequently, they do not pause to investigate further the actual ways in which scientists deal with equipment. Is all of the equipment commonly used by scientists treated like “black boxes?” To even begin to answer this question, or simply to discuss the notion of the “black box”

adequately, we need to know more about what kinds of equipment, under what circumstance, are classified in this way. As with the science and technology issue, in the absence of more empirical evidence, our understanding of the role that black boxes may or may not play in scientific practice is too limited.

Regrettably, the situation has become even muddier with the emergence of a third philosophical debate. One group of researchers (see Callon, 1986, 1995; Latour 1987, 1988; Law 1993) conceptualizes the whole array of instruments, supplies, and other tools employed by scientists as “material agents” which, like “human agents,” play a role as “actants” in the power game of accomplishing science. In other words, these scholars argue that scientists, bureaucrats, scientific apparatus, raw materials, funding, and all other objects are to be seen as constituent parts of the network that supports the scientific enterprise, with no privileged position being attributed to scientists or other humans in the network. This approach is at the heart of the epistemological “chicken debate,” which was first played out in a collection of essays on science studies edited by Andrew Pickering (1992).¹ In this debate, Harry Collins and Steven Yearley attack the “extended symmetry” of actor-network theory with respect to human and non-human (material) agency. They argue that there are two alternatives. Either we see scientists as producing accounts of material agency and then analyze these accounts sociologically as the products of human agents (the position favored by Collins and Yearley), or we accept the existence of material agency and yield our analytical and descriptive powers to the scientists themselves, as they are the only ones qualified (scientifically) to address such issues (Pickering, 1995:12). In other words, either we can do social studies of science, which assume the uniqueness of human agency, or we abandon our efforts in favor of truly internalist accounts of science.

In their response, Michel Callon and Bruno Latour offer a third interpretation based on the idea of treating both human and non-human agency semiotically. In other words, in discussing science we are merely speaking (using various signs and symbols) as if humans and

¹ Pickering (1995) provides an excellent summary and analysis of the debate, which forms the basis for my coverage.

machines have agency, and the issue of whether these things are real entities or social constructs is not important. Pickering (1995:13-15) identifies two problems with this response. First, he points out that a semiotic approach leads us away from studying the temporally emergent practice of science into an atemporal “world of texts and representations.” Second, he draws attention to the fact that while it may be permissible, semiotically, to speak as though there is no difference between human and non-human agents, in practice it is not. As he (1995:15) observes,

I find it hard to imagine any combination of naked human minds and bodies that could substitute for a telescope, never mind an electron microscope, or a machine tool, or for an atomic bomb (or for penicillin, heroin,...)

Pickering's alternative solution to the chicken debate (the mangle of practice) was discussed in Chapter 4. Moving ahead, however, there are a number of issues of consequence to the social study of science raised by the chicken debate. First, it illustrates the degree to which social studies of science have been hijacked by efforts to contribute to a philosophical agenda that is largely irresolvable. Second, it leaves us wondering what we might discover if researchers abandoned the philosophical agenda and returned to the open empirical exploration of science. Third, it emphasizes the fact that while machines are clearly recognized as central to the scientific enterprise, we know almost nothing about how the relationship between scientists and their machines manifests itself in practice.

Turning now to an examination of some actual field studies of science, there are a number of studies that offer a glimpse of the daily activities that scientists engage in with respect to equipment. Sharon Traweek (1988) stops short of attributing agency to machines, but she does view the “detectors” that one particular community of physicists use to collect data as the “material embodiment of the high energy physics culture” (1988:46).

Detectors are distinctive and serve as the “signature” of the group. These machines are at the heart of the research activity of particle physics. Detectors - their conception and development, their maintenance, their performance during the precious allotment of beamtime for an experiment - are the stuff of frustration, hope, heartbreak, and triumph for research groups. (Traweek, 1988:49)

Several aspects of this statement are important for the present project. First, Traweek's emphasis on detectors reflects the importance attached to these objects by the scientists

themselves, and suggests that the scientists' identities, perspectives, and careers are intimately tied in with them. Second, we see that it is not the detectors themselves (in some external objective sense) that are important for these physicists, but rather it is the range of activities associated with these devices that constitutes the scientific enterprise. Third, the actual activities of conceptualizing, designing, building, operating, and maintaining these detectors are just the kinds of activities reported by the scientists studied for the present project. Finally, Traweek's statement alerts us to the human side of the scientific enterprise, by drawing attention to the frustration, triumph, and other human products that emerge from scientific work, along with whatever scientific knowledge and discoveries might result.

Another interesting observation that comes out of Traweek's study has to do with the cultural differences between America and Japan with respect to how the process of "fixing" detectors is carried out. In the case of the American physicists, the approach to equipment is much more pragmatic and hands-on, while in Japan the situation is very different.

[T]he experimentalists did not have the means to make and unmake their own equipment, even if they wanted to. Their own training had not prepared them for dismantling and rearranging detectors, but for overseeing the work of engineers. (Traweek, 1988:69)

Not only does this statement emphasize that we must be cautious when we attempt to conflate science and technology, but it contains the significant implication that a machine in Japan is not necessarily the same as a machine in America. Rather, cultural differences between groups of scientists will be reflected in the meanings that members of these groups attach to detectors and other devices. While in this particular case the potential for alternate interpretations is based on national differences, other studies (see Jordan and Lynch, 1992; Fujimura, 1996) provide evidence that the same phenomenon, i.e., multiple interpretations of things otherwise assumed to be objective and given, can be observed across a range of settings, right down to a single laboratory.

In a collection of papers edited by Adele Clarke and Joan Fujimura (1992), a number of scholars examine various "tools" that researchers in the life sciences have used to help them reach their goals. The studies are largely historical, and can be seen to reflect an aspect of Thomas Kuhn's work.

So long as the tools [that] a paradigm supplies continue to prove capable of solving the problems it defines, science moves fastest and penetrates most deeply through confident employment of those tools. The reason is clear. As in manufacture so in science – retooling is an extravagance to be reserved for the occasion that demands it. The significance of crises is the indication they provide that an occasion for retooling has arrived. (Kuhn, 1970:76)

For Kuhn, as for those represented in the Clarke and Fujimura volume, the notion of tools encompasses not just specific pieces of equipment, but also procedures, tests, and other patterns of activities that scientists may engage in.

Among the findings are the notions that “tools” are constructed, disciplined (made to ‘behave’ consistently in predictable ways), and reconstructed along with the jobs that they are designed to perform and the appropriateness, or “rightness,” of the tools for those jobs (see, Clarke and Fujimura, 1992:17). In other words, in the process of accomplishing science, scientists not only construct and modify their tools, but many aspects of their life-worlds may go through simultaneous transformations. That is to say, a change in one aspect of scientific practice, particularly with respect to tools, often is accompanied by changes to other aspects (e.g., research interests, allocation of resources).

While some issues discussed in the studies reported in the Clarke and Fujimura collection are consistent with the approach advocated in this study, there is one major weakness. In only one study out of ten (Jordan and Lynch, 1992) do we hear directly from the scientists involved. Through their analysis of the “plasmid prep” (a tool on its way to becoming a black box), Jordan and Lynch discover that a seemingly standard procedure is subject to a diversity of local variations that reflect the particular circumstances of those employing the procedure. These authors contend that the notion of a black box is primarily a “representational convention,” and that a more accurate description of what takes place in practice would be to refer to these tools as “translucent” boxes. That is, reflecting the processual and emergent nature of scientific practice, the outlines of these devices are not clearly resolved, and the inner workings are open to dispute. Unfortunately, Jordan and Lynch, along with several other scholars represented in the volume, use these discoveries of the contingent nature of scientific practice to engage in discussions of what constitutes rationality and realism in science. We are more interested in

hearing from scientists directly, because, as indicated, an interactionist approach to science is not to be limited by some philosophical research agenda.

As was pointed out in Chapter 4, one study that shifts the focus away from the equipment itself towards the people responsible for looking after the equipment, was carried out by Stephen Barley and Beth Bechky (1994). These scholars were interested in the work of technicians in scientific laboratories, and they make a number of observations pertinent to the focus of this chapter.

Technicians focused more on the instruments than on the cells they analyzed, knowledge of optics, lasers, fluorescence, and computers was considered more crucial than knowledge of cell biology. (Barley and Bechky, 1994:93)

This statement illustrates the fact that the distinction between technology and science, and the knowledge and activities associated with them, has consequences for the way in which science is carried out on a daily basis. At the same time, however, the reliance of science on technology, and on people who can work with it, is also highlighted.

Much of the technician's and the research support specialist's work involved setting up and aligning instruments, a time-consuming task that required adapting the devices to the study at hand. (Barley and Bechky, 1994:94)

As will become clear throughout this chapter, many of the activities that Barley and Bechky describe as part of technicians' work are carried out in varying degrees by the scientists interviewed for this study.

in a somewhat related effort, Frank Nutch (1996) presents a study of what he calls "gadget-scientists," who employ a variety of "gadgets, gizmos, and instruments" in carrying out their research. Based on his reading of Fujimura (1987) and Traweek (1988), and through his own study of scientists at the Crescent Reef Marine Laboratory, Nutch argues that unlike gadget-scientists, "conventional" scientists use "ready-made," "off-the-shelf" equipment to carry out their work. When they are confronted with difficulties related to the use of such equipment, these "conventional" scientists

are more likely (1) to rely on technical support and mass-produced equipment, (2) to undertake a different project, or (3) to change their research project significantly. (Nutch, 1996:220)

Unfortunately, there is little empirical evidence to indicate how Nutch determined these three options, or how scientists determine which of these options to take.

Two elements of Nutch's study have particular relevance for the present project. First, the creation of a typology in which some scientists are labeled as "gadget-scientists" and all others are "conventional" scientists seems overly simplistic. In spite of its apparent basis in practice, it is unlikely that this dualism can reflect the dynamic nature of the scientific career, within which perspectives, identities, and activities are constantly changing. Second, my research seems to indicate that the kind of activities that gadget-scientists engage in (adapting, modifying, repairing, and tinkering) are the norm among many scientists, rather than the exception. On a more positive note, through his fieldwork, Nutch came to realize just how much of scientists' daily activities are centered around issues of dealing with equipment.

What these studies demonstrate is that it is not the equipment itself, but rather the meanings that scientists attach to it on a daily basis, that is crucial to the scientific enterprise. Building on this insight, the material in this chapter contributes to a more detailed understanding of the content and complexity of the activities that comprise the process of managing equipment. In other words, our interest is in the range of activities that scientists engage in as they employ certain material resources (machines, tools, equipment, and related procedures) to accomplish their goals. The scientists interviewed indicated that the kinds of activities involved here include:

Selecting Equipment

Acquiring Equipment

Operating Equipment

An examination of these activities will show that the practical accomplishment of science on a day-to-day basis is a collective enterprise that arises out of the ongoing interaction and interpretation processes engaged in by scientists and members of other groups with whom they come in contact. At the same time, it will demonstrate how, even though science is a purely human endeavor, scientists carry out their work in a complex technical environment made up of a vast array of objects that must be continually taken into account. The details of the activities

they engage in also help us to see how scientists compare to members of other groups that work with equipment (e.g., bikers, chefs, auto mechanics, musicians).

Selecting Equipment

The equipment that scientists use to carry out their research varies with the kinds of problems they pursue and the different approaches to solving these problems that particular scientists identify with. The ways in which equipment is selected reflect the broader range of perspectives associated with the particular paradigms or thought styles within which scientists may be working, and these ways emerge in response to those objects that are meaningful in the context and situation at hand. In some cases, the selection process may appear to be quite straightforward and well defined.

We equipped ourselves for a neurophysiology station. So, that meant an oscilloscope, an FM tape-recorder, an electrode puller – all of the electronic equipment for doing intracellular measurements and injections. (B5)

My work doesn't require sophisticated equipment. Basically, I need a high quality microscope, and bench space, and a freezer, and dissecting equipment. (B1)

This work could have been carried out in the 1920s. The techniques were so simple, and everything I needed was at hand. (B6)

Other cases may reflect a greater degree of complexity and investment, where the interests and expertise of a number of associated groups is taken into account.

It was a lot of hard work, as you can imagine, because of course it involves a lot of electronics and engineering and computer science and programming, in addition to chemistry, but I had a lot of fun doing it. (C7)

This statement is a pointed reminder not only that the practical accomplishment of science is intersubjective, but that much of the success and satisfaction derived from participation in the scientific enterprise is directly related to overcoming the array of challenges and struggles that arise in working with others.

The coverage of the equipment selection process is divided into three subprocesses that serve to illustrate the variety of ways in which this process can be seen as emerging out of collective action. These are:

Adhering to Standards

Working within Limitations
Learning from Others

As with all of the processes and activities that comprise the doing of science, there is a great deal of overlap and interrelatedness not only among these three elements, but also with many of the facets of the practical achievement of scientific goals covered elsewhere in this work.

Adhering to Standards

In many cases, the selection of equipment is carried out within the context of the ways in which research is, or has been, carried out by others engaged in similar or related projects. This collective activity can lead to a situation where certain standards are set with respect to what pieces of equipment are considered appropriate for certain research activities, and to specific ways in which those pieces are to be used. On the one hand, this process of standardization serves to remove uncertainty both with respect to how certain scientific procedures are to be carried out, and how the results of using such procedures will be received by other scientists (see Jordan and Lynch, 1992). On the other hand, standards may constrain individual creativity, or the ways in which scientists may take unanticipated results into account.

There are instances in which the level of standardization leads to a situation where scientists representing a diversity of research programs and objectives will basically utilize the same stock of equipment for their research.

The bulk of astronomers, observational astronomers, in this country work at universities, but don't have their own telescope facilities. So, they use other facilities, such as the Canada-France-Hawaii telescope and the James Clerk Maxwell telescope, which are both located in Hawaii and have their primary value at different wavelength regions. (P1)

This particular level of standardization (international) is taken into account when scientists determine their research objectives, and both the determination and the consequences of the compromises they make are shared among all participants in this area of research.

Certainly, in North America, because of the way in which the big telescopes and big observatories have developed, there have been a few locations which have developed techniques for retrieving that information, and those techniques are made available to everybody who wants them, for free. But, that means they are standardized and that's good, because it allows other people to have a pretty good idea of what you've done and how you've gotten your data base. So,

when they're comparing, when they're reading a paper that you've written, or looking at your data set, they have an ability to evaluate it from their own perspective, because they've done similar things. So, you say, "I took this with such and such a CCD (charge coupled device), and such and such a telescope, and I used these techniques to reduce it, and then after that I used these techniques." A lot of these techniques are very similar from one place to another, and that allows people to... it makes it easier for someone like me to get the data and for me to understand other people's data bases. The other thing it does, of course, is it means that you don't have - unless you are working at an observatory or you're an instrument builder yourself, you don't have the flexibility to design a specific program for your specific needs. So, anytime you are doing any observation, what you're doing is always a compromise between what you want and what the instrument has to offer. (P1)

The process of ensuring compatibility with other equipment or software, or the procedures to be carried out in some other application, does not only take place at the level of an astronomical observatory.

During one of my interviews, a student from another laboratory came into the office to see if he could borrow a particular piece of equipment from the scientist being interviewed that he needed for an experiment he was intending to carry out. The student had picked up the apparatus in the scientist's lab, and had it in his hand. The scientist nodded his consent and let the student go on his way.

He's a very bright person, and I have a great deal of respect for him, but he hasn't thought about the problem. The first thing that he knows is that he needs a regulator, and without even thinking, he's just happy to have found a regulator. He won't get the job done. Think about it from the ground up. This is my instrument, and it meets 1500 psi. They would have to have a compatible system (which they don't), otherwise it won't work. (C2)

While there may have been some pedagogical purpose in this specific course of events, the important point for consideration here is that, when multiple pieces of equipment are put together in order to carry out some activity or other, success will be related to how well participants are able to incorporate a number of elements into their interpretation of the situation. The possession or selection of one piece of equipment has consequences for the process of selecting another device.

Working within Limitations

The notion of working within limitations centers on how scientists take into account the level of access that they have to material and human resources, as they attempt to accomplish their goals. In certain cases, these limitations may be conceptualized financially.

There are all sorts of great experiments that we know will provide most definite answers to the questions that we are posing, and you cannot do, simply because you don't have the instrumentation. And the kinds of instrumentation that I'm talking about is 150,000 to a quarter of a million dollars. It's just not cheap things. (C2)

Linked to the process of assessing feasibility, scientists may hope to carry out certain research activities, but when it comes to selecting the appropriate equipment to carry out their plans, they may find themselves making adaptations that match the limitations of their present circumstances.

The easiest way to get good pure preparations of DNA is to use an ultracentrifuge to spin out your samples in cesium chloride, at speeds of maybe 60 to even 90 thousand Gs. But, I don't have an ultracentrifuge, so I have to use a chemical method, which works, but is quite finicky, and we're having a tough time getting it to work properly. (B3)

Even when scientists possess, or have access to, seemingly appropriate pieces of equipment, regardless of their monetary value, other conditions may need to be taken into account. For example, when the community of scientists gains access to new and improved material resources, existing resources, regardless of how much is invested in them, are still conceived of as old.

We see great scientists that cannot make the measurements they need to make, simply because they don't have the equipment to make the measurements with. And, to make things worse, things are changing at an accelerated pace. So, if you've got something that's state of the art today, a few years down the line, it is yesterday's news, and I think that computers are a classic example of this. (P4)

Keeping in mind that our concern is not with the equipment itself, but with how scientists deal with that equipment, it is instructive to consider how changes in computer technology, for example, affect the ways in which people practice science. For example, Paul Edwards (1995) points out that the majority of studies examining the social aspects of computer technology have tended to be "impact" studies reflecting either a utopian or dystopian perspective. Edwards

(1995:258) argues not only that these studies tend to be ahistorical, but that they lack the kind of “detailed exploration of local effects characteristic of some of the best science studies literature.” Based on three case studies (computers in the military, computerization in banking, and gender identity and computer use), Edwards (1995:284) concludes that it is inappropriate to view computers as “causing” social change. Rather, it is essential to take a “social process” approach to understanding precisely how computers are integrated into existing networks of social relationships. As one of the scientists interviewed for the present project stated:

All three of these things – how you could do the calculations, how you could write the papers, and how you get scientific information – that dramatically changed in the last 15 years, and all because of the computer – combined with the internet. (P2)

Here, three aspects of accomplishing science on a day-to-day basis are identified as having been effected by computer-related innovations. Further research might follow up on any or all of these three, to examine the details of the specific changes that have taken place in the activities of scientists that emerge as they integrate computers into their daily practice. This will involve taking more into account than the technical capacity of computers and other equipment, not to mention the changing nature of scientific problems. Such research will also need to reflect the attitudes of administrators, funding agencies, and colleagues, with respect to their awareness and appreciation of the broader consequences, particularly around resource allocation, that technological advances imply.

The process of selecting equipment reflects not only the scientists' understanding of what some particular piece of equipment will do for them, it also incorporates a level of understanding of broader-based trends and anticipated changes in the capabilities of that equipment.

Well, spectrographs have a variety of limitations. The most severe one that is now being resolved would be that conventionally, you take a spectrum of one object at a time, and that means that not only are you taking a long time to take that one object, but you're only able to do a few in a night. There are now a group of spectrographs that are being developed in various ways that will allow you to take spectra of 15 or 20 or 30 objects at the same time. They use fiber optics, and again, you have to have the sophisticated computer capabilities to do that online, on site, then and there. (P1)

I needed at least 8 megabytes of RAM to handle the symbol manipulation software, but I got 16 megabytes of RAM because I really wanted to be sure

that I could do these calculations. On a previous purchase, I skimped a little bit on that, and I thought on this one, "I'm going to get it. It's an extra couple of hundred dollars, but it's worth the money." (P2)

I bought this thing (computer) a year ago – I expect it will last me a few years – but I'll have to upgrade it probably in another three years because there just will be better software developed, and it will require bigger capacity. You'll be able to do that much more. (P2)

Scientists often balance the immediate cost of equipment with considerations of the "life-expectancy" of that equipment. The ways in which scientists interpret pieces of equipment and other objects are subject to change, and in selecting equipment scientists attempt to predict to some degree what those changes might be.

Well, a mixture. It was done the way a person would do any typical consumer purchase. You want the best value for the least amount of money – computers especially. Since this is for professional purposes, this was top of the line, it was the fastest – I mean, it isn't now, I expect – but when I got it, it was the fastest machine on campus. So, what criteria? I wanted something that had a lot of storage space. So, this has a one gigabyte hard drive – the previous computer I had had a 60 megabyte hard drive. So, this is more than ten times the size. Why? Software programs take up a lot more room on the disc, and storing my data which are my papers and my calculations – they take up space – and I just need that space. I wanted a machine that, for economic reasons, I wanted to get the top of the line, so I could be convinced that it would last me on the order of five years, which I expect this machine will last me. (P3)

Thus, the notion of working within limitations can be seen to reflect scientists' understanding of their own work and the part that equipment may play in accomplishing that work. Equipment may be selected on the basis of an assessment of the scientists' present circumstances (in terms of resource allocation) or on the basis of an assessment of how the incorporation of any particular piece of equipment into the scientists' working environment may alter ongoing or planned courses of action.

Learning from Others

In other instances, the selection of equipment will be based on what scientists can learn from others, either through formal or informal means. This process may involve observing the operation of some piece of equipment in someone else's laboratory, or it may even involve trying out the piece of equipment to see if it meets some particular criterion or other.

The guy I was working with was a pure neurophysiologist, and I was a

neuroendocrinologist with not a lot of experience on the neuro side. So, he knew exactly what equipment he wanted, and he also knew what the better ones were that we could afford. (B5)

Well, it's a combination of things. You read the literature. You find out that people are doing a certain type of measurement. You have materials which you think may have the same properties as these people are studying, so you want to do that measurement as well. So, usually the first step, and I guess we sort of more or less followed this, is that you find someone who already has the existing equipment and you say, "Can you measure this material for me and see if it has these properties?" And they do, and sure enough it has the properties you want and you get all excited. And then you start doing a few other things in their lab and you say, "Could I measure this and could I measure that?" And then you start doing the measurement yourself in their lab because nobody wants to spend usually weeks and months doing your measurements. So, I will send my student over to their lab to do these measurements and I will also go there. And after you do a little bit of this, you realize that the materials that you're making do have the properties that you want, and at some point it becomes apparent that this is going to become a major research effort in your lab, and so you're going to have to acquire this equipment yourself, otherwise you're not going to get very far. (C5)

Here, the scientist makes use of various relationships and associations with scientists in related research areas to gain access to equipment on a trial basis in order to evaluate its appropriateness, before allocating significant resources and committing to a new or modified course of action. The various stages of involvement mentioned here each provide an opportunity for continuance or termination. At each point, the scientist is simultaneously selecting a piece of equipment and a course of action for their research, on the basis of ongoing interaction with others and a process of ongoing interpretation.

I've had to acquire a lot of new equipment to study these materials. In fact, an entirely new set of apparatus for measuring the conductivity in these materials. We had all of the wherewithal to make the materials and to do some of the studies of their structure, and then we had to set up a new part of my lab to measure their properties. (C5)

Thus, the decision to acquire a new piece of equipment may also entail making other modifications in the way that scientific activity is carried out, with respect to pursuing funding, managing students, and other activities illustrated in this work.

The process of learning from others when selecting equipment shows a continuous and emergent quality, where ongoing interaction with colleagues constitutes the intersubjective achievement of collective goals.

Then usually you turn to the person whose lab you've been in, whose equipment you've been borrowing, for counsel as to how to build something

like this. You talk to many other people, then you start to publish your own research in this area. That brings you to conferences where other people are publishing the same sort of data, so then you talk to these people about what kind of apparatus they have, how did they build their's... people share this kind of information usually. "Oh, well I got one of those French systems." You know, things like that. So you slowly over a period of time acquire information as to where to go for all the bits and pieces. (C5)

Collins (1992:55-56) describes the difficulties faced by scientists in their attempts to build a TEA (Transversely Excited Atmospheric pressure) laser. Not one of the scientists he spoke with was able to construct the laser solely on the basis of the available published materials. Success came only through direct personal contact with the original builder of the laser. Further, no scientist was successful in building a laser whose informant was a "middle man," that is someone who may have been involved, but that had not actually built a laser themselves. Even among those that succeeded in building the laser, only those that engaged in an extended period of contact with the original builder, such as spending time in their lab, were able to get their lasers working.

The process of selecting equipment not only represents a distinct set of activities in the daily practice of scientists, but these activities are clearly linked to many other aspects of the scientific enterprise. The illustrations for each of the three subprocesses in this section demonstrate that selecting equipment is very much a process of human interaction and interpretation. Further research might look more closely at the way in which scientists' identities are linked to the types of equipment that they choose to work with. Traweek's (1988) work on high energy physicists and their detectors and Wolf's (1991) study of outlaw bikers and their Harley-Davidson motorcycles provide an excellent starting point for this kind of inquiry.

Acquiring Equipment

Once scientists have selected the appropriate equipment for the work that they wish to carry out, they have several options with respect to acquiring that equipment. Not only is this process closely linked to the selection processes just discussed, acquiring equipment is most clearly associated with processes of acquiring and allocating funds. The four subprocesses outlined here demonstrate the variety of social arrangements that scientists may enter into, as

they attempt to acquire the things that they have determined will help them to accomplish their goals. These are:

- Purchasing Commercial Equipment
- Borrowing Equipment from Others
- Accessing Shared Equipment
- Designing and Building

Purchasing Commercial Equipment

Those cases where equipment can be purchased off-the-shelf reflect situations where some other human enterprises (equipment manufacturers) have anticipated the (standardized) practices of the scientific community, and have produced a variety of materials that scientists regularly make use of in their work.

Sometimes in a situation like this you literally just buy a piece of equipment. It turns out that it's a packaged unit. You get it from some company, and you plug it in, and you read the manuals, and you know more or less that running the thing is usually not that difficult. (C3)

While this process may sound simple enough, in some instances, the actual purchasing process may be tied in with the grant application process and thereby be spread over a considerable period of time during which certain parameters may change.

I had to go to companies and get price quotes for machines, and submit them with the grant application. Then they didn't give me what – I think I asked for about thirty thousand, and they gave me twenty-two thousand to buy the machine. It was enough. The prices had tumbled in the interim, and I was able to get the machine and a laser printer as well. New technology had come out from the submission time to the acceptance time of the grant. (P2)

In this case, the scientist was able to respond to the situation in such a way as to get more than was originally intended. Other situations may reflect very different circumstances.

We asked three companies (A,B,C) to quote on certain equipment, and I went with A. It was the most expensive. We haven't gone with them since, because they were about 30% high. The thing is that I didn't know very much then. The only reason that we went with them is that their main facility was nearby, and if I needed something, I could go to them. (P3)

Here, the scientists knowingly traded greater financial output for easier access to service. The process of acquiring equipment is not as simple as trading money for material. Many other elements, such as service, are taken into account in making decisions to buy.

One situation in which commercial purchases may make the most sense is when there is a high level of standardization or regularity in the desired components (black boxes). Not only does this situation remove a level of uncertainty or ambiguity from the day-to-day work of scientists, it means that resources, financial or otherwise, can be allocated elsewhere.

You look at the optics, then you say to yourself, "How many components are going to be at a specific optical axis height?" And, in the majority of cases, you are going to go commercial, because it's going to be a lot cheaper to go commercial, and then we'll build the rest of them to match that optical axis height. (P3)

Components used by a number of scientists may even be available at in-house facilities run by the university itself. This is one way in which other members of the university community become collaborators in the scientific enterprise.

Concerning the electronics components, for the smaller ones – I mean, there are very popular ones that are readily available in the electronics shop. We just go and get it off the shelf – some wiring, connectors, and so on. They are already developed, and they are not expensive, so they are readily available to us. (P3)

In other cases, the decision to go to a commercial supplier may reflect an extended process during which scientists evaluate a number of variations of a product that were manufactured in different ways.

To get the things that we didn't have – essentially the detectors – for these, I started building my own detectors. Now that I've learned how, it's not a really complex project, but if you want to get a state of the art detector then probably what you need is someone who really knows how to improve – how to get the best out of these detectors. So, at times, the electronics shop builds detectors for us, but right now the way I would go is commercial – straight commercial – because what you get from a commercial instrument is going to be far superior, at least from what I've seen today, to what I can get either from the electronics shop or what I can build. (P3)

Here, the scientist assesses the different possibilities and determines, all things considered, the most beneficial course of action. Similarly, the process of investigating potential suppliers may lead to a reassessment of the situation.

It took us quite a bit of time to find this regulator, and I made several phone calls, and I found out who sells them and how much they cost. I wasn't sure whether I had that kind of money to spend seven hundred dollars - for something that we are not going to use frequently. (C2)

As with the process of selecting equipment, the activities around acquiring equipment reflect a complex interplay of individuals, along with an ongoing process of interpretation.

Eventually we ran into a French piece of equipment that people had been using in France and they had only sold four of them in North America. It turned out one of them had been sold to the person that I worked for as a post-doctoral fellow who was down in Texas. So, of course, I called him up and said, "Gee, how is it working?" You know, all those sorts of questions. And ultimately we ended up buying one ourselves and so we... that wasn't the whole part, that was just one little component... my post-doc and I and a couple of my grad students had sort of hooked up that component to the other components, rewritten some of the software ourselves, and, we sort of, over the last year I guess, put it all together. And now we're up and running. So it was a complicated interplay of talking to many people, bringing someone into my lab who knew a fair bit about this, and reading a lot of the literature as well, as to what sort of different things people were using. (C5)

Here, the acquisition of a piece of equipment can clearly be seen as intersubjective accomplishment, intricately tied in with a variety of other social processes.

Borrowing Equipment from Others

In some instances, scientists may borrow equipment from colleagues in order to try things out prior to making an investment. Even in those instances where scientists eventually incorporate new equipment into their own operation, these initial stages of research carried out on borrowed equipment do not merely represent trials or tests. In other words, scientific research activities do not begin only after scientists have incorporated equipment into their own setting. Rather, as can be seen in many of the cases presented throughout this chapter, the achievement of scientific goals is being carried out on a more or less continuous basis, in response to a variety of circumstances.

When we first started to do these experiments, we were actually borrowing equipment for those measurements from the physics department and from all over, other places, other universities. And now we've built up our own set up in our own lab to do the measurements here. (C5)

No one in the department had the equipment to make the quality of electrodes we needed, so what we did was go down to the research hospital on our bicycles about twice a week, pulling electrodes and then going back to our lab to use them. (B5)

We're very fortunate to have access to imaging equipment from a colleague down the hall. We could never afford to get our own, and besides, she does not use it that often. So, it is very convenient. (B7)

In certain situations, scientists may borrow a specific component that becomes part of a larger configuration of equipment.

I needed it (gauge), otherwise a quarter of a million dollar piece of equipment wouldn't work. In fact, I didn't buy it because it was expensive, and it took me a few days of looking around, wading through all the equipment, and I was fortunate enough to find one under somebody's sink in their lab. It was sitting there for ten years. Nobody used it. (C2)

In this case, a bit of searching around not only saved the scientist a significant outlay of money, but it had much broader consequences in terms of the overall situation and context within which the scientist operated.

Borrowing equipment can have significant consequences for the lender as well as the borrower, and these issues are taken into account when the decision to borrow or lend is made.

The danger is that, if something goes wrong... Uh, if I let you drive my car and something goes wrong, there is always going to be the suspicion that you were not driving it properly. That's why what happened happened. God forbid there is an accident, and then you're in trouble. We don't have the money to buy this instrument, and that's why you're getting somebody else's, and if it breaks while you're using it, where are you going to find ten or twenty thousand dollars to get it fixed. And then the problem gets to be more complicated, because you are affecting the principal investigator's research, because if his instrument goes down for two months, and it is your fault, what's going to happen to his research, and it gets to be more and more complicated. (C2)

As this case illustrates, borrowing equipment can be a high-risk venture from which a variety of consequences, intended or unintended, may emerge. In all of these cases, however, it is clear that the accomplishment of science is a collective enterprise, based on a diversity of relationships and associations.

I went there to do some work that he had very specialized equipment for. He could do quantitative analysis of our preparation live on video, and with that record you can get more data in a couple of days to get a handle on – well, I was able to eliminate certain ways of doing things as being impractical, or because they would not give us the information we need. So, I was able to go back to my lab and not have to worry about specific aspects of the project. (B5)

Accessing Shared Equipment

Sharon Traweek (1988) draws attention to the way in which the high energy physics community develops around the small number of sites, where the kind of research that they

engage in can be carried out. A similar situation obtains for observational astronomers, where a small number of telescopes and related devices are shared by scientists from many universities worldwide. In these cases, it is not the equipment itself that scientists acquire, rather it is "access" to the equipment. Whereas high energy physicists think in terms of "beamtime," many astronomers base much of their activity on "observing time."

I apply for observing time. I fill out a form in which I describe the science that is special about the project that I want to do, and I talk about how that science can't be done without that telescope. I specify the instrument I want to use. I specify why that telescope is the telescope of choice, and other things. So it's like a grant proposal in a sense - it's like a mini research paper. And, more often than not, your proposal is with two or three other people. You submit the proposal. It is refereed, in the same way that a manuscript for publication is refereed, and the referee evaluates the science - whether the science is useful, whether the project is going to be able to accomplish that science, whether it can be accomplished with that telescope, and whether your results are realistic. Then, you get a ranking, and depending on what you are asking for - for instance, if I'm looking at faint objects, I need to have time when the moon is not up, I'd like it to be a time of year when the objects that I'm looking at are up in the middle of the sky, and so they're up for the longest part of the night. So, that means I have about a week over two or three or four months, say March through May, for instance. Dark time is what I would say I want - sometimes. Well that's galaxy season in another hemisphere, so that means it's very competitive time. So, sometimes I have a success ratio of one in three, sometimes one in four. So, when you get that observing time, you essentially have been given a research grant. (P1)

This complex process closely resembles some the observations made regarding identifying sources of funding, preparing grant applications, entering partnerships and making adjustments discussed previously. Here, without going outside the broader community of scientists, we see evidence of how generic social processes provide a way to explore the range of activities that occur in different situations and contexts. For example, further exploration of this process could tell us a great deal about how scientists acquire perspectives, achieve identity, and form and coordinate associations.

On a smaller scale, access to shared equipment will often reflect some perception of how individuals fit into the group.

They knew I was qualified to use it, and once they got to know me, they gave me a key to the electron microscope suite, and I would go down and sign in for times during the day when it wasn't being used that heavily. (B5)

As with the longer quotation above, this brief observation points to the interpersonal aspect of accessing equipment, but also leaves us looking for more evidence. While the decision could

have been taken to leave out a discussion of this subprocess, an important aspect of the present project is to draw attention to the vast variety of activities that take place in science, while also highlighting the open-ended nature of this type of inquiry.

Designing and Building

There are occasions in scientific research when the equipment needed for some procedure is not known to exist, and so scientists decide to design and build such things from scratch. This process may involve taking several existing pieces of equipment and adapting them to new applications, or it may involve the “creation” of something totally new. For example, Collins (1992:80-81) describes the attempts by Joseph Weber to build a gravity wave detector by suspending a massive aluminum alloy bar on thin wire in a metal vacuum chamber, insulated with lead and rubber sheets. Similarly, Jed Buchwald (1995b:154) outlines how Heinrich Hertz “fabricated a clever device” to demonstrate that cathode rays were a very different phenomenon from what was thought by others working in the same laboratory.

In some instances, this whole process of putting together a piece of apparatus can consume the majority of the time allotted to some specific course of action.

The experiments and the equipment and so on are things that we have built ourselves, and therefore, that is to say, we do not use commercial instrumentation, which you simply install and then turn on and then put a sample in. That's, I think, a fairly common conception of how chemistry research is done. Somehow, you have an instrument like an NMR (nuclear magnetic resonance spectrometer), or something, and you put your sample in, and you measure your spectrum, and then you go away and think about it. And in our business, the majority of the work – you know 60 or 70% of the work is building the instrument, because it doesn't exist. That's the point. Once you build the research equipment, then you use it to measure something. (C7)

In other cases, only a portion of a complex array of equipment will be built to tie into existing pieces.

In the case we're speaking of, we actually had to build most of the equipment, in part from scratch. So, we started off on a multi-step route of kind of hooking together all sorts of bits and pieces, and what helped there was that I hired a post-doc from France from a group that already had a lot of experience in this area, and so I had a built-in expert in my group to a certain extent who had already been playing with different bits and pieces of this. And even then there wasn't... it turned out that for parts of this research there wasn't a lot of equipment out there in the market, exactly what we wanted. So, he spent a lot of time doing part of the design work on putting things together the way we wanted them. (C5)

The process of putting together these complex composites of equipment may be seen as resulting from the alignment of various material resources, but the critical process is the aligning of various human activities (and actors), in such a way as to accomplish specified goals.

As was illustrated in the case of purchasing equipment, in the designing and building of equipment, there are also opportunities for members of the broader university community to collaborate in the accomplishment of science.

I have become reasonably proficient with the machines downstairs in the shops, and when I need to build something I go and built it with the graduate students. If it is so sophisticated that it is beyond my capabilities, then I ask the machine shops downstairs to do it, and they are charging us at a considerably lesser rate, than when you go outside. Outside it is 45 dollars, and inside it is 15 dollars an hour for that kind of work. (P3)

The process of acquiring equipment encompasses a diversity of activities that demonstrate the ability of human actors to respond to the situations at hand, and collectively determine courses of action that will help them to accomplish their goals. These activities are part of the daily practice of science, and may have similarities to activities carried out in many other aspects of human group life. The skills and knowledge that actors use to carry out these activities can be considered no more or less scientific, than those associated with any realm of human endeavor. For example, it would be interesting to observe the activities taking place in a manufacturing facility, as equipment is purchased, constructed, modified, and/or adapted to allow the shift from the production of one type of product to another (e.g., from automobiles to tanks).

Operating Equipment

Even though we have the equipment, it is still slow work. We get four data points a day if we are lucky and if we aren't we get maybe one or two. Sometimes a day will go by that no matter what you do, nothing will happen, and then on other days it all fits into place. (B5)

How do scientists manage their equipment on a day-to-day basis? The answer to this question is tied into the processes of selecting and acquiring equipment, but it focuses more directly on how equipment is utilized in scientific practice. Collins (1992:74) argues that the

determination by scientists that their equipment is operating correctly is linked directly to the scientists obtaining the kinds of results that they anticipated they would get from the equipment. In other words, not only is it impossible to separate the equipment being used from the experimental procedure being performed, but the results of such experiments are directly linked to the operational parameters of the particular equipment. Our concern is not with the epistemological issues raised by scientists' equipment use, but with demonstrating the kinds of activities that scientists engage in as they work with this equipment to accomplish their goals.

As Nutch (1996) points out, some scientists may have minimal or only indirect contact with a piece of equipment, but it still may be an integral part of their operation, particularly when the broader work group is taken into account.

I have a workstation – it's licensed for 8 users. I use it now and then, but, for me, I use it primarily for e-mail, although it can do a number of other things. My graduate students all use it. They write their papers on it. They do their calculations on it, and it is also connected to the internet, of course. (P2)

In other instances, scientists may deal with certain pieces of equipment in a more sustained and direct manner. The data presented in this section are divided among two subprocesses that allow us to draw attention to the forms that these daily encounters may take, and to show how the activities engaged in are collective in nature. The subprocesses are:

Learning How to Operate Equipment
Maintaining Equipment

Learning How to Operate Equipment

In some ways the notion of operating equipment on a day-to-day basis does not accurately represent what takes place in the research setting. As new or improved equipment becomes available and as personnel changes take place in a particular setting, the operation of equipment centers around a process of continuous learning. This learning process may entail a slight adaptation of skills or knowledge previously learned, or it may involve a significant shift in the way that some aspect of doing science is conceptualized.

The microscope I had used before was an English design, and this new one was Dutch, and it was much easier to use. So, they had to show me that the knob that I was used to being over here is now over here, and that sort of thing -- more like an upgrading, than learning something new. (B5)

And the language, at least I found, is not too hard to learn. It's all typed in at an instructional level and text is typed in the way you would on an ordinary typewriter, but mathematical equations are typed using particular instructions, and parts of the equations look just the way you would write them down. Other parts look differently. Now at first it's a bit slow to type in the equation, but once you've written a paper, often another paper makes use of equations that are very similar. So, you can start copying and borrowing and, once you've got enough volume developed, you can write a paper very rapidly. In addition to this, as you write a paper you can see it evolve on the printer. (P2)

When I started observing, as a graduate student, I ran the telescope myself. I moved it around. I pushed buttons. I cut my own plates. Now, I go down and I sit in the control room. A telescope operator runs the telescope, and monitors what we're doing with the instrument, because he knows the instrument better than we do, and it's almost - not quite - to the point where I wouldn't really have to go down there anymore. (P1)

Not only do these last two examples represent very different levels of conceptualization, in terms of equipment and its use, they also illustrate the difference between a very "hands-on" approach to using equipment and a situation where a more "hands-off" approach is considered to be appropriate.

In other circumstances, scientists may definitely take this more hands-off approach to equipment, leaving the day-to-day operation, and the responsibility for passing this information on, to their students.

For the complicated things, I teach the students how to use it. It's not really that complicated, but for things that do need a bit of instruction - especially with something that involves electronics, or vacuum, and stuff like that - I teach the students. It doesn't take a heck of a lot of time - a couple of hours basically, so they understand the principles of the machine - how it reacts. Then, they have to have experience to be able to see it for themselves. So I don't spend a heck of a lot of time doing that, because once they learn it, students that I trust, and there's really quite a few of them, I'll ask them to pass that on to someone else who's wanting to learn how to use it. I figure they owe me that for having spent the time with them initially, and it work's out pretty well. (C3)

While this approach may have a decidedly pedagogical thrust, it also demonstrates how the various tasks to be performed in accomplishing goals are distributed among the members of a group in such a way as to maximize the use of resources.

Similarly, in some situations, new techniques that scientists have developed to get the most out of their instruments may lead to circumstances where direct human involvement at a certain level may actually impede the performance of some devices.

Astronomy has a much improved information capability in the last two decades, primarily because we are able to use improved detectors, CCD (charge-coupled device) detectors which are practically standard technology. Fifteen years ago they were extremely difficult things to use. Well you don't even look through the telescope anymore. My last observing run, I was using a telescope that I've been using off and on for quite a few years, but they now have done a lot to the dome environment itself, and so the control room is not even near the telescope. It's on a completely different level, to try to keep the level on which the telescope is as isolated from man-made influences as possible - heat sources, basically - which disturb the environment around the telescope. But, we now have this marvelous ability to get digital information, digital images. (P1)

This seeming non-involvement in the operation of equipment is also something that has to be learned and understood in the context of the overall conception of what constitutes research in this particular area of science.

There are occasions where the best way to conceptualize operating equipment may be in terms of what the equipment can and is doing for the scientist.

The new program that I was talking about has a variety of capabilities, but one of the things that it does that is going to make life so much easier for all of us is - normally, I would take this and I would do the photometry for this picture. Then, I have say three different pictures of this field with three different filters, and then I would have to mesh these data sets, so that I can inter-compare the brightness in one filter with the brightness in another, but all they are are numbers and coordinates. Then, I have to blend those data sets together. The way the system is working now, I have to do that myself. This program will allow you to pick a primary frame which defines the coordinates, so that every other frame you use will have the same coordinates, and then it will allow you to mesh the data much more easily, much more fluidly, and with a higher confidence that you're not matching two spurious objects to each other, and thinking you really haven't. So, it will help with the ease of operation, but it will also help with confidence on marginal detections, which is often where the exciting stuff is. You're always working to the limit of your instrument. So, we'll have better confidence with marginal detections, and we'll know that something was legitimately detected on this frame or this frame, but not on this one, because the system was looking for it in that location. It knew where to look and didn't find it, as opposed to doing it and then looking afterwards to see if it's there. I know people who have used this so far, and it's designed to allow us to do multiple - we'll have ten to fifteen exposures - ram them in and it just goes. So, it's much much easier, and the results will be a much higher level of confidence. But, there is something else. First of all, it takes a lot of disc space - you have to have all of these frames stored on your disc, and you have to have room for the intermediate stages in the process - a lot to be stored on a disc. So, even if somebody had developed the program five years ago, most people wouldn't have had the disc space on their computers to use it. (P1)

Here, the process of operating equipment is tied into broader notions of how a specific type of research is carried out. What may appear at first to be purely technical improvements are

hinged upon the understanding of a community of scientists, who integrate not only the results but all of the details of the process into their research activities. This case also illustrates how changes in one aspect of carrying out science influence and build upon changes in related aspects. Again, this points to the idea of a continuous learning environment around the operation of equipment.

Maintaining Equipment

Another way to look at how scientists manage equipment is with respect to the maintenance of that equipment. On occasion equipment may break or stop working. Sometimes it may require realignment or recalibration in order to keep operating according to specifications set by the manufacturer or the user. In any of these situations, scientists have a number of options, the most immediate of which may be to call in outside assistance, but they also may choose to do it themselves. Either way, a number of issues will be brought to bear in determining which course of action to take. So, for example,

My technician takes care of the equipment. He's learned to do those things over the years. I never touch it, and the students, particularly at the master's level, are here for too short of a time to be bothered fooling around with that. (B7)

In some cases, every aspect of the maintenance process will be taken care of "automatically" by someone else in a way that allows the users to carry on with their work unhindered by these concerns.

With a workstation, you need licensing from the company, and that connects you to a group of people who are on the campus, that service the machine, and install and upgrade the software. (P2)

It's really important to understand who's running these telescopes and how they're running it, and see what they're doing. Now, generally my experience is that these places are run extremely well, with less resources than they should have, of course, but the people who are working at these places are extremely skilled and quite committed, and they work very hard to make it possible for the rest of us to come there for a three or four night run, and get something out of it. (P1)

In both of these cases, maintenance issues may not be taken into account as regularly as when these kinds of arrangements do not exist.

Some scientists indicate that they will attempt to fix some things, where they feel that their knowledge or skills are adequate to the task.

As far as the maintenance goes, I'm not an electronics person so, if something goes wrong electronically, I will try to troubleshoot it as much as I can, given what I know about these things and what I can read in the manual, and then I take it over to an electronics shop and have somebody over there tell me what's wrong with it. (G2)

As far as regular maintenance goes, I do some of the minor stuff like changing bulbs and other things - aligning equipment, but when it comes to taking something totally apart, and then having to reassemble it; it's far more effective cost-wise for me to call in a professional to do it. If I have to spend a day taking apart a microscope and a professional could take it apart in three hours, and it costs me \$200, that's a far more effective use of the \$200 than having me sort of fooling around for a whole day, when I'm not even sure that I can reassemble it. (G3)

In both of these cases, the scientists will carry out maintenance activities to a certain point before enlisting the help of others. In the first case, the shift is made when the scientist determines that the limit of his own expertise has been reached. In the second case, the scientist assesses the situation more in terms of the allocation of resources. These two conditions are not necessarily mutually exclusive, but they do help to emphasize the array of possible interpretations that arise out of day-to-day activities in scientific practice.

In some situations, maintenance activities may be reduced to that of replacement.

When something breaks, it is replaced, and minimal time is consumed trying to determine what may be wrong or how something may be fixed.

I don't have to worry about electronics wearing out, and having to get expensive parts for electronics. These things are very simple. There are a couple of electronic things in there that will wear out, but they're affordable. Uh, by affordable, I mean you can replace things for \$1,000 or \$2,000... that's in the realm of affordability. Probably, it's not that tough to dig up \$1,000 or \$2,000. It would be tough to get \$100,000 for much more sophisticated electronics. (G3)

Here, the scientist is aware of the order of magnitude of the problem that may arise if something goes wrong, and this understanding is taken into account in the day-to-day operation of the scientist's work.

Scientists may also invest a limited amount of time in investigating a problem to establish what might be involved in repairing a piece of equipment.

I've taken apart a camera on one of my microscopes. The camera cost \$1,000, but I figured there's screws here, and I've got a screwdriver that fits, and let me see what it does. So, I took it apart and I moved a few things around, and it started working again. I would have been charged a service call for that, and it just turns out that it was one of those crazy little things

that happens with mechanical motors – sometimes they just - they don't move, something jams them - a piece of dust or something. So, you develop a little bit of confidence, and a little bit of cockiness. It can possibly backfire. So far, it has worked. I like tinkering with things anyhow, so I guess that's part of my nature, although I probably wouldn't let a grad student do that, unless I knew that they had some mechanical aptitude. (G3)

In this case, the consequences of exploration were positive, saving time and money. As indicated, in other situations, scientists may be reluctant to trust others to carry out such operations.

Part of the daily operation of equipment is tied in with following prescribed practices and ways of using particular devices. Any deviation from this normal mode of operation may lead to problems.

With proper care, I'll probably see it a couple of decades from now. This equipment is all hard mechanical metal, plastic, and glass parts, and so it will last a long time. And, you know, graduate students misuse it, turn it at the bottom with their hands, when they should be using a pliers to do it. I chew them out a little bit, and I say, "Look, you know this is not how you treat these things." In a nice way; I don't want them to think I'm an idiot. I say, "Look, this is the way I prefer to see it done." And, they're all amenable to keeping me on their side. (G3)

Thus, the concept of maintenance may be seen as synonymous with notions of proper and appropriate use. Yet, this aspect of managing equipment is just one part of the overall course of events in the research setting. The ways in which members of the group respond to objects in their environment are established and maintained through ongoing interaction and interpretation, and sometimes inappropriate behavior can lead to serious problems.

If you start fiddling around with things, they do break, and this is what happened with one of our spectrometers. You go back and trace the problem one step at a time and in trying to fix a problem you may create another one in the process, and those things do happen. They have happened before and it's not a question of slowing up your work, it is part of what happens. We had a problem with one of our spectrometers, with one of the cables. We ran out of space to get around the instrument. You had to almost lean against the instrument and as somebody was pushing against it, he pulled out one of the cables, but it was not pulled out all the way. It was partially pulled out, and it was not making contact at all. So, all of a sudden, the instrument is down and it's dead and we cannot take measurements, we cannot do anything, and it was just something as simple as that. We took the instrument apart and there just didn't seem to be anything wrong, until we started making serious measurements, building other test equipment, only to find out that inside the cable, there was a connector that was pulled back because somebody was trying to get around the instrument, and they just pulled it out. But there was two weeks of instrument time, and a lot of aggravation, and a lot of my time trying to

chase the problem down. And now it is not giving the performance it used to. So, we have to go all the way back and realign and retest and find out why. (C3)

There are some instances where the process of trying to get the most out of a piece of equipment can lead to a reconceptualization of the design and manufacture of the instrument.

Now, you can't believe the amount of time that was spent. We had to go and get – the way we got around it was funny. You see, a lot of people – not only us, but other people – have had problems with argon and krypton lasers from various companies. So, we put it on the (inter)net that we had these problems, and so the company got a hold of it and they were upset. So, they sent us the schematic. For some reason that they thought was important, they had built a magnetic field inside it – the whole beam was in a magnetic field. And, they said that they were doing that because it will boost the intensity of the beam. We just turned it off, and we saw no loss in intensity, and the modulation dropped to reasonable levels. But, while we were in there, we said that the other thing that causes the modulation in these systems is that they use an ac (alternating current) filament. So, we switched it from an ac to a dc (direct current) – we had to build another power supply, but now we have no modulation whatsoever. It works quite well. (P3)

Here, the effort to rectify an equipment problem led to collective action involving not just the user and the manufacturer of the equipment, but members of the broader community of users of such devices. The “solution” to the problem was arrived at through intersubjective processes of interaction and interpretation that arose out of the attempt to accomplish certain goals. None of this may appear controversial or out of the ordinary, and part of the point being made here is that it is not. Rather, I have been drawing attention to these mundane and common sense elements of the scientific enterprise because they have largely been ignored by other researchers, but that is not the only reason. If we are genuinely interested in knowing what goes on in science, and in determining how the activities of scientists compare with what takes place in other human groups, then the details of these everyday activities need to be documented.

The whole area of operating equipment, both in terms of regular day-to-day operation and maintenance, can be seen to reflect the very human nature of the scientific enterprise. The environment of scientific practice is not constructed solely out of the materials and machines that take up physical space in the research setting. Rather, scientific practice emerges out of the ways that scientists interpret these objects, and communicate their interpretations to others through ongoing symbolic interaction.

Discussion

As with the preceding chapter on pursuing funding, the present chapter draws attention to a neglected aspect of the scientific enterprise and provides details of the mundane and everyday activities that scientists engage in associated with it. In dealing with scientists' use of equipment, a number of studies have emphasized the link between science and technology (see Latour, 1987; Giere, 1993), while others have focused on the role of instruments and equipment in scientific experiments (see Gooding, Pinch, and Schaffer, 1989; Galison and Hevly, 1992; Buchwald, 1995a). What has generally been missing from these studies is evidence of how practicing scientists deal with these objects in the here and now. In order to assess how the material presented in this chapter contributes to our understanding of the daily practice of science, it will be helpful to return to three of the issues introduced at the outset, namely, the link between technology and science, the notion of a black box, and the idea of non-human agency.

As discussed in Chapter 4, Bruno Latour (1987) collapses science and technology into what he calls technoscience, arguing that scientists make the distinction between science and technology as part of a rhetorical strategy to establish and maintain occupational boundaries and control the allocation of resources. The material presented in this chapter lends support to Latour's argument on at least two counts, but a more nuanced conception of the term technoscience is required. First, the way in which the distinction between science and technology is used to differentiate between the work carried out by scientists and that carried out by technicians varies on a practical, and not just a rhetorical, level. Second, particular situations and contexts of scientific practice differ with respect to how much and what kind of technology, if any, is employed. Therefore, to present a monolithic conception of technoscience distorts what actually takes place in the day-to-day practice of science. At the same time, however, the term is useful because it may not be possible on the basis of practice alone for scientists or those that study science to differentiate in an absolute sense between science and technology.

In their study of technicians in scientific laboratories, Barley and Bechky (1994) demonstrate how the work that technicians carry out helps to make the work of scientists more manageable. In other words, scientists can concentrate on what some might consider the more

“scientific” aspects of research, while the technicians take care of the mundane, routine, and well-established elements of the work of the laboratory. However, as the data presented here indicate, there are a number of similarities and a great deal of overlap in the activities carried out by scientists and by technicians. For example, members of both groups establish preferred routines with respect to the regular operation of equipment, and members of both groups engage in troubleshooting when equipment is not operating as expected. Also, the criteria used to determine when to turn a particular task over to a technician, or to a scientist, appear to vary with the situation and the context. As a result, there is no way for us to determine in an absolute or objective manner when an activity is “merely” technical or when it can be considered scientific. A more detailed comparison of the activities of these two groups will not only provide us with a more comprehensive understanding of how science is accomplished on a daily basis, it will help us to understand more fully the ways in which the identities of scientists and technicians are constructed.

The data also show that, while some scientists employ elaborate and expensive equipment in their research, others carry out what might be considered very low technology research. We cannot, however, assume from this distinction that high technology science is somehow “more” or “less” scientific than low technology science. What we can point out is that the daily routines of scientists differ markedly depending on the amount and complexity of technology that they use in carrying out their research. Similarly, the more generic and process-oriented approach used here allows us to demonstrate how equipment-related activities are tied in with activities around pursuing funding, covered in the previous chapter, and also with the broader range of activities, such as getting ideas and assessing feasibility, covered in Chapter 6. Further, we can see from the data that technology may impact one portion of the scientists' work and not others. This is particularly the case with computer technology. As one scientist indicated, computers have facilitated the way in which calculations are carried out, papers are written, and scientific information is disseminated. Technically mediated changes in the ways that these activities are carried out mean that scientists are in a position to alter their

interpretation of these activities, and potentially alter the way they carry out many other aspects of their work on a day-to-day basis. This issue remains to be explored.

Turning now to the notion of the black box, Latour (1987) indicates that there are two critical features of these devices. First, they produce a predictable and consistent output from a given input, and second their internal workings are taken for granted or considered to be unproblematic by the broader community of scientists. In their study of the plasmid prep, Jordan and Lynch (1992) call these assumptions into question and posit the notion of a "translucent box," in order to emphasize both the socially constructed and dynamic nature of the devices that are labeled this way. The data presented here support the idea that the predictability and consistency of operation associated with these devices varies directly with the level of understanding of these objects possessed by the scientists who employ them. In one case, a scientist recruited a post-doc from another country because of his familiarity with a particular piece of equipment. As argued by Collins (1992), the ability to construct, operate, and maintain scientific equipment depends on the tacit knowledge of its users, which they can only acquire through direct contact with other scientists. Similarly, both the physical make-up of equipment and the uses to which it is put are undergoing changes on a regular basis as individuals gain more experience with it, and as others enter or leave the research group. Thus, a more accurate conception of a black box is to think of it not just in terms of its role as one piece in a larger epistemological puzzle, but rather in terms of its more nuanced real role in the daily practice of science. Through processes of interaction and interpretation, scientists respond to these black boxes within the situation and context that they find themselves in, and that are subject to constant change. Rather than being focal points of "forgetting," equipment often serves, for the scientists, as a focal point of awareness, of "reminding" them that scientific knowledge is constructed, negotiated, and so on. As Dolby (1996:42) argues, "a perception, like seeing an instrumental reading of '167.39', gains its significance in the context of understanding the instrument used in context."

The third issue that concerns us is the debate over non-human agency, or, as it is often presented, the debate over whether to treat humans and non-humans (machines, animals, and

so on) symmetrically with respect to the production of scientific knowledge. As has been argued throughout, a symbolic interactionist approach to the study of science is opposed to any notion of non-human agency, and I do not limit the goal of scientific activity to the production of knowledge. The data presented in this chapter demonstrate the intersubjective accomplishment of daily practice, as scientists encounter the recalcitrance and obdurateness of material objects in their environments. In contrast to actor-network approaches to science that advocate treating human and non-human agency symmetrically the present project emphasizes that what is important to observe is how scientists take these objects into account. As the observations on selecting, acquiring, and operating equipment show, activities are carried out collectively in ways that reflect what the objects mean to the scientists. To state the point another way, we are not interested in what equipment does but rather with what scientists do with it in practice. A symbolic interactionist approach to science focuses on the emergent and processual aspects of the enterprise, and that is why I have been describing science as what scientists do. The intention in this is not to be overly simplistic or solipsistic, but rather to provide a mechanism that allows us to step away from some of the other approaches to science studies discussed throughout this work, to get at the day-to-day practice of science more directly.

The following list of activities reflects one way to frame this aspect of the scientific enterprise in order to facilitate analysis and communication.

- Selecting Equipment
 - Adhering to Standards
 - Working within Limitations
 - Learning from Others
- Acquiring Equipment
 - Purchasing Commercial Equipment
 - Borrowing Equipment from Others
 - Accessing Shared Equipment
 - Designing and Building
- Operating Equipment
 - Learning How to Operate Equipment
 - Maintaining Equipment

These categories help to emphasize the observation that science involves far more than just carrying out experiments. The steps that lead up to experimentation, particularly with respect to constructing the apparatus through which experiments will be carried out, are just as intrinsically a part of science as the experimental events themselves.

In order to illustrate how some of the processes just listed may resemble what goes on in other areas of human endeavor, I first want to turn to a study of sex workers carried out by Lisa Jean Moore (1997). Moore interviewed seventeen women and two men, who “exchange(d) sexual services for economic compensation,” and who employed various “technologies” to avoid contracting disease. As with the scientists interviewed for the present study, the sex workers described a variety of activities that they engaged in with respect to the selection, acquisition, and operation of a number of devices that were an integral part of their daily work. The “tools” they used included male and female condoms, latex gloves, dental dams, and plastic wrap, and the uses to which these devices were put varied with the imagination, confidence, attitude, and experience of the worker and the client. In other words, the meanings of these devices arose out of the processes of interaction and interpretation engaged in by the parties involved in particular situations and contexts. Just as scientists attempted to gain consistency and predictability of operation of equipment by adhering to standards, sex workers reported that they sometimes contact the CDC (Centers for Disease Control) for the latest test results with respect to the safety of the devices they use and practices they engage in. Unlike some scientists, sex workers are not likely to design and build their own equipment, however, they do adapt products designed for seemingly unrelated applications (e.g., dental dams) to their own needs. With respect to purchasing commercial “off-the-shelf” equipment, one sex worker indicated that it made sense to him to purchase condoms by the thousand, for eighty-eight dollars, plus shipping and handling. Another indicated that she had a preferred supplier who would ensure that her favorite brand was always available in the quantities she required. While an outsider may view the male condom, for example, as a black box with rather limited and predictable operational parameters, the sex workers indicated that the efficient and effective use of this device was highly dependent on their ability, acquired mainly through experience, to respond to such factors as differences in male anatomy, the nature of the particular sex act to be performed, and the experience level of the client. Here again the evidence indicates that a more useful conceptualization of the equipment used by members of various groups to achieve their goals is with respect to the meanings that are attributed to these objects in practice, and not with respect to the objects themselves.

As with the detectors employed by the high-energy physicists (Traweek, 1988), Wolf (1991) points out that motorcycles act as "subcultural identity markers" for the biker gangs that ride them. That is, by learning from others and adhering to standards, scientists and bikers are not only selecting equipment they are engaging in the construction and maintenance of the perspectives and identities of the groups of which they are a part. For example, Wolf (1991:13) indicates that when he was trying to make contact with a gang, a group of bikers told him that in order to be taken seriously, he would have to get rid of his Norton (a British made motorcycle) and get a "real" bike (a Harley-Davidson). Just as scientists and sex workers look for specific characteristics when selecting equipment, even seemingly simple off-the-shelf items, bikers are faced with a number of options when selecting a Harley-Davidson. For example, "hogs" (the bikers' term of endearment for their machines) come in many models, including panheads (the engine block design common from 1948 to 1965) and shovelheads (1963 to 1984), each with its own performance strengths and limitations.

As is the case with scientific equipment, acquiring a bike involves more than just the direct purchase of a motorcycle from the appropriate manufacturer, usually through a dealer. Rather, the acquisition process involves a number of steps through which individual bikers customize their machines to reflect their own identity. In many cases, bikers will purchase one or more old bikes that are no longer road worthy, or they may literally build a bike from the ground up, one piece at a time. Bikers refer to the process of customizing a bike as "chopping," hence the term "chopper" used to describe a bike that has been stripped of all accessories and non-essential parts, had its front end extended, and received custom body work and painting. The precise modifications that are made will reflect the economic resources of the individual, the personal style of that individual, and the application for which the bike is being built. These activities directly parallel the ways that scientists, purchase, borrow, design and build the experimental set-ups that they use to carry out their research, and establish their individual place within the community of scientists.

Just as scientists cannot carry out their work if their equipment is not operating correctly, bikers are unable to ride if their machines break down. These interruptions in daily practice can

have serious consequences for members of both groups. When bikers cannot ride, they risk losing membership in their club. When scientists cannot carry out the experiments and procedures that are part of their work, their careers may be altered in ways that are contrary to their goals and aspirations. Both bikers and scientists may "tinker" (see, Knorr-Cetina, 1979) with their machines themselves, they may call upon colleagues for help, or they may leave the problem to specially trained technicians. Just as the lives of high-energy physicists are tied into the "beamtime" they can acquire, bikers "live to ride and ride to live" (Wolf, 1991:43). For the physicists, their detectors must be ready to go when the "precious allotment" (Traweek, 1988:49) of beamtime comes available, while for the bikers, as one set of club by-laws indicates, "all bikes must be on the road by April 30th" (Wolf, 1991:358). The point to be made here, however, is that it is the processes of social interaction and interpretation, precipitated by dealing with machines, that constitute the members' identities and accomplishments.

The activities outlined in this chapter also provide a basis for studying a variety of other groups that we may associate with a reliance on equipment or special facilities. For example, it would be interesting to study a rock band on the road, the efforts to mobilize a major military support mission, or the running of the surgical and diagnostic departments of a hospital. More specifically, when a new diagnostic procedure (e.g., MRI - magnetic resonance imaging) which involves an elaborate piece of equipment is introduced into a hospital setting, we are presented with an opportunity to examine the dynamics of the relationships among, administrators, doctors, nurses, technicians, custodial staff, and patients, as they take the operation and maintenance of the new device into account in their daily lives. Regardless of the specific focus, a symbolic interactionist perspective and the employment of the generic social process scheme allow us to obtain the maximum benefit from such studies, and thereby gain greater insight into the nature of human group life, in this case, as universally conditioned by our interactions with machines.

Chapter Nine

EVALUATION AND IMPLICATIONS

Throughout this work, I have repeatedly argued that what goes on in science on a day-to-day basis is best viewed as arising out of the intersubjective accomplishment of practicing scientists, as they endeavor to achieve the goals that they have set for themselves. In support of this contention, I have provided several examples of scientists' own descriptions of their activities in the here and now, without trying to ground these observations in the context of some preconceived historical, philosophical, or scientific goal. Rather, the observations have been contextualized within an emergent and flexible heuristic framework that aids the communication and interpretation of empirical evidence through the development of generic social processes.

The notion of intersubjective accomplishment implies that the performance of activities and the achievement of goals cannot be viewed as either a wholly subjective or objective phenomenon. Rather, human achievement is collective, arising out of the processes of interaction and interpretation in which people continually engage. This conception of social enterprise obtains for all realms of human group life, including science. Thus, for certain purposes, the study of the scientific enterprise will resemble the study of any other aspect of human activity. What differentiates one study from another are the particular circumstances that the researcher encounters in the specific context and situation being observed.

In the second chapter, I introduced the problem of trying to differentiate between science and non-science (the so-called demarcation problem). The present study supports the notion that there is no one way to conceive of science and consequently there is no one way to study science. The choice was made to conceive of science in operational terms, as what scientists do. In other words, science is made up of the everyday practical activities in which scientists engage. Coupled with this conception of science, I advocated an alternate approach to studying science that I described as a social study of science. That is, this study was based on the idea that science must be examined within the specific contexts and situations in which it takes place, it must include practicing scientists as research subjects, and it must not assume that the only

goal of scientific activity is the pursuit of truth. Other approaches to the study of science may conceive of things differently, but the choices made here were made in support of a symbolic interactionist approach to science, that would respect the human nature of the scientific enterprise.

Turning now to the three specific goals of this project, the first goal was to establish the basis for a symbolic interactionist approach to science by demonstrating what such an approach would entail. The most significant aspect of this approach is the notion that people act toward the objects they encounter in everyday life on the basis of the meanings that those objects have for them. For instance, in response to receiving fewer dollars of government funding than had been applied for, one chemist commented that other sources would have to be sought, while a biologist indicated that the original plan for spending the money would have to be altered. That is, the same event (a funding cut) evokes similar but different adaptive responses. Until we have documented many of these alternative courses of action, we cannot really claim to know enough about how science is actually carried out on a daily basis. Much of what goes on in science reflects a pragmatic response to particular sets of circumstances. Thus, one geologist indicated that, for simple mechanical problems, he was willing to service his own equipment, but for complex electronic problems he would call in someone with the appropriate expertise. Of equal importance is the notion that meanings emerge and change through ongoing processes of interaction and interpretation. One geologist indicated that in response to the expectations of funding agencies, the decision was eventually taken to drop out of the competition altogether, and operate on a limited budget, rather than spend one-third of his time looking for money. By incorporating the generic social process scheme, researchers have a mechanism which allows them not just to carry out fieldwork within an interactionist perspective, but it also provides them with a way to communicate their findings in an effective and efficient manner. This benefits all those who may be interested in the results of such research and especially those who might wish to incorporate some element of the findings into their own research projects, in or outside of the sociology of science.

The second goal of this study was to carry out a fieldwork project that would demonstrate the instructive quality of the symbolic interactionist approach for the study of science. Chapter 6 presented a brief survey of the diversity of activities that academic research scientists engage in on a daily basis. This presentation was organized around the generic social process of “performing activities,” in order to provide a framework for analysis and communication, and, based on initial findings, this process was further divided into a number of subprocesses that reflect in more detail the cycle of activities that constitute science, namely, getting ideas, assessing feasibility, getting prepared, implementing plans, obtaining results, and disseminating results. While this scheme might appear to impose a certain linearity or inherent logic to scientific practice, this is solely for the benefit of those studying science. The point of this scheme is to emphasize the fact that scientific practice takes place in time, that is, it is emergent rather than static. Furthermore, it emphasizes the fact that it is the process of science and not the products of science that provide the greatest insight into what science is. Among the reasons for this is the observation that many details of what takes place in science are “forgotten” when it comes to writing up work for publication. It is also important to recognize that scientists may be involved in any or all of these activities simultaneously. Finally, the set of processes outlined here is not the only such framework that would be consistent with a symbolic interactionist approach to science. Some other set of processes, reflecting a different experience in the research process, may be just as valid.

Generally, the data presented in Chapter 6 demonstrate the contingent and pragmatic nature of scientific activity that does not readily emerge when science is approached from some predetermined or teleological perspective. While many historical, philosophical, and sociological approaches to science may provide accurate portrayals of what goes on in science, some studies of science have demonstrated a mismatch between various ideological conceptions of science, whether historical, philosophical, or sociological, and what actually takes place in science. The distinct merit of the symbolic interactionist approach in this regard is that, by eschewing *a priori* assumptions and concentrating on process, a naturalistic picture of science is allowed to emerge that may look very different from our expectations or from what scientists or others might want

us to believe. Once the picture emerges, the symbolic interactionist does not have to waste time modifying assumptions, looking for alternate explanations, or blaming the respondents, i.e., rewriting history or engaging in philosophical speculation. The social order that manifests itself does so because it is an intersubjective accomplishment of those engaged in achieving goals within a particular life-world. What the data in this chapter point to is an order comprised of such a diversity of activities, that more narrowly-defined conceptions of science, i.e., as method, would dismiss the vast majority of what goes on in the scientific enterprise. To then argue that scientists only get to "do science" a small portion of their time would be to miss and misrepresent the way that science is carried out. The symbolic interactionist approach, more so than any other, allows us to explore in a balanced and open manner this broader conception of the scientific enterprise.

Similarly, the data illustrate the messiness and ambiguity of daily practice, the constant uncertainty (change) that scientists deal with, and the way in which decisions on how to proceed emerge out of social interaction and interpretation. By focusing on how scientists spend their time, those that study science can gain a better understanding of how scientists resemble members of other social groups, for certainly all aspects of social life are messy and ambiguous in some ways. However, when more sustained attention has been paid to the kind of exploration advocated here, we might be in a position to indicate with some degree of confidence those activities that differentiate science from other forms of collective behavior. At the same time, it is consistent with this approach to expect, that some of those elements that are distinct to science today may not be distinct to science at some future time, nor may they have been distinct to science at some time in the past. As with Fleck's notion of the passive elements of science, even those aspects of science that certain generations of scientists treat as firmly established, or given, are still subject to change.

At several points throughout this study, I have indicated that many of those who started out to explore science in the field have since turned to historical and philosophical studies, either as a way to explain the problems they uncovered in their fieldwork, or because they could see no clear reason for returning to the field. As has been just argued the symbolic interactionist

approach, not only demonstrates the value of doing more fieldwork, it provides a suitable method for carrying it out. The data in Chapter 6 illustrate the tremendous potential for more sustained research into any and all of the activities and processes identified. For example, with the process of getting prepared, it would be interesting to investigate in greater detail how scientists learn new technologies and/or techniques that can be used to further their research objectives. Similarly, with respect to the actual implementation of research plans, it would be valuable to examine more closely the actual, and not just the assumed, data collection activities of scientists. By taking an interactionist approach to such studies, researchers will be providing us with a greater understanding of science and every aspect of human group life.

The third goal was to present data on some aspects of the scientific enterprise that have not received adequate attention, and two such areas were covered, namely, pursuing funding (Chapter 7), and managing equipment (Chapter 8). Each of these chapters amounts to a self-contained exploration of one aspect of scientific practice. In each case, the data reinforce a conception of science as intersubjective accomplishment, and we gain new insights into the ways in which scientists deal with these issues on a day-to-day basis. Chapter 7 illustrates the ways that scientists identify sources of funding, apply for funding, seek out alternate sources of funding, market materials and services, and make adjustments based on their success in acquiring funding. The data also provide confirmation and elaboration of a number of observations regarding funding made in other studies of science. First, the process of pursuing funding is pluralistic, not just in the sense that scientists seek funds from multiple sources, but also from the point of view that scientists take into account the diversity of perspectives and objectives that these different sources represent and respond accordingly. Second, funding sources differ with respect to whether they fund specific people, projects, or problems. Scientists take these differences into account when determining what research path to follow, how to present it, and which possible source to present it to. Third, there is often a knowledge gap, both with respect to the scientific content of proposed research and regarding the workings of some particular agency, between scientists and those that evaluate their proposals for funding. Further, the data suggest that it may make more sense to speak of the intersubjective

accomplishment of science, rather than the social construction of science. Part of the reason for this is that the use of the word "social" maintains the fundamental asymmetry between the cognitive and social elements of science that was assumed by many of those who studied science, but which the evidence does not support. Further, the word "construction," or some variation on it, has not only been subject to numerous interpretations, it has come to be a sort of pejorative collective term for a variety of what some consider to be anti-science initiatives (see the discussion of the so-called "science wars" that follows). Chapter 8 documents how scientists design, build, operate, and maintain the equipment that they use to probe and analyze the objects they select to study. The data also provide a basis for a reconceptualization of a number of notions commonly employed in science studies. First, the data point to the importance of maintaining and exploring further the distinction made in practice between science and technology. Second, they suggest that the notion of a black box is more accurately conceived as a matrix of meanings associated with an object, rather than with the object itself. Third, the evidence indicates that the notion of non-human agency is an unnecessary and distorting construct that serves to deflect attention away from the sustained examination of how the meanings that scientists attribute to objects emerge as they go about accomplishing their goals.

While I would contend that this project has met the three goals that acted as starting points for my research, I would also contend that the project is a long way from complete. An interactionist approach to science, or any other realm of human group life, is by definition open-ended, and any notion of completeness is both naive and inconsistent with this approach. Just as scientific practice is seen to emerge out of the day-to-day activities of its practitioners, a genuine social study of science is an ongoing process. Definitive statements about the nature of science are antithetical to an interactionist perspective, and thus researchers must guard against any temptation to engage in reductionism, not only with respect to the area of research, but also with respect to the application of a research tool like the generic social process scheme.

At several points throughout this study lists of generic social processes are presented through which the activities of scientists are illustrated. The purpose of these lists is to provide for conceptual development, when comparing what takes place in different situations and

contexts, and for ease of communication about the findings that emerge within the setting. At no time, should the researcher, or anyone reading the results, assume that these processes describe exactly what happens in science in any comprehensive manner, or that everything that takes place in science can be fit into these processes. The generic social process scheme is a heuristic device and not some kind of positivistic typology of scientific practice. The scheme is used to get at the daily activity of human groups, and many alternate sets of processes may be just as effective for drawing out these details. The specific set of processes used in this project reflect my interpretation of the original scheme as it was laid out by Robert Prus (1987, 1996) and of the activities that were described to me by the scientists interviewed for this study.

One of the primary advantages of the GSP scheme is that it allows the researcher to highlight the specifics of some situation or other, without sacrificing the ability to compare these findings to what takes place elsewhere. In this sense, there is good reason to maintain the basic outline as provided by Prus. At the same time, however, the present project illustrates how even within one subprocess there is literally no limit to the further delineations that can evolve out of the effort to describe social life. One potential drawback with the GSP scheme, then, is that researchers may engage in the endless proliferation of processes, as a substantial sociological project in itself. Another potential problem is that, as more researchers attempt to use this approach, too much effort might be expended in trying to determine which set of processes best fits the data. An even greater threat is that researchers will waste time arguing over which particular set of processes is superior, with no recourse to what actually takes place in daily life. As has already been stated, dogmatism and rigid typologies will not further our understanding of science or any other aspect of human group life. The point of a generic social process scheme is to allow the researcher to get a hold on some aspect of human group life, in a way that facilitates communication across substantive concerns.

Many of the scholars whose works have been examined in the preceding chapters have indicated that in order for the sociology of science to emerge as a significant area of research within sociology, and within the broader range of historical, philosophical, and social studies of science, more fieldwork needs to be done. I would agree with this, but I would contend that no

amount of fieldwork is going to contribute to our understanding of science if there is no effective way of incorporating the findings of such research projects into a broader, more comprehensive plan for studying science. A symbolic interactionist approach to science that makes use of the generic social process scheme is capable of sustaining continued and complementary field studies of all aspects of science, as well as other areas of human group life. For instance, what we learn about assessing the feasibility of possible courses of action from scientists might help us to understand how symphony orchestras plan their concert season, or how service clubs determine in what ways they will be able to help the communities they serve.

Perhaps the most obvious place to start, in terms of continuing the research begun in this study, would be to expand the sample in terms of numbers and amount of coverage. That is, the sample could be numerically expanded to include other biologists, chemists, geologists, and physicists from the same institutions, or from other locations. At the same time, members of other scientific groups (e.g., pharmacologists and psychologists) could be included, along with technicians, post-doctoral researchers and graduate students. As a next step, directors of research in industry and institutions of higher learning, board members of various granting agencies, and bureaucrats in government departments that deal with science policy or funding issues could be added. Even staying with the existing sample group, the amount of coverage could be expanded by spending more time with some or all of these individuals to learn more about the activities discussed here, or to investigate other issues. One interesting possibility for further research that moves away from a strict focus on science would be to take a single process or activity and look for evidence of this activity in a number of different human groups. This kind of study would allow social researchers to develop a more general appreciation of the intersubjective accomplishment of human goals, and would demonstrate more broadly how researchers with diverse interests could contribute to the accomplishment of each other's goals.

The Science Wars

A recent conflict arising from the social study of science displays graphically the urgency of adopting the approach to the sociology of science advocated in this project. In recent years,

the so-called "science wars" have broken out in some scholarly journals, the popular press, and at numerous conferences around the world. Nick Jardine and Marina Frasca-Spada (1997:219) trace the origin of these wars to three related events. First, biologist Paul Gross and mathematician Norman Levitt published the book *Higher Superstition* (1994), accusing an "academic left," made up of postmodernists, feminists, multiculturalists, and radical environmentalists, of seeking to undermine the foundations of science. They (1994:2) argue that these initiatives are characterized not just by "open hostility" toward the content of scientific knowledge, but "toward the assumption, which one might have supposed universal among educated people, that scientific knowledge is reasonably reliable and rests on a sound methodology." This statement appears bold, if not uninformed, when we reflect back on the material presented in Chapter 2 with respect to a number of ongoing and unresolved debates over various aspects of science. For example, there is no clear consensus, even among scientists (see Jardine and Frasca-Spada, 1997:229), on how to demarcate (on the basis of methodology or otherwise) science from non-science, just as there is no clear understanding of the relationship between theory and evidence. Gross and Levitt (1994:4) go on to argue that the greatest danger arising from these initiatives is not with respect to science itself, but rather:

What is threatened is the capability of the larger culture, which embraces the mass media as well as the more serious processes of education, to interact fruitfully with the sciences, to draw insight from scientific advances, and, above all, to evaluate science intelligently.

So, not only do the authors present and defend a monolithic and largely taken-for-granted conception of science, they are of the opinion that science is, for the most part, immune from the attacks of those in the academy who do not share this view. For Gross and Levitt, it is the public that is at greatest risk. What the authors appear to argue is that, in order for the public to evaluate and benefit from the work done by scientists, and at the same time protect themselves from the anti-science rhetoric of the "left," members of the public must accept the scientists' definition of what science is.

Gross and Levitt construct their argument around the analysis of what they have determined to be four distinct but related efforts to assess science from within the broader category of cultural studies: social (cultural) constructivists, postmodernists, feminists, and

radical environmentalists. These areas are distinct in that they all reflect certain “doctrinal idiosyncracies” (e.g., sexism on the part of feminists), but they are related in as much as they all represent some form of radical relativism, with respect to the ontological, epistemological, and methodological foundations of modern Western science. So, for example, Gross and Levitt select the work of sociologists and historians of science Stanley Aronowitz, Bruno Latour, Steve Shapin, and Simon Schaffer, to represent the social constructivist perspective. What they argue is that these scholars are attempting to demonstrate how social and political factors, particularly as they relate to the struggle over material and human resources, turn science into a power game, in which references to reason and evidence are merely rhetorical tools. In an effort to turn the tables and point out the “value-laden” nature of social constructivism, Gross and Levitt (1994:69) conclude that:

The central ambition of the cultural constructivist program – to explain the deepest and most enduring insights of science as a corollary of social assumptions and ideological agenda – is futile and perverse. The chances are excellent, however, that one can account for the intellectual phenomenon of cultural constructivism *itself* in precisely such terms. (emphasis in original)

Here, we find, notwithstanding any intended irony or sarcasm, the reiteration of a basic tenet of some versions of the sociology of knowledge discussed earlier, whereby it is thought quite legitimate for sociology to investigate the social aspects of science (context of discovery), but not the internal workings of science (context of justification).

For their analysis of postmodernism, Gross and Levitt select philosopher Steven Best, Andrew Ross, co-editor of *Social Text*, who coined the term “science wars,” and literary critic N. Katherine Hayles. Postmodernist scholars are accused of treating “science as metaphor,” and of playing a sort of “intellectual hooky,” thinly disguised as philosophy. According to Gross and Levitt (1994:74), most postmodernists hold that

“reality” is chimerical or at best inaccessible to human cognition, and that all human awareness is a creature and a prisoner of the language games that encode it, (thus) it is a short step to the belief that mastery over words, over terminology and lexicon, is mastery over the world.

The catch is that, in the opinion of Gross and Levitt, postmodernists rarely make the effort to understand the meanings of the words they use in their critiques, for those that use them. In other words, before some term used by physicists, say, can be used in some other context, it is

incumbent upon the borrower to first discover what physicists mean by that term. So, for example, both Best and Hayles are accused of totally distorting what physicists and mathematicians understand by "chaos theory," and its related vocabulary of nonlinearity, fractal geometry, and so on, thus reverting to the "magical, emblematic thinking of premodern (rather than postmodern) times" (Gross and Levitt, 1994:105). Postmodernists, however, freed from the ideological encumbrances of scientists, claim to see more "meaning" in these words than even the scientists are aware of.

Virtually all of them claim to discern important intellectual themes and political motifs in past and current science, themes and motifs that are quite invisible to the scientists themselves. These supposed insights rest, as we have seen, on a technical competence so shallow and incomplete as to be analytically worthless. Their arrogance, then, is comparable to that of "creation scientists" in addressing evolutionary biology, or to that of Galileo's persecutors within the Inquisition in their response to his cosmology. (Gross and Levitt, 1994:106)

Again, as with their critique of the social constructivists, Gross and Levitt are supporting an internalist conception of science that is assumed to be true, and cannot be proven to be false except on its own terms.

With respect to feminism (represented by Sandra Harding, Helen Longino, and others), the argument is basically the same. Gross and Levitt (1994:148) argue that:

What begins as an epistemological inquiry into science ends as familiar anti-science tricked out in the ambient cliches of the business – science "harnessed to the making of money and the waging of war" – the old moral one-upswomanship, and the call to political action. It ends with the universal complaint of religious zealots, utopians, and totalizers generally. (emphasis in the original)

All members of the "academic left" are viewed as "totalizers," who Gross and Levitt (1994:225) define as those with "the impulse to bring the entire range of human phenomena within the rubric of a favored doctrinal system." Science, for Gross and Levitt, is not doctrine, as it is based on logical reasoning and the rules of evidence. Similarly, the "actions" of science are superior to the "talk" of other disciplines. As they (1994:178) conclude with respect to so-called radical environmentalists (represented primarily by Carolyn Merchant), "it is self-evident that a 1 percent improvement in the efficiency of photo-voltaic cells, say, is, in environmental terms, worth substantially more than all the utopian eco-babble ever published."

The remainder of their text is devoted to the reiteration of similar arguments with evidence of the social constructivist and relativistic distortion of the scientific “reality” associated with such phenomena as the AIDS epidemic, the animal rights movement, and Afrocentrism (multiculturalism) in science education. In a chapter entitled “Why Do the People Imagine a Vain Thing?” the authors’ critique turns away from objective analysis towards the kind of zealotry they rail against throughout their text (*afflictatio facit religiosos*). In blatant support of the “intellectual hegemony of Western science,” they (1994:220) state that: “No other civilization bears a like gem. Thus science becomes an irresistible target for those whose sense of their own heritage has become an intolerable moral burden.” Gross and Levitt conclude their book by returning to an examination of the consequences of the science wars. Here, they highlight the destructive nature of the failure to respect academic colleagues, regardless of their discipline, the (re)creation of a schism in the academy, the debasement of science education at all levels, and what they (1994:248) refer to as the “inanition” of public discourse. By way of an illustration of inanition, and as a closing comment on *Higher Superstition*, I offer this “thought experiment” by the authors.

If, taking a fanciful hypothesis, the humanities department of MIT (a bastion, by the way of left-wing rectitude) were to walk out in a huff, the scientific faculty could, at need and with enough release time, patch together a humanities curriculum, to be taught by the scientists themselves. It would have obvious gaps and rough spots, to be sure, and it might with some regularity prove inane; but on the whole it would be, we imagine, no worse than operative. What the opposite situation – a walkout by the scientists – would produce, as the humanities department tried to cope with the demand for science education, we leave to the reader’s imagination. (Gross and Levitt, 1994:243)

Vanity of vanities, *qui nimium probat, nihil probat.*

Returning to an examination of the origins of the science wars, the second event occurred, when, in a special issue of the cultural studies journal *Social Text* devoted to responses to Gross and Levitt’s book, physicist Alan Sokal published an article, “Transgressing the Boundaries” (1996b). The article, which presented a “hermeneutics of quantum gravity,” was accepted as a serious and legitimate contribution to the cultural studies of science, but Sokal had purposefully duped the editors. In an interview in which he describes how he wrote this article, Sokal says:

I structured the article around the silliest quotations about mathematics and physics from the most prominent academics, and I invented an argument praising them and linking them together. All this was very easy to carry off, because my article wasn't obliged to respect any standards of evidence or logic. (cited in Jardine and Frasca-Spada, 1997:219)

Sokal's intention was to provide support for Gross and Levitt's evaluation of the cultural studies of science, by demonstrating that cultural studies journals, lacking any rigorous criteria to judge sound scientific work, would publish any "article liberally salted with nonsense if (a) it sounded good and (b) it flattered the editors' ideological preconceptions" (Sokal, 1996a:62).

The third event took place when Sokal (1996a) admitted to his "hoax" in another article, "A Physicist Experiments with Cultural Studies," published at about the same time, in *Lingua Franca*, in which he echoes much of the same sentiment regarding "science bashing" expressed by Gross and Levitt. Further, in explaining why he turned to parody and satire for his critique, he (1996a:64) claims that those involved in the cultural studies of science "have by now become a self-perpetuating academic subculture that typically ignores (or disdains) reasoned criticism from the outside," so a powerful blow needed to be struck.

The responses to Sokal's "experiment" were swift and widespread, and the science wars were under way. Scientists and those that studied them became engaged in a war of words over the integrity and substance of each other's disciplines. Now, much effort on both sides is being expended on rhetorical and philosophical arguments designed to defend turf and ward off attack. Consequently, in some quarters, less effort is being directed at carrying out science or the social (cultural) study of what goes on in science. Rather than being drawn further into these debates, our central concern is to determine if a symbolic interactionist approach to science can help us to understand and move beyond these exercises in "taking in each other's washing" (Blumer, 1969:142).

In an attempt to put the so-called "Sokal Affair" in context, Stephen Hilgartner (1997:509) argues that not only can Sokal's experiment be viewed as a direct contribution to the social study of science, but that it fits very specifically into a set of studies aimed at determining how journals assess the credibility of submissions (see Chubin and Hackett, 1990; Hargens, 1990; LaFollette, 1992), and, even more specifically, into a subset of studies that have employed deception as

part of their method (see Mahoney, 1977; Peters and Ceci, 1982; Epstein, 1990). By comparing Sokal's efforts to those of William Epstein (1990), Hilgartner demonstrates that Sokal's piece comes off as quite unimpressive, especially when judged by his own standards, namely, evidence and logic. In other words, Sokal's efforts appear less scientific. So, for example, whereas Epstein set out to test the distinct hypothesis that confirmational bias exists among social work journals, Sokal refers more vaguely to such elements as intellectual laziness and weak scholarship, as characterizing the editorial practices of cultural studies journals. Similarly, Epstein employed a sample of 146 peer-reviewed journals compared to Sokal's sample of one journal with no peer review process. Epstein submitted two versions of an article, one with positive results and one with negative results, to randomly selected groups of journals, and then analyzed the results statistically. Sokal carried out none of these steps, but rather draws what Hilgartner (1997:511) refers to as a "voilà" conclusion (i.e., I pulled it off, therefore, I was right).

Hilgartner then goes on to explore the very different reception that the two articles received among academics and in the public forum. Epstein was severely criticized by the social work profession, to the point of almost being thrown out of his professional association, for his deception, and the press also responded negatively to his seemingly flagrant abuse of professional ethics. When the results of his study were published in *Science, Technology, & Human Values* (Epstein, 1990), they were accompanied by thirty pages of commentary on the ethical implications of his work by five separate scholars. Sokal's articles, on the other hand, not only received far greater attention in all venues, but his conclusions were by and large accepted as a highly accurate and worthy critique of cultural studies in general, and of science studies more particularly, by the mass media and in many scholarly circles. Given the obvious methodological inferiority of Sokal's efforts compared to Epstein's, Hilgartner (1997:516-517) offers a number of possible alternate factors that might help to account for the vastly different reception these articles received. For example, he argues that physics, the most scientific of sciences, enjoys a much higher status than either social work or cultural studies, and therefore Sokal, as a physicist, was *ipso facto* more credible. Similarly, he argues that the institutional resources available to the intended targets of these studies were vastly different. The editors of

the social work journals had the backing of a national association, with professional codes and ethics committees, to defend themselves against Epstein, who had clearly violated their trust. On the other hand, cultural studies are diverse and fragmented, and so *Social Text*, as a single journal, was not in much of a position to mount a defense against Sokal. As a third element, Hilgartner also argues that the timing of Sokal's experiment fit nicely into the present fascination on campuses, in popular books, and in the media with the so-called decline of the universities. What I think Hilgartner is suggesting is that members of various groups responded to these articles on the basis of the meaning that the articles had for them. While this may appear like an inane truism, it points to the fact that there is much to be learned from exploring the contexts and processes being engaged in, in these disputes, rather than limiting analysis to the content and products. As has been argued throughout the present work, neither science nor science studies are monolithic, and, therefore, it is unreasonable to expect that clear monolithic pro-science and anti-science positions can be identified among the protagonists. At the same time, I would argue that the science wars are being waged on a philosophical battlefield, well removed from the practice of science or the social study of that practice.

Reflecting on the impact of Gross and Levitt's book and his own experiment, Sokal (1997:126) states that the issue at the heart of the science wars is "the nature of truth, reason, and objectivity." Similarly, Jardine and Frasca-Spada (1997:223) argue that the science wars have provoked "a renewed and public engagement with scientists on central issues concerning the nature of scientific truth and rationality, and the unity and autonomy of science." In other words, representatives from both sides of these debates identify a philosophical battlefield where the ontology, epistemology, and methodology of science are at stake. According to Gross and Levitt (1994:86), the vast majority of scientists hold with some form of logical positivism (with a few Popperian addenda), while those on the academic left favor some version of radical (i.e., ontological, epistemological, and methodological) relativism. As the material presented in the earlier chapters of the present work indicates, not only are philosophical debates about the nature of science ongoing and largely unresolvable, at least by purely philosophical means, it is grossly inaccurate to characterize the philosophical positions of scientists, and those that study

science, as simplistically as Gross and Levitt's comments would imply. Consequently, the absence of an adequate foundation (shared constellation of meanings) upon which to carry out constructive debate over the nature of science has regrettably led to specious posturing and *ad hominem* attacks. Jardine and Frasca-Spada (1997:222) provide the following two statements to illustrate this phenomenon, the first by biologist Lewis Wolpert and the second by sociologist (anthropologist) of science Bruno Latour.

You might expect that the sociologists and philosophers would have helped to illuminate the nature of science. The great disappointment is that not only have they failed to illuminate it, but they have actually obfuscated it... Why, are the sociologists of science doing this? I can only give a sociological explanation. It's little more than envy. For me science has been remarkably successful in providing us with an understanding of the world.

A small number of theoretical physicists deprived of the fat budgets of the cold war, seek a new menace against which they heroically offer the protection of their *esprit*... France, in their eyes, has become another Colombia, a country of dealers who produce hard drugs – derridium, lacanium, to which American doctoral students have no more resistance than to crack.

The upshot of this situation, at least with respect to social studies of science, is that the time has come to abandon philosophical debates about the nature of science, and return to, or perhaps begin to undertake, the genuine and, I would argue, largely unexplored social study of science. In a recent statement, Sokal (1998) argues that:

Science Studies' epistemological conceits are a diversion from the important matters that motivated Science Studies in the first place: namely, the social, economic and political roles of science and technology. To be sure those conceits are not an accident; they have a history, which can be subjected to sociological study. But Science Studies practitioners are not obliged to persist in a misguided epistemology; they can give it up, and go on the serious task of studying science. Perhaps, from the perspective of a few years from now, today's so-called "Science Wars" will turn out to have marked such a turning point.

Apart from the obvious "physician heal thyself" remark that this statement elicits, Sokal is on the right track in one important respect. The social study of science is not about trying to solve philosophical issues about what science essentially is or should be. Rather, it is about trying to capture what science actually is, i.e., how it is carried out on a day-to-day basis. This will only happen though through the open exploration of daily practice through interaction with working scientists. What Blumer (1969:46) advocates is "naturalistic" investigation –

investigation that is directed to a given empirical world in its natural, ongoing character instead of to a simulation of such a world, or to an abstraction from it (as in the case of laboratory experimentation), or to a substitute for the world in the form of a preset image of it.

This statement holds for both physicists and sociologists. Studying subatomic particles requires a method that respects the nature of quarks and leptons. Studying the scientists that carry out this research requires a method that respects the (human) nature of the scientists. This is perhaps the key insight of symbolic interactionism. Other approaches to science start out from the position that there is a philosophically consistent set of propositions that define what science should be, or that some set of institutional norms exist that constrain the behavior of scientists, or, as some postmodernists would have it, science is all just language games. Starting out in any of these ways inevitably leads to arguments over competing ideologies, with the result that empirical investigations of what goes on in science are generally dismissed as inherently biased, or they are just not done. The symbolic interactionist approach is based on starting from the position that there is no *a priori* foundation for science, selecting to explore instead, the ways that meanings are constructed, maintained, and altered, by those engaged in doing science. The science wars would quickly fade if the protagonists abandoned philosophizing and mud slinging, and returned to engaging in the activities that constitute the realms of human endeavor through which they intersubjectively construct their identities and accomplish their goals. For those in the social study of science, this means studying science socially.

Six Positions on the Social Study of Science

Several scholars engaged in the study of science have offered a series of tenets, prescriptions or propositions that either form the basis for their studies, or reflect what they have learned through their research (see, e.g., Bloor, 1976; Knorr-Cetina, 1981; Latour, 1987; Clarke and Gerson, 1990; Collins, 1992). By way of a final statement regarding this project, I am reluctant to present a series of dogmatic statements that might be interpreted as yet another effort to restrict the way in which science can and should be studied. Rather, the following is a set of "positions" that provides a succinct way in which to describe a symbolic interactionist approach to the social study of science, as I see it.

Position One: *There is no one science.* Following Kuhn and Fleck, I would contend that there are many sciences, which can be distinguished on the basis of the paradigms (beliefs and practices) or thought styles shared by the practitioners of these various sciences, and that come be accepted by those that they interact with.

Position Two: *There is no one way to study science.* While, following Blumer and Prus, I would advocate a symbolic interactionist approach to science, the critical criterion for any proposed method is that it be naturalistic, that is, that it respect the nature of the phenomenon being studied. Thus, studying the knowledge content of a science or the historical development of a science require different methods, as does the study of the contemporary practice of science.

Position Three: *Science is demarcated from non-science on the basis of a boundary negotiated among scientists, members of other pursuits, and the public.* The first implication of this statement is that the science/non-science boundary changes regularly, as the result of interaction among the members of various groups. Second, the determination of what is science involves many people beyond the scientists themselves. These might include medical doctors, high school biology teachers, government bureaucrats, and average citizens, in their roles as consumers, taxpayers, and moral (or immoral) beings. Third, boundaries are negotiated across all levels from the lab bench to international treaty talks. Thus, in practice, distinctions among micro, meso, and macro do not obtain in any definitively restrictive sense.

Position Four: *The most promising unit of analysis for the study of science is the community.* Following Fleck, Kuhn, and symbolic interactionism, I would contend that the dynamics of the scientific enterprise are most apparent in the social interaction that takes place among the members of various groups doing science, whether we call these networks, collectives, subcultures, or communities.

Position Five: *Symbolic interactionism allows for a science of science.* A symbolic interactionist approach to science is totally reflexive. This approach can be applied to any aspect of human group life, including the study of the study of human group life, without modifications or the imposition of any special conditions.

Position Six: *Science is what scientists do.* If the material I have presented to this point has any merit, then this statement will not be viewed as either tautological or relativistic in any absolute sense. Rather, this statement reflects both the practical and intersubjective nature of the scientific enterprise as a realm of human endeavor with particular goals, and a constellation of shared and evolving meanings about the nature of those goals, ways of accomplishing them, and ways of determining when they have been achieved.

Neque enim fas est homini cunctas divinae operae machinas vel ingenio comprehendere vel explicare sermone. (Boethius)

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