1-1-1978

# Good faith social science and the activity of inquiry.

Robert Christopher Knight
*University of Massachusetts Amherst*

# GOOD FAITH SOCIAL SCIENCE AND THE ACTIVITY OF INQUIRY

A Dissertation Presented

By

ROBERT CHRISTOPHER KNIGHT

Submitted to the Graduate School of the
University of Massachusetts in partial fulfillment
of the requirements for the degree of

DOCTOR OF PHILOSOPHY

September 1978

Psychology Department

GOOD FAITH SOCIAL SCIENCE AND THE

ACTIVITY OF INQUIRY

A Dissertation

By

ROBERT CHRISTOPHER KNIGHT

Approved as to style and content by:

Howard Gadlin

Howard Gadlin, Chairperson

Ronnie Janoff-Bulman

Ronnie Janoff-Bulman, Member

Peter Park, Member

Harold Raush, Member

Bonnie Strickland

Bonnie Strickland, Chairperson
Department of Psychology

ACKNOWLEDGMENTS

I have failed nine or ten times in my attempts to write these acknowledgments. And I am willing to admit now that I cannot express my appreciation adequately within the constraints of this page. My feelings for all of those who have contributed to this dissertation and the personal growth I have experienced in writing it run too deep. I can only list some of the most important colleague/friends that have supported, pushed, pulled, and loved me through this experience.

I want to thank first my committee: Howard Gadlin, Ronnie Janoff-Bulman, Peter Park, and Harold Raush. Thanks to my co-conspirators in Method Madness (you know who you are), especially Doug Coulson and Grant Ingle. Finally, I want to thank those who were most certain of the craziness of this enterprise (quite rightfully so) and nonetheless supported me throughout: all my friends, and in particular, Dalton Jones, Dan Blyth, and my long-suffering partner, Sally Powell.

So ends the public record: short and painfully inadequate.

# ABSTRACT

Good Faith Social Science and
the Activity of Inquiry

September 1978

Robert Christopher Knight, A.B., Occidental College
M.A., San Diego State University
Ph.D., University of Massachusetts

Directed by: Associate Professor Howard Gadlin

Good Faith Social Science and the Activity of Inquiry is a theoretical discussion of the relationship of social science to social action. It is argued that the claim of empiricist social science that objective knowledge may be accrued through proper scientific methods is unfounded. Moreover, such claims serve to obscure the inherent relationships that must exist between a scientist and those who are the subject of inquiry. The analysis focuses on the role of social concepts as constituents of social reality and the ways in which social scientific commerce in these concepts must necessarily implicate scientists in the dual role of participant-inquirer. This role places the investigator in the position of describing from a particular point of view, or social perspective.

Since the social scientific aspiration of accruing objective facts is impossible, an alternative enterprise is suggested. Good faith social science is proposed as a more legitimate form of social participation. It is proposed that the pursuit of theoretical self-consciousness must be included as a crucial criterion of rigorous social science. Such self-consciousness is seen to emerge from the conflicts and discourse between those holding discrepant social commitments and incompatible social concepts. Such conflicts may not be argued in the arena of empirical validity. These conflicts involve the interests of those who are committed to different ways of life. The disputes are inherently political. It is argued then that for social scientists, rigorous empirical research, theoretical self-consciousness, political dispute, and social responsibility tend to merge within the perspective of good faith social science.

TABLE OF CONTENTS

# INTRODUCTION

The last decade may be described as a period of crisis in social psychology (Elms, 1975; Armistead, 1974). I believe this crisis can be characterized fairly as a crisis of legitimacy and purpose. In the face of criticism from a variety of philosophical perspectives (e.g., inter-subjective approaches, phenomenology, critical theory) methods of inquiry and even the status of social science as a "true science" are beginning to appear questionable. These questions are being raised not only in the distant philosophical literature but more recently in specific critiques in the familiar journals of the scientific specialties.

Of course, criticism of the experimental method and the empiricist model of inquiry did not originate in this decade. And to be sure, many researchers in social psychology and other social sciences have long operated in an essentially positivist mode while holding considerable misgivings about its limitations and even its legitimacy. Nonetheless, when faced with philosophical critiques of common empirical practices, those of us who consider ourselves active researchers are often shaken but rarely moved. While epistemological critiques may strike sympathetic chords they do not often offer clear promise for alternative investigative activities to replace the well established prescriptions of the empiricist model and the experimental method.

I, like many of my colleagues, have found myself no longer satisfied with classical methods of investigation. Hence the crisis. But enither have I found myself drawn to alternative paradigms. For the substantive researcher there are no alternative systematic paradigms to rival the experimental method for prescription of investigative activity and clear criteria of competency.

Such crises as we are experiencing are brought about, at least partially, by effective criticism and attack on the prevailing conceptions of legitimate activity and self-definition. However, such a crisis will soon foster a reactionary response unless viable alternatives begin to develop. My concern is that substantive researchers will not long suffer a paralyzing state of crisis that does not allow continued empirical investigation with a sense of purpose and integrity. And unless the epistemological criticism of the experimental method is followed by the development of more concrete alternative activities of inquiry, we will return to (or continue with) research approaches that allow robust, if not completely satisfying, pursuit of our scientific interest in substantive social questions.

The following discussion will attempt to close the gap between some critical epistemological literature and the conduct of empirical investigations. I will attempt to draw out the implications of interpretive (i.e., non-positivist) models of inquiry for the substantive researcher. I will address myself not only to our philosophical selves but also to the part of us that longs to pursue basic research, social curiosities, and pressing social issues with intelligence, integrity and good faith.

I invite the reader to join me in reconsidering the social scientific enterprise, from the broadest perspective. I will examine alternately the criticisms of current research practices in social psychology, the epistemological foundations of American social science, and the activities of conducting empirical research. I will cover a large territory, gaining perspective, I hope, both from promontory vistas and some focused close-ups. Necessarily, much will be omitted. And, because it is my host discipline, the preponderance of the discussion of social scientific method will be considered within the context of social psychology. Be that as it may, I am discussing here the broad problematics of social inquiry and explanation, and these problematics are endemic within all the social sciences and their subfield specialties. I hope that through the following very singular tour of our common concerns new insights will emerge that may in some way contribute to a transformed profession of social science.

In the first section I will attempt to refocus what I consider to be a rather confusing array of criticism in the social sciences. We have, in fact, heard an almost cacophonous racket of disgruntlement in the past few years, informed and uninformed, revisionist and radical. I will discuss the forms of these critiques as they have emerged in the social psychological literature (Chapter I), highlighting those elements which reject the empiricist tradition and therefore suggest the most challenging revision for the social scientific enterprise. Chapter II will characterize an underlying epistemological position that likewise abandons the empiricist perspective and the pursuit of social "facts." I will argue that social concepts are inherently contestable and that the use of particular social concepts must implicate the user in a set of social commitments that help consitute a way of life. This epistemological position suggests the foundation for an alternative more self-conscious and rigorous practice of social science. I will propose new aspirations and guides to the scientific way of life. I will propose the development of a Good Faith Social Science.

Chapter III redirects attention to the actual conduct of empirical research. If the criticisms of current notions in social science are to have any meaning then they must touch upon not only the more general philosophical-theoretical concerns but also the activities of conducting substantive inquiry. There is a rather substantial literature in social psychology which has been attempting to reformulate or expand the repertoire of research strategies in the "behavioral" sciences. The third chapter will review this discussion, refocusing attention on the inevitable role of the researcher as actor and decision maker in the research relationship.

In Chapter IV I will consider researcher actions and decisions as defining and constraining researcher-participant relationships. At the same time, these relationships must constrain the range of possible empirical results. I will argue that these relationships and the behavioral regularities and empirical findings that emerge from them are all implicated within social commitments to particular ways of life. Therefore, empirically derived social "facts" are limited by the socially constructed commitments within which they exist.

Finally, since social commitment and empirical relationships are seen as internally related, self-consciousness about these commitments will be viewed as a necessary element of any social science that pretends to be rigorous and explicit (Chapter V). Moreover, the pursuit of self-consciousness and what I am calling Good Faith Social Science will necessarily engage difficult issues concerning the legitimacy of social perspectives and social commitments. Rigorous social science and concerns with social responsibility will tend to merge. And insofar as this is true, social scientists in good faith will face some difficult issues in the arena of social responsibility, that is, in the arena of politics.

# CHAPTER I

## CURRENT CRITICISM OF RESEARCH IN SOCIAL PSYCHOLOGY: EMPIRICISM, REVISIONS, AND REJECTIONS

American social psychology, perhaps more than some other social sciences, may be noted for a strong orientation toward controlled experimental methods as a strategy of research. This emphasis is easily discernable over a variety of interest areas. Doctoral students' training in social psychology is typically stronger in experimental methods than it is in other alternative approaches. The most prestigious journals reflect the same method values in title and content. *The Journal of Personality and Social Psychology* and *The Journal of Experimental Social Psychology* both publish a preponderance of highly controlled experiments (Higbee and Wells, 1972). And even a cursory reading of other APA journals reveals the same emphasis (Gergen, 1975). Moreover, the preeminence of experimental approaches is not limited to researchers pursuing questions in laboratory settings. The experimental paradigm is often considered the most efficacious approach to social evaluation (Campbell and Stanley, 1963; Cook and Campbell, 1975) as well as studies of setting effects on behavior in naturally occurring ecologies (e.g., Willems, 1977; Tunnell, 1977).

A survey of recent criticisms of research methods in social psychology reveals a mixed-bag of attacks on "the experimental method," "laboratory techniques," and/or the underlying assumptions of this "empiricist tradition." Within the professional journals of the field however, critiques most frequently focus on procedures, techniques, and strategies without questioning the basic experimental paradigm or the objectives of an empiricist social science. It is much less common to read analyses of the experimental approach which call into question the tenets of its epistemological foundation.

The distinction between revisionist proposals and more thorough-going criticisms of empiricism have too often been blurred and misapprehended. The differences are profound, and any attempt to address the current malaise and crisis in social science must first identify and distinguish between expressions of revisionist empiricism and alternatives that suggest another epistemological perspective.

## Empiricism in Social Psychology

A short summary of basic elements of empiricism in American social psychology will have the ring of familiarity to almost all active researchers. However, we normally refer to these elements not as one philosophical position but rather as the "foundations of the scientific endeavor."

If it can be said that there is a dominant paradigm in social psychology, then its basic outlines certainly reside in part within the epistemic perspectives and goals of the enterprise. Underlying a variety of methods and strategies of inquiry there are a limited number of shared goals and assumptions that have allowed a certain unity to social psychology as well as the other sciences of human social interaction. To reject these shared perspectives, it is thought, is to run the risk of placing oneself outside the community of social science. A review of the most prominent canons of this empiricist tradition will serve to clarify the fundamental differences between popular revisionist proposals and the more thoroughgoing critiques.

The central concern of science is held to be the establishment of *general laws* through systematic observation. Even the most superficial examination of the social psychological literature fixes this objective as holding paramount importance, no less than in the physical sciences (DiRenzo, 1966; Krech, Crutchfield, and Ballachy, 1966; Jones and Gerard, 1969; Runkel and McGrath, 1972; Crano and Brewer, 1973; Brandt, 1972; Manis, 1975; Thorngate, 1975). While "descriptive" research may be a legitimate intermediate step in a program of study, the ultimate aim of establishing general laws of human interaction remains. In the end, social science seeks to establish stable concepts and laws that will not change their meaning across situations, times, cultures, etc. The principles of "attitude change," "social learning," "need-achievement," "internality-externality," "undermanning-overmanning," "small group process," etc., may be thought to be incomplete, or even incorrect. But these are nonetheless concepts, theories and models in pursuit of general laws of social behavior.

It follows, that if we are searching for "general laws" of human interaction then our criteria for empirically demonstrating such laws must include a moment of *prediction.* The strong version of this criterion of scientific inquiry states that complete explanation carries predictive power. Because, unlike the physical sciences, social interaction is always immersed in "open systems" of influences social theories are not expected to perfectly predict behavior. The social sciences have therefore adopted the "probabilistic" or weaker version of the criterion of prediction.

The arbiters of competing explanations (i.e., general laws, or theories) are the *empirical tests of* their *predictions* in the observable events which follow from the theory. Theories are tested; predictions of observable events are made. As a result of the process of pitting competing explanations against each other, examining their ability to predict a range of observable events, inadequate theories are rejected or revised and more adequate theories are retained. Through this manner of testing, knowledge of the general laws of human interaction is said to accrue.

My objective here has not been to comprehensively review the empiricist foundations of social psychological inquiry. Rather, I have attempted only to remind the reader of some of the most pervasive canons of this philosophical position, those that have become virtually synonymous with the conduct of social science. The point then is to note some criteria from the empiricist conception of science that have become so much a part of the social sciences that they often escape examination. Indeed, we are often in danger of believing that the "pursuit of general laws," the criterion of "prediction," and "relative theoretical efficacy" *define* the activity of true science. And to be sure, even some of the severest critics of social psychological inquiry have also assumed this empiricist perspective on science.

## Methodological Revisions in the Empiricist Tradition

There are a variety of familiar complaints and suggested revisions for the common practices of experimental social psychology. Most of these take the laboratory experiment as their target for criticism and focus primarily on technical-procedural issues. Experiments are thought to be simple-mindedly limited to linear relationships involving a minimum number of variables, while social phenomena are complex and multifaceted. The development of more sophisticated multivariate statistical techniques is recommended (McGuire, 1973). Experimenter artifacts haunt the endeavor with their potential biasing effects: demand characteristics, evaluation apprehension, etc. (Orne, 1962; Rosenberg, 1969; Gadlin and Ingle, 1975). Rigorous research, a code word for the experiment, is seen as eliciting a type of reactance from subjects resistant to the controlling atmosphere of experimental procedures (Argyris, 1968). Attempts to predict social behavior are thought to be doomed by the open textured nature of the phenomena; each new circumstance adds higher order interactions to analyses, befuddling attempts at prediction and replication (Cronbach, 1975). Others argue that social psychologists have given insufficient attention to the centrality of social meanings in shaping social behavior. Indeed, a number of these authors contend that social scientists will not develop any systematic knowledge about social behavior until phenomenological realities, meanings, and rules are more adequately included in scientific research (e.g., Harré and Secord, 1972; Bronfenbrenner, 1974, 1977).

All other criticisms aside, however, the major thrust of dissatisfaction with the laboratory experimental method has concerned its "artificiality" or as some have called it, its lack of "ecological validity." Perhaps the best known for their position on the essential importance of research "in situ" have been the ecological psychologists (e.g., Barker, 1965, 1968, 1969; Willems, 1969; Wright, 1967; Gump and Kounin, 1960). Along with others, they argue that an understanding of the behavior patterns of individuals and groups cannot arise from collecting data that is wrenched from natural contexts and "behavior settings." The context is viewed as an essential part of the phenomena of social behavior. There is a call for greater documentation of the distribution of phenomena in nature, in the "investigator-free" environment (Willems, 1969, 1977; Elms, 1975; Sells, 1974). If they are not shunned completely as impediments to the lawful explanation of human behavior manipulation and control, as the hallmarks of the laboratory experiment, are looked upon with suspicion (Bass, 1974; Brandt, 1972; Gump and Kounin, 1960; Willems, 1969, 1977).

Those authors listed above, and others (e.g., Bronfenbrenner, 1974, 1977; Feldman, Hass, and Wilbur, 1970; Fredrickson, 1972), have certainly contributed measurably to widening the perspectives of social psychologists insensitive to the role of social and physical contexts in social behavior. They may even have contributed to some of the uneasiness we feel about our empirical work. Be that as it may, it would appear that the empiricist goal of general laws, and the development of objective knowledge through prediction tests and theoretical competition remain for many unchanged. A few sample quotations from prominent critics may illustrate this point.

> . . .*behavior* is largely *controlled* by the environmental setting in which it occurs and, . . .changing the environmental setting will result in changes in behavior. (Willems, 1977, p. 51)

> (Behavioral ecology) will provide *comprehensive, progressive,* and *cumulative information* on ecobehavioral systems—homes, schools, and many other institutions and settings. (Willems, 1977, p. 64)

> Until we can assign to environmental variables the *proportions of variance in behavior for which they account*, our understanding of behavior will be incomplete. (Sells, 1969, p. 26)

> . . .investigators that have combined the three dimensions (of naturalistic research) have accrued at least three advantages: (a) New *empirical laws have been discovered,* (b) the research has been made more credible to participants, thereby *increasing internal validity*, and (c) the research has been given *greater external validity*, a valued asset in a discipline supposedly concerned with real-world events. (Tunnell, 1977, p. 426)

> In the first instance we shall be trying to devise a system of concepts for understanding social interactions and to *check this system against reality.* It may be that after generations of human ethogenists have studied the

> lives of men, women, and children, they may come to see that certain very subtle patterns of interaction do have the *force of causal laws*. But though our *methodology* cannot assume this, it *must* be such that it may ultimately *lead to knowledge of laws*. (Harre and Secord, 1972, p. 129)
>
> [in all quotations, emphasis is mine]

The preceding summary of empiricist currents in social psychology should be noncontroversial to most observers as well as participants in the field. It does not take a cracker-jack investigative reporter to discern that the overriding goal of most social scientists is to discover general laws through empirical testing. It is, in fact, no more than a reaffirmation to find that well-known critics of current empirical methods share this objective and its underlying epistemological perspectives.

## Critiques of Empiricism in Social Psychology

In recent years some scholars have taken a much deeper critical view of theory and research in social psychology. Informed by critics of empiricism in both the philosophical and sociological literature, they have cast doubt on the basic tenets underlying research activities and theory building. These forms of argument question the most basic propositions of the empiricist perspective: (1) that the rigors of systematic observation, predictive criteria, and theory testing may achieve the detached objectivity required for discovering general laws of social behavior, and (2) social phenomena are stable and transhistorical and therefore appropriately describable by general laws. The bottom line contention is that the social sciences do not and *cannot* assume a detached perspecitve on social phenomena. As social scientists, as social beings ourselves, we are intimately intertwined in the social world we pretend to dispassionately describe. The social scientist, the profession of social science, the society, and the description of abstracted social phenomena mutually constitute each other. Denial of this relationship in assuming the empiricist perspective is then seen as a major impediment to the understanding of human social actions.

For social psychologists the most familiar example of such relational critiques may be the often cited article by Kenneth Gergen (1973), "Social Psychology as History." Social behavior patterns that have been theoretically characterized as, for example, "the authoritarian personality," "conformity," "affiliation," or "social comparison" were seen as primarily historical phenomena rather than basic psychological processed. As historical periods, cultures, and subcultures change these phenomena would also be altered. In this way it was claimed that "while methods of research are scientific in character, theories of social behavior are primarily reflections of contemporary history"[1] (p. 309). Gergen takes the position that social behavior and social meaning arise out of social relationships embedded in cultural-historical contexts.

The primary concern for us here, is the relationship between the scientific community and the society. Not only are the phenomona of social scientific study seen as historically transient, but, according to Gergen, the social scientist is implicated as a moving force in this process. Not only do we study behavior, isolate concepts, and describe lawful relationships, we also make these analyses available (albeit indirectly) to those we have observed. There is a kind of feedback loop between social science and society. Concepts such as "authoritarian behavior," "dogmatism," or "reinforcement" seep into the folk culture through such outlets as *Psychology Today* or the paperback book trade. This may happen despite the best efforts at abstruse jargon and technical language. Based on common values of freedom and individuality, people are resistant to seeing themselves as predictable in the way theoretical positions depict them. And knowledge of this social psychological theory then becomes an opportunity for liberating oneself from that predictable

[1]Gergen (1973) has been criticized for basing his arguments on superficial phenomena and ignoring truly transhistorical and abstract "process models" of social behavior (Schlenker, 1974; Manis, 1975). For counter arguments to this "historical variability" but "process invariance" argument see Gergen (1976), Hendricks (1974), and Thorngate (1975).

pattern of behavior. This is especially true, in Gergen's view, since so many theories are prescriptive. We prefer to be "unauthoritarian," certainly not "conforming," or susceptable to every manipulative attempt to "change our attitudes."

> As a general surmise, sophistication as to psychological principles liberates one from their behavioral implications. Established principles of behavior become inputs into one's decision making. As Winch (1959) has pointed out, "Since understanding something involves understanding its contradiction, someone who, with understanding, performs X must be capable of envisioning the possibility of doing not X" (p. 89). Psychological principles also sensitize one to influences acting on him and draw attention to certain aspects of the environment and himself. In doing so, one's patterns of behavior may be strongly influenced. (Gergen, 1973, p. 313)

I would not like to dwell too much on Gergen's introduction of the relational problems of an empiricist pursuit of general laws. Indeed, social scientific theory may so effect society as to be the impetus for its own invalidation. And as Gergen ironically points out, the most suitable protection against such eventualities might be a more active concealment of scientific findings.

> . . .science could be removed from the public domain and scientific understanding reserved for a select elite. (p. 314)

But Gergen has only addressed a portion of the relational critique of empiricist aspirations. If social science is so immersed in society that its theory may alter current historical patterns of behavior, then we must consider that current and local history may also help constitute the questions, perspectives, and general laws of the science.

Observing that social science and society are mutually constitutive or, as here, noting the influence of cultural history on scientific concepts and "general laws" is much more than accusation of failure to be "value-neutral." Gergen's observation that many social psychological concepts contain "prescriptive bias" (e.g., authoritarian-nonauthoritarian) is one familiar version of this accusation that the social scientist often indulges his/her value commitments in "describing" concepts. But these values are surface phenomena, easily discerned and articulated, and indeed, by Gergen's argument, consciously normative to the extent that public (non-scientific) knowledge of the behavioral indices of say "authoritarianess" or "conformity" leads to widespread avoidance of these behaviors. So the argument goes, given a moral dimension within the phenomena under study social scientists cannot observe objectively but inevitably tilt to one moral alternative or the other.

To suggest that social science and society mutually constitute each other through their concepts and perspectives is to raise a much more serious criticism. Some have argued that both social science and society are formed and understood within the same systematic body of concepts and a particular manner of thinking. For example, a recent article by Edward Sampson, "Psychology and the American Ideal" (1977), reviewed the cultural and historical ideology of "self-sufficient individualism" and its penetration of such theoretical models as androgyny, mental health and moral development, equity theory, the teacher-scholar model, and encounter group research. In noting the relationship of an American ideology of individualism in forming current conceptions of androgyny (which emerges as a rather super-individual) to replace "less healthy" masculine or feminine sex roles, Sampson illustrated some of the contradictions of an empiricist perspective.

> It seems that we have adoped only one form of synthesis as the preferred mode, in part because we are blinded by our own cultural heritage and have difficulty in seeing its impact on our formulations, in part because we have few theories or methods that direct us toward alternative formulations. What I am saying, therefore, is that within an individualistic historical and cultural ethos, the self-contained individual, the androgynous type for example, is the ideal. In an alternative system, however, that same character is neither ideal nor perhaps desirable. Androgyny as a sign of good health thereby reflects an individualistic social arrangement in which persons wish to be self-contained and self-sufficient in order to be successful. (p. 774)
>
> In this connection, it is important to note that I am *not* disputing the research findings involving androgyny and self-esteem and androgyny and improved

> adaptability. I am attempting to locate these outcomes in their proper historical and cultural context and view them thereby as syntheses that are uniquely suited to our contemporary individualistic and self-contained ideal.
>
> Likewise, by placing the concept of androgyny within its historical and cultural setting, we can see that androgyny is neither a necessary, an inevitable, nor a fundamentally more desirable psychological quality. (Sampson, 1977, p. 774)

These comments illustrate several disturbing themes for the scientist pursuing general laws through empirical theory testing. Social psychology, and social scientists, are so immersed in cultural ideologies (such as self-sufficient individualism as an ideal type) that the conceivable "descriptive" concepts even alternative and competing concepts (e.g., masculine-feminine sex roles in America vs. androgyny), tend to be tightly bounded and restricted. Moreover, theoretical tests of say "the adaptability of androgynous sex roles" are empirically self-confirming within a cultural ethos and social structure that support the individualist ideal. I might add, if it is not already obvious, that a belief that general laws are confirmed through these "tests" of empirical prediction simply adds to the invisibility of the underlying cultural perspectives and social structures that predispose the results.

Other social scientists have also criticized the field for inattention to the relationship of scientific concepts with the larger society, culture and subcultural ideologies and even the local interactions within the research activity itself (e.g., Buss, 1975; Bronfenbrenner, 1977). Sigmund Koch, for example, reconsidering what he had learned in his classic work *Psychology: A Study of a Science* (1959, 1962, 1962) has commented,

> . . .it should be emphasized that "paradigms," theories, models (or whatever one's label for conceptual ordering devices) can never prove *pre-emptive* or preclusive of alternative organizations. That is so for any field of inquiry, but conspicuously so in relation to the psychological and social studies. The presumption on the part of their promulgators that the gappy, sensibility-dependent, and often arbitrary paradigms of psychology *do* encapsulate pre-emptive truths is no mere cognitive blunder. Nor can it be written off as an innocuous excess of enthusiasm. It raises a grave moral issue reflective of a widespread moral bankruptcy within psychology. In the psychological studies, the attribution to any paradigm of a pre-emptive finality has the force of telling human beings precisely what they are, of fixing their essence, defining their ultimate worth, potential, meaning; cauterizing away that quality of ambiguity, mystery, search, that makes progress through a biography an adventure. (Koch, 1977, p. 5-6)

The mutually constitutive relationship of social science with the social phenomena of its interest has been analyzed not only at the broad level of science and society, concepts and ideology, but also within the local interactions of the researcher and those being researcher. In two related papers Gadlin and Ingle (1975) and Ingle (1977) have reconsidered the experimental paradigm in social psychology in terms of its underlying "objectivist" (read empiricist) perspective and consequent failure to consider the relational character of the experimenter-subject encounter. Interestingly, they find that social psychologist have rarely utilized social psychological findings to gain understanding of the social psychology experiment. An examination of recent literature revealed some instructive and disturbing findings that cast doubt on the possibility of constructing a social science of general laws independent of the relationship of the scientist/researcher with his/her "subjects." Assuming the role of the dispassionate empiricist, social experimenters have become unaware of their own role in the empirical findings which they generate. Looking at the social behavior of others, social psychologsits have found that: (1) the presence of a potentially evaluative audience systematically alters persons' task performance, (2) when attributing the causes of events actors tend to see these as more often residing in the social or physical environment while observers attribute causes to internal actor dispositions, and (3) in assymetrical power relationships (physical, expert, or social power) the participants act out their roles through social power self-presentation and ingratiation, respectively. It takes very little reflection to recognize the relevance of these social relationship phenomena for the encounter of the social scientist with his/her experimental "subjects." While their arguments focused largely on the particular relationship of the social psychological experimenter to his/her subjects, it is clear that we cannot escape this relational critique merely by leaving the laboratory setting or pursuing unobstrusiveness in our observations.

> . . .any attempt to describe *the* ideal research relationship would be. . .short-sighted—all research relationships impose their own unique set of limitations upon the knowledge which they eventually produce. Thus, the question of an appropriate research relationship is asked prematurely if questions of content and the uses of that content are not adequately addressed first. (Ingle, 1977, p. 87)

> We are suggesting that we abandon what we consider a futile attempt to control, inhibit, or deny the relational aspect of research. Rather, we suggest that the relational quality of research be attended to; that it be developed and investigated. (Gadlin and Ingle, 1975, p. 1008)

The authors suggest that advances in social psychological research will depend in part on our,

> . . .acknowledging that the study of human behavior necessarily includes the behavior of psychologists. This recognition implies, of course, that the psychologist is as prone to psychological processes as anyone else, and should be especially self-conscious of this fact when acting as a scientist. This self-consciousness includes the psychologist's awareness of his relation to and with his subject matter, and the awareness of his own role with respect to inquiry. The knowledge that derives from such

> reflexivity is a tripartite knowledge—about the subject, about the researcher, and about knowledge itself. (Gadlin and Ingle, 1975, p. 1008)

## Current Criticism and the Conduct of Social Science

Criticisms such as those discussed above, criticisms which question basic aspirations and procedures, have made very few inroads into the actual conduct of inquiry in social science. Of course, many if not most, social scientists are familiar with the criticisms sporadically appearing in the literature. And many of us are at least partially aware of the robust literature in the philosophical disciplines that is questioning empiricist perspectives and developing alternative views of science. If these positions are found to be interesting, then they are too often interesting as mere intellectual curiosities. If the criticism is disturbing, this disturbance is an undercurrent that wells up small whirlpools of doubt but does not significantly change the current of the empiricist stream. The crisis in social psychology and other social sciences is played out in occasional critical articles, small and isolated pockets of deviant scientists, and in the silent and personal doubts of mainstream researchers. Perhaps this is, if you will, normal for a crisis.

Following the analysis of Thomas Kuhn (*The Structure of Scientific Revolutions*, 1962), I would agree that old paradigms are replaced by more promising paradigms rather abandoned in the face of criticism. And I believe such an analysis takes us some distance in understanding the tenacity of the empiricist perspective in theface of recent criticism. The great majority of social scientists are very practical-minded men and women intent on pursuing substantive questions. The empiricist tradition has allowed them a robust perspective and procedure within which to indulge their desires to answer these nagging questions about social behavior. Philosophical criticism or general critiques within the social sciences may raise doubts. But the empiricist perspective will not be abandoned without a clearer promise for an alternative paradigm.

In the succeeding chapters, I would like to offer an analysis of scientific activities that may move us beyond the more general or philosophical criticism toward an alternative empirical social science without empiricism. I will argue that the presumptions and tenets of philosophical empiricism are demonstrably untenable. That is not to say that systematic observation has no place in social science. I do not wish to dispute the centrality of empirical research for a legitimate social science. I do dispute the empiricist conception of empirical activity and empirical aspirations. Empiricism places systematic observation within a value-free enterprise, objectively establishing "basic knowledge," and "facts."

I will argue that this empiricist enterprise grossly misapprehends the relationship between the empirical research of social scientists and the social phenomena under investigation. In making this argument I will attempt to address two broad issues: (1) translating epistemological critiques of empiricism into the familiar problematics we face in considering strategies of inquiry and, (2) analyzing the deep implications of a non-empiricist perspective, a new way of life for the social scientist. I will begin this analysis by first stepping back to reconsider some discussions from the epistemological literature. It is true that the criticisms that have appeared in the social sciences are informed by or have derived from the more basic philsophical discourse. But they are not of the same form or logic. I believe the translations from this literature by social scientists have not always captured the full importance and pertinence of the philosophical discussions. Therefore, I will briefly characterize my reading of epistemological literature that has focused criticism on empiricist suppositions.

I will argue that social scientists *cannot* detach themselves from the social concepts that are the subject of inquiry. I will argue that social concepts are not just phenomena (located out there in the world); they are social commitments that can and do change. They are social commitments that help constitute a way of life. From this discussion I will suggest an alternative epistemological base for the conduct of inquiry. This then will set the stage for considering the implications of such an alternative for the way of life we have come to call social science.

# CHAPTER II

## GOOD FAITH SOCIAL SCIENCE

> The world *is* for us what is presented through our concepts. That is not to say that our concepts may not change; but when they do, that means that our concept of the world has changed too. (Winch, 1958, p. 15)

Making sense of the world and the use of social concepts in doing so is, of course, not a uniquely scientific activity. It is at the heart of human social existence. Who are kinsmen? What is a mistake? What kinds of activities speak of intelligence or insanity? What is a cause? "The world *is* for us what is presented through our concepts." For social science no less than for other realms of social life the process of forming concepts lies at the very heart of all our undertakings. And if we are to understand the dimensions of our social existence we must come to grips with this process of conceptualization which helps constitute our reality.

I would like to argue that social scientists are *necessarily* active agents in the social construction of reality. Moreover, this participant relationship is precisely what the empiricist tradition denies and therefore obscures.

A Good Faith Social Science must place self-consciousness and explicitness about scientists' social participation at the center of the enterprise.

Social science is, of course, in a special position with respect to social concepts. Everyone participates in the construction of social reality. However, as social scientists we are not only social participants, but at the same time we are engaged in the profession of social explanation. Social explanation is the raison d'entre of the social scientific profession. We have taken social concepts as our coin. We are, in a sense, merchants in conceptual reality. But concepts then become *both* the currency of our world and the products of our enterprise. We are at the same time participants in the making and brokers in the exchange. And our most serious challenges are to avoid inadvertently brokering in counterfeit concepts. If we are to trade in social concepts and explanations, if we presume to be social scientists, then we must come to grips with this inherent complicity.

Attention has too long been diverted from the basic social scientific problematic of concept formation. The statistical and experimental technology for manipulating observed and conceptualized phenomena has been developing at a staggering rate. Scientists have become dazzled and enchanted by their own mechanical handywork. In this state of entrancement, scientists have lost touch with their attachments and intimacy with the theoretical concepts that have been reduced to mere objects and manipulanda.

Before we can consider the methods and strategies of inquiry we must reconsider our relationship to the substance of our investigations. We must reexamine the scientific problematic of description and characterization. We must rediscover our relationships within the social experience of conceptualizing and ordering the phenomenal world. This chapter will attempt to re-place social scientific activities within the social fabric. I will further argue that with the realization of social attachments must come a reorientation to the tasks of social science. The pretensions of empiricist detachment are untenable. We must begin to develop a social science that not only recognizes its embededness in social relationships but also its obligations to participate in good faith.

## Describing From a Normative Point of View

Borrowing an analogy (Connolly, 1974), it may be said that the connection between the substance of empirically verified "facts" and the terms and concepts of theoretical formulation are comparable in some respects to the relationship of a jury to a legal system.[2]

---

[2] The discussions of "describing from a point of view," "describing from a moral point of view," and

> The jury examines the evidence and reaches a verdict, but prior to its deliberations, the judge, acting as official interpreter of the law, charges the jury with a set of responsibilities, establishes what can be considered as evidence, and specifies what constitutes a punishable offense. If, for instance, the jury is to decide whether the defendant negligently caused or contributed to injuries received by the plaintiff, the judge instructs the jury as to what sort of conduct legally constitute "negligence" and "contributory negligence", he also informs the jury where the burden of proof rests and screens claims from it, such as "hearsay" testimony, that are not legally admissible as evidence. In charging the jury and in regulating the presentation of evidence to it, the judge, we might say, specifies the terms within which the jury considers evidence and reaches a verdict. (p. 2)

It is my position that empirical conclusions are necessarily defined and limited by the dimensions of their constituent theoretical terms and concepts. Our concepts dictate the domain and structure of the "reality" to be observed and measured. Alternative conceptions may constitute the phenomenal world into incommensurate realities. And if this is true, then the empiricist contention that competing theoretical conceptions may be empirically compared and tested for their "truth" potential is without foundation. It is a contention that requires a singular reality and conceptual domain

---

"essentially contested concepts" are indebted to William E. Connolly's analyses of these issues in *The Terms of Political Discourse* (1974) and some personal communications (1977). My presentations of these issues are essentially the same as Connolly's.

as foundation for its empirical arbitration.

> To describe a situation is not to name something, but to characterize it. Thus, we are not describing when we say "Empire State Building" or "Jim", we are when we say that the building is very tall and made of grey concrete or that Jim is a quiet, intense person who is quite industrious. It is the tendency to think of describing as if it were the same thing as naming that accounts for so many commentators ignoring a fundamental feature of description: A description does not refer to data or elements that are bound together merely on the basis of similarities adhering in them, but to describe is to characterize a situation from the vantage point of certain interests, purposes, or standards. Connolly, 1974, pp. 22-23)

The proposition that to describe is to characterize from one or more points of view would seem an innocent enough observation. Consider some examples. The definition of a chair is indeed contained in its purpose. A chair may take on a variety of shapes, textures, and styles; but an object is a chair from the point of view of its "sitableness." Indeed, a broad assortment of objects may be used for this purpose. And any number of objects may become chairs or benches, but only when they are seen as "sitable" objects. Unruly weeds become herbs only when we discover their properties of taste, or medicines when they are believed to have curative uses. When terms such as "dangerous," "fearful," or "menacing" are used to describe persons or settings they are used to call attention to features which may be harmful to participants. These terms do not "name" a reality, they *orient* the listener to certain characteristics; and these characterizations serve the purpose of issuing a warning.

Beyond describing from a point of view, the terms and concepts of human discourse are often *descriptions from a moral or normative point of view.* And this is especially true as we go beyond physical objects to consider personal and social characterizations. Genocide, racism, aggression, altruism, intelligence, and personal control are all terms which describe from an evaluative and normative point of view, or they are in the words of Julius Kovesi (1967) "moral notions." Such concepts are moral notions in that while certain specified conditions must be met before the concepts may be applied, these concepts are also to a large extent constituted by their appraisive or evaluative meaning.

A term such as aggression may be applied to a very wide range of human activities. There are, to be sure, some socially established limiting conditions for its application. It is a description from a moral point of view not in the sense that to say someone was aggressive is always to say they were wrong, but rather in the sense that the concept would not be formed unless there was some point in doing so--unless we shared a moral point of view that this concept concretizes and reflects. Any act that may be described as aggressive must also appear liable to moral injunctions. In the same way that candidates for the term chair must have certain characteristics which allow sitting (a reasonably flat horizontal surface, for instance), actions which may be characterized as aggressive must have some attributes which could lead us to condemn the agent. In this way the criteria and the moral point of a concept are dialectically related; it is from the point of view of the kind of conduct which we consider unacceptable that the concept "aggression" is formed. And it is only if the acts meet these criteria that we have reason to accuse the agent of being aggressive.[3]

Moral or normative notions, concepts which describe from a moral point of view, may vary in their level of completeness (Kovesi, 1967; Connolly, 1974). Murder, for example, is a relatively complete moral notion. Intentionally taking the life of another for personal advantage

---

[3] Some might suggest that behavioral operationalization could be used to avoid the normative elements of common language concepts such as "aggression". But it is clear that this strategy cannot succeed in avoiding the normative dimensions of a concept without at the same time fundamentally changing its meaning. The elements of any operational definition must either reflect the moral point of the concept, or on the other hand, adopt definitional elements that alter its existing normative point, or seek a level of abstraction that dismembers the concept by wrenching all moral perspective from it. That is, the would be "neutral observer" must adopt the prevailing normative contours and behavioral criteria of a concept or unilaterally and arbitrarily change the perspective. In the latter case the scientist is of course dealing with a concept without social connection or social meaning.

constitutes adequate reason for condemnation. There is indeed little room for the "good murder." Of course, other concepts share this characteristic of relative moral completeness. The absurdity of violating the moral point of such concepts may serve as the best illustration. What could one possibly mean by the terms "ethical genocide" or "justifiable racism." But other concepts that describe from a moral point of view are relatively incomplete. While there is a moral point which helps constitute the term, there is room for extenuating circumstances or special conditions. Actions which are taken with the intent of harming or thwarting another person are aggressive; such actions are inherently suspect or at the very least liable to condemnation. However, aggression is an incomplete moral notion because there are special situations in which it is acceptable or circumstances which allow dispensation from its moral sanctions (e.g., aggressive football player, aggressive businessman, aggression in self-defense). Nonetheless, such a concept does have a moral point, and we cannot violate this point without special justification or the term is in danger of losing its sense altogether.

Many other concepts, familiar to the social sciences, may be characterized as incomplete moral notions. Such concepts as self-sufficiency, personal control, individuation, assertiveness, responsibility, and altruism range in their completeness but are all, nonetheless, concepts which describe from a normative or moral point of view. That the moral point of these terms does not constrain their uses as forcefully as concepts such as good, genocide, or murder can be conceded. We may not say, "yes, it was genocide, but was it justifiable genocide," without violating the moral point of "genocide" as a concept. We are, in making such a statement, liable to being accused of totally misconstruing the term. With an incomplete moral notion such as self-sufficiency we are also constrained by the point of the term. We may say, "isn't it a shame how he has become so self-sufficient," but if we do then the *burden of proof is upon us* to defend this use of the term. We have violated its moral point. An argument could be made for the negative implications of self-sufficiency in this *particular* case, "but for those sharing the concept, it embodies a standard to be applied unless so defeated" (Connolly, 1974, p. 31).

If in particular instances we may violate the point of view embodied in a concept, we may not do so as a matter of course. To change the moral point of a concept is to fundamentally change its meaning, and to subtract the moral point of view completely is to render the concept empty and without sense. Such a concept will fall into disuse. This has been the fate of such terms as "spinster," "saving yourself for marriage," "patriarch," "man of the house," "negro," and others. Our society has changed in some ways, and the moral points of view which once helped constitute these terms have either changed or were lost; and with this these concepts have radically altered their meaning or have disappeared from common usage altogether.

## Essentially Contestable Concepts

All concepts are descriptions from a point of view and often from a moral or normative point of view. Moreover, within a complex and changing social fabric the criteria for applying concepts to concrete events may be unclear and disputable. Concepts such as "intimacy," "achievement," "competence," "personal control" and "responsibility" must undergo almost constant adjustments in their criteria for application as new and unforeseen situations arise. And as new criteria arise and established criteria change so do the dimensions of meaning that help constitute the concept. Intimacy does not mean today what it did 100 years ago (Gadlin, 1977), although it still maintains its moral point. New situations and circumstances have interceded which required social adjustments in our way of life and our concept of what it is to have a close relationship.

Although many concepts change and some even disappear, not all concepts are contestable. Following the analyses of W.B. Gallie (1956) and William Connolly (1974, p. 10), certain conditions must be met before a concept is contestable.[4]

1. The concept must have a moral point. It must describe a valued or devalued achievement.
2. The concept or practice involved must be internally complex. That is, its characterization must involve reference to several dimensions.
3. The criteria for applying the concept must be relatively open enabling the parties to integrate even shared rules differently as new and unforeseen situations arise.

The related concepts of womanhood, "feminine," and "femininity" are both contestable and contested in contemporary America. Womanhood is a valued achievement by all the contesting parties. It is an internally complex concept with constituent dimensions and criteria of application which are themselves disputable (e.g.,

---

[4]These two authors discussed "essentially contested concepts" within the context of political science. I have changed the perspective to one of "potential contestability" in order to adapt this idea to a broader arena of social science.

self-respect, achievement, equality, justice, motherhood, demeanor, etc.). The debate over the Equal Rights Amendment, for example, may be viewed as a contest over the relationship of equality to justice and full womanhood. Those opposing ERA have argued that legal equality will undermine "justice" for women as they conceive them. Conflict over the rights of women to equal pay for equal work is a contest about the concept of an "achieving woman" and whether the work world will be considered an appropriate arena, requiring equal protection under the law. The conceptual landscape is changing, and the emerging "new womanhood" represents an entirely new social phenomenon, reshaping relationships and creating its own dynamic of change with every other element of social life.

Other concepts have been contested and expanded in recent years: intelligence, racism, and institutional racism, for example. But perhaps of more interest here are contestable concepts that have not been raised to the level of public dispute. These are concepts of moral-normative importance. They are internally complex. And alternative formulations of these notions are not only possible but evident in isolated subcultures, deviant subpopulations, and some scholarly analyses. This issue becomes critically important as we realize that it is the relative stability of some social concepts which has made them appear "natural," immutable, and transhistorical.

It is such a misconstrual of social phenomena which allows researchers and theoreticions to apply the tools of empirical natural sciences. Understanding social concepts as fixed, allows us to treat them as stable objects. And, in the end, this empiricist objectification reinforces the conceptual status quo for those phenomena.

Social concepts with moral or evaluative relevance are social concepts with real human consequences. They help constitute a way of life. They are essentially contestable. Such moral concepts may only avoid social dispute to the extent that they remain invisible or immutable, and are viewed as part of the "natural order" rather than the social order. Their evaluative nature creates the moving force for competing self-interests to dispute the criteria of application or even the moral perspective itself. But such social disputation may only occur when the different perspectives and interests can come to a point of mutually recognized contradiction.

Families, intimate friends and lovers, for example, inevitably contain within them myths, customs and forms of interaction which have evaluative or moral significance. The acceptable means of exhibiting affection, anger, sexuality, etc., may be established within these small and bounded relationships. If they are indeed segregated from wider societal intrusions they may contain dimensions that are quite idiosyncratic. In a society that does not engage in open discussion of intimacy and sexuality, for example, contradictory myths and conceptions may be generated within this privacy and isolation. There is little opportunity for these private moral concepts to come into recognized contradiction with other perspectives. They may remain uncontested concepts. It is only as these domains receive public light that social and personal contradictions may be addressed and moral concepts disputed.

Those social concepts which most thoroughly permeate a society are perhaps the most difficult to discern. Their very ubiquitousness within the domains of social intercourse, individual aspiration and institutional structures creates their appearance as part of the natural order. Sampson's analysis (1977) of the interpenetration of "self-sufficient individualism" and social scientific theory is, of course, illustrative of just such a concept in the American social order. Self-sufficiency, personal control, and individualism are constitutive of our American ideal and are therefore the foundation for the ordering of other concepts which define the ideals of our way of life. We dispute the dimensions of mental health but not the centrality of self-sufficiency at the core of the considered alternatives. Sex role complementarity pales next to the competency and independence of an androgynous alternative. In environmental psychology notions of personal control create the theoretical order for evaluating the adequacy of built environments for human use (Knight, Zimring, and Kent, 1976; Altman, 1975; Baron and Mandel, 1975). Quite "naturally" concepts of "privacy" dominate the theoretical and empirical literature. These concepts have intuitive appeal; that is the measure of their congruence with prevailing social conceptions. And, of course, to the extent that we discipline these theories to closely reflect underlying social ideals and manage our research to select "appropriate tests," "empirical verification" will be forthcoming.

Of course, no social order is simple and monothematic. Even concepts as ubiquitous as self-sufficient individualism are not universally endorsed. A moment's consideration will bring to mind a variety of social arrangements inconsistent with this ideal (e.g., communal and utopian communities, cooperative houses and apartments, etc.). But such arrangements are socially deviant, in some cases illegal (e.g., zoning laws often prohibit cooperative housing), and always faced with difficulties both from within and without. The society is so structured as to enforce the atomization of individuals and cooperative groups are always challenged by the members' own difficulty with relinquishing individualistically defined aspirations, needs, and desires. Cooperatives in America are notoriously short lived.

Contestable concepts, especially those that constitute deep and pervasive commitments within a social order, may overpower the abilities of those who would dispute them. The communal and cooperative movement has had many false starts in the face of an American individualistic and competitive social structure and ethos. The aspirations of women did not change suddenly to form the modern feminist movement. History is filled with instances of women organizing for change as in the British and American suffragist movements. But when isolated women have attempted to push the boundaries of the prevailing conception of womanhood they often suffered personal doubts, frustrations and failure. Whatever the accomplishments of a person such as George Sand, for example, the image of women in 19th century France did not undergo the fundamental transformations that would allow for the routine acceptance of literary genius such as hers. When social concepts are widely held and especially when they appear to define "the natural order," these contestable concepts may not surface as true social dispute. When the social structure labels and isolates deviants, the prerogative of naming the reality is with the powerful. Prevailing concepts of the poor and unemployed (cf. Ryan, 1972), marijuana users in the 1950's (cf. Becker, 1963), or any other relatively powerless and isolated group, may be contestable. But their position in society will preclude anything that could meaningfully be considered social dispute. Under such conditions the concepts of the "lazy and shiftless poor" and the "perverted hop-head" may persist as realities; *deviant* opinions will remain just that and no more.

The concepts we use in contruing and constituting the world are at the center of our social existence. Moreover, normative and moral notions embody the force of value, aspiration, sanction, and condemnation. Their dimensions and criteria of application help define the shape of the human spirit and the constraints of a social way of life. Because we exist in a complex social world these concepts and criteria that define our worth and worthyness may become contested. To the extent that evaluative concepts can be viewed as the "natural order," so long as we may understand such concepts as competency, achievement, intelligence, womanhood, control, or individuality as immutable, then the social world may remain static. When victims of the prevailing moral concepts can being alternative perspectives into the social arena and present their contradictions, only then may the forces of contest and change be engaged.

The Problematics of a Social Science

The empiricist tradition in social science has failed to comprehend the relationship of our social concepts and our social way of life. In light of the preceding discussion, the idea of an empiricist social science loses its sense. If we understand the essential contestability of social concepts, then the scientific belief in the possibility of "objective description," "general laws," "prediction," and "empirical tests of theory" take on new meaning. They no longer represent the canons of discovery. Rather, they become the rules for conservative social action. Such a world view operates as a force to fix conceptual reality at the status quo.

> The rules for the proper use of our moral notions, however, are at the same time rules for what those notions are about; they are rules for our behavior. (Kovesi, *Moral Notions*, p. 53)

> . . . the objectivated world loses its comprehensibility as a human enterprise and becomes fixated as a non-human, non-humanizable, inert facticity. Typically the real relationship between man and his world is reversed in consciousness. Man the producer of a world, is apprehended as its product, and the human activity as an epiphenomenon of non-human processes. Human meanings are no longer understood as world producing but as being, in their turn, products of the "nature of things." It must be emphasized that reification is a modality of consciousness, more precisely, a modality of man's objectification of the human world. *Even while apprehending*

> *the world in reified terms, man continues to produce it. That is, man is capable paradoxically of producing a reality that denies himself.* (Berger and Luckmann, 1966, p. 89). [Emphasis added]

Empiricist aspirations are a hoax that have obscured the essential embededness of the social scientist within the phenomena of inquiry. And to understand that we are necessarily both inquirers and participants in social reality is to fundamentally change the problematics of the social scientific enterprise. If we may not take the "objective" point of view then from what perspective are we viewing phenomena? If we do not pursue "general laws" then what are our goals? What replaces the "empirical test" and the criteria of "prediction" as the arbitrer of competing concepts and theories?

Let us reconsider then some of the problematics of a social science. While the discussion thus far obviously precludes the possibility of an objective and detached science, we may not simply endorse the converse of objectivity. To say that the concepts we use in describing social reality *help constitute* that reality is not to claim that reality is *no more* than its subjective or intersubjective representations. We may not say that social reality is no more than the concepts we use in describing it.

> Human beings can only act toward the world on the basis of some "understanding," but it does not follow from this that their activity, or the world, possesses

> the character which they "understand it to have." (Lichtman, 1970, p. 92)
>
> Human activities have a relationship to each other which is an objective constituent of the world. These relationships may come to be known, but they are obviously not identical to any knowing of them. (p. 77)

Some philosophers and social thinkers have come very close to the completely volunteerist alternative to empiricism (e.g., Winch, 1958; Blumer, 1969; Berger and Luckmann, 1966). But if empiricism alienates us from our generative contributions to the social order, then volunteerism underestimates significant elements of the human condition that constrain social participation and self-consciousness.

The concepts of "self-deception," "false-consciousness," "repression," and "unconscious" have persisted in the lexicon for understanding human actions because they continue to offer explanatory power generally and, I believe, because almost all of us at one time or another have identified these processes within our own biographies. Moreover, if we accept the position that social existence and subjective conceptions of it perfectly mirror each other, then complete understanding of society should be achievable through close study of its isolated individuals. The impossibility if not absurdity of such a contention is self-evident. Within the volunteerist position there is no room for a distinction between "appearance" and "reality." There is no possibility for deception and detection, or social illusion and social structure (cf. Connolly, 1977). If such distinctions were not central to our social existence one might wonder at the persistence of our "queries," "discoveries," "reforms," and "mistakes" about social life. As I have argued previously, supporting claims to the "correct" or "valid" characterization of reality is problematic, but at the same time we may not abandon this concept completely without leaping into an abyss of solypsism or radical relativism (Lichtman, 1970; Connolly, 1977).

The new problematic for social science then lies in our participation in constituting a social reality that has real consequences which themselves are, in their turn, conceived within our commitments to a way of life. We may not objectify social reality; neither may we deny that, in part, it stands outside our subjective understandings of it. If this understanding of the problematic is accepted then it is clear that social science must establish new aspirations that accept the impossibility of accruing "facts" through detached observation.

## Good Faith Social Science

I am proposing that the idea of an "objective and valid" social science be expanded by the broader imperative of a science in good faith. The aspiration of objectivity through valid methods would be replaced by the pursuit of disciplined self-consciousness. I am using the term "good faith" not in the Sartrian sense of exorcising all self-deception ("mauvaise foi," *Being and Nothingness*). Rather, by good faith I mean to focus on the problematics of placing self-consciousness at the center of the participant-inquirer dilemma. *Good faith social science* then *describes a form of relationship within a society and a way of life*. It is the pursuit of self-consciousness concerning the perspectives of our concepts, theories and empirical activities. But self-consciousness is not something to be achieved and reported by individuals. It involves a dialectical relationship that may be established among good faith participants attempting to illuminate perspectives and their consequences, and from this form and re-form their way of life.

The central issues for a good faith social science will reside in our relationships with those we are studying and the society of which we are members. If the reader has followed the discussion this far, it should be clear that I am arguing that within the theoretical-empirical enterprise is the negotiation of reality. And as in any negotiation the central themes are not only truth and falsehood but fairness and deception. While there are technical and procedural issues involved, at the heart of the relationship are the human concerns with morality and social responsibility. And the challenge for the social sciences is to reconsider the activities and practices of our discipline within this imperative. We are faced with very basic questions and dilemmas regarding the social scientific way of life. The contours of conceptual meaning and behavior that help constitute our relationships constrain and form the interpretations of disciplined inquiry. And the worthiness or validity of any conceptual point of view cannot be established or refuted through simple empirical tests. The issues are moral and normative; they reflect commitments to a way of life and a position in the social order. And because our concepts are also commitments they elude simple individual self-reflection and introspection.

The legitimate aspirations for a good faith social science must include the elucidation of socially and historically generated conceptions and the disciplined investigation of their constitutive human actions. Conceptual realities, social actions and scientific inquiry are all internally related. Nonetheless, I believe that the development of a self-reflective and critical consciousness within our discipline can begin to establish a reasoned dynamic to these relations. Our specific tasks in this regard may not be defined and set into mechanical motion. Rather, the critical question may be, where do we begin? I believe that within the social sciences we can effectively begin this process by a reconsideration of our most cherished rituals of empiricism. The method and mechanics of inquiry have represented a sort of sacrosanct and holy cookbook . . . results guaranteed. In succeeding chapters I will suggest that our methods may be re-viewed. Methods are not only directions for disciplined inquiry; they are also the vehicles of our social perspectives and commitments. They may therefore be the most useful vehicles for self-reflection. And if we replace the dimensions of method within this dialectical perspective, perhaps the dynamics of a critically self-conscious dialogue may be initiated.

# CHAPTER III

## SOCIAL RESEARCH: METHODS AS ACTIONS

Research methods and the "rules of competent inquiry" help constitute the heart of the empiricist enterprise. Research methods are, of course, the actions and decisions of investigators, designed to allow more systematic consideration of social phenomena. However, these canons of scientific investigation, "reconstructed logic" in the words of Abraham Kaplan (1964), are often understood in formal/structural terms that tend to obscure the role of the researcher as actor and decision maker. It is my contention that a simple refocusing, highlighting and explicating these dimensions of human action will allow important insights into the nature of social scientific enterprises and the social commitments within which they operate.

"Statistics," "research methods," and "experimental design" have been viewed as the first and initiating courses for any serious student of the social sciences. Methodological concerns are, in the empiricist view, the procedures for valid discovery, hence their central role in science. As has been aptly pointed out by others, our methods have preceded our substance (Koch, 1959). Research methods have been seen as "tools" for discovery and these tools have become prior to and detached from the tool users. *Research methods and designs are applied* to "a phenomenon" and a "subject population." The correct application of method is thought to insure the discovery or demonstration of scientifically valid laws.

This conception of research methods as tools that exist detached from the researcher or the researched is reflected in the language with which methods are described. It seems that research design and techniques are seen as independent entities.

> *Designs* are carefully worked out to *yield* dependable and valid answers to the research questions epitomized by the hypothesis.
>
> *How does design accomplish this? Research design sets up* the framework for adequate tests of the relations among variables. *Design tells* us, in a sense, what observations to make, how to make them, and how to analyze the quantitative representations of the observations.
>
> Finally, an *adequate design outlines* possible conclusions to be drawn from statistical analysis.
> (Kerlinger, 1966, p. 276)
> [Emphasis added]

This general discussion above is followed by the description of "poor" and "good" research designs. Moreover, this abstracted conception of research design is not unique to Kerlinger but is echoed in most major research methods texts (e.g., Festinger and Katz, 1953;

Selltiz et al., 1951; Crano and Brewer, 1973; Runkel and McGrath, 1972; and others).

To say that research methods are detached from their human users and contexts is not to deny that there are a great many controversies about proper methods of inquiry. But of course, these controversies are themselves understood as disputes over the *rules for scientific discovery.* Debates revolve around abstract and utilitarian characterizations of the research enterprise: where should research be conducted (laboratory vs field and naturalistic observation)?, what is the best form of resultant data (quantitative vs qualitative designs, multivariate vs univariate designs, orthogonal and non-orthogonal relationships)?, what are the most powerful mechanics of research analysis (experimental, correlational, time series analysis)? The discourse has remained technical and object oriented. We've got a hammer, where do we pound?; we've got a hammer how can we make it a better pounding tool?

The current conceptions of social scientific methods tend to obscure the fact that research strategies are constituted by researcher decisions and actions within a social context. A social research project is not an abstract exercise in scientific investigaton; it is a substantive and meaningful relationship between an investigator and the participants in the study. The obscuring of this social relationship between the researcher, the participants, and the activities of inquiry stands as both a fundamental element of empiricism and a major impediment to self-consciousness about our roles in the negotiation of social reality. If social scientists are to pursue a self-consciousness about the scientific enterprise, the methods of inquiry must be reconsidered as researcher actions on and with particular phenomena and particular people.

In this chapter, I will re-characterize *research methods as the decisions and actions of researchers.* I will focus attention on what social scientists do to and with those being studied. My intention is not to dazzle the reader with startling insight so much as to review familiar concepts from a slightly different point of view. Many of the characterizations that follow will appear obvious upon the naming. Nonetheless, this analysis will support further consideration of the scientist's role in empirical inquiry. In subsequent chapters researcher actions and decisions will be reconsidered as the elements from which researcher-participant relationships are built. This perspective will later provide a viewpoint from which to address issues that are largely ignored by empiricism. Within the social scientific way of life, how do our social relations help form and constrain scientific conclusions?; how do our scientific conclusions participate in shaping the constitution of social reality. But first it will be useful to focus closer attention on some dimensions of researcher action with the subjects of social inquiry.

Controlling Antecedent Conditions

The term "antecedent condition" refers to researcher defined social or physical conditions that are thought to affect human behavior in some way. Together, "antecedent" and "consequent" conditions represent the key concepts of theoretical models; the hypothesized relationship between these two elements is the subject of empirical study. The researcher may ask, for example, does "frustration" (antecedent condition) lead to increased "aggression" (consequent). E.P. Willems (1969, p. 46) has defined the dimension of controlling antecedent conditions as "the degree of the investigator's influence upon, or manipulation of antecedent conditions of behavior." The act of controlling antecedent conditions is of course the hallmark of what has come to be called the experimental paradigm. Researchers communicate persuasive messages, present carefully designed experimental tasks, and in general expose "subjects" to highly controlled and systematically varied sets of "stimulus conditions." Research strategies in which investigators may not control antecedent conditions include a wide range of survey research projects and observational studies. While behaviors or verbal reports are carefully measured, the researcher has no control over preceding conditions. Although these investigators may sample across systematically defined populations (socioeconomic levels, education levels, etc.), they do not control the prior experiences of those they are researching.

The use of systematic researcher control permeates well beyond the confines of the laboratory experiment. Similarly, exerting control over antecedent conditions is not a necessary feature of experimentation. The focus here remains, what the researcher does, not where research occurs or what the activity is called. Participant observers, for example, often include elements of researcher control as one tactic for generating information. Self-conscious violation of social rules (Turner, 1974) or judicious attempts at role consonant behavior are often effective for creating or controlling a situation in the interests of generating opportunities to observe the response of others (McCall and Simmons, 1969; Wheeler, 1978). Clinical and "depth" interviewers "probe," "assume roles in the relationship," and utilize a variety of other techniques of explicit interpersonal control in the interests of eliciting interpretable responses. Moreover, it should be recognized that serendipitous or naturalistic experiments may sometimes be instances of true experiments in the field, the conditions occurring outside of the researcher's control. Social programs instituted by governmental agencies, natural disasters, or draft lotteries may offer occasions of low researcher control and random assignment of individuals to experimental groups (Cook and Campbell, 1975, pp. 116-195).

Researcher control over research conditions is an activity that serves the researchers' own scientific purposes. This activity may be referred to as "experimental control," "role participation," "role taking" or any number of other designations. The controlling relationship is designed to create systematic variations in social conditions for the purpose of researcher theory testing, and/or to create surrogate events to stand-in for difficult to study natural phenomena (e.g., highly frustrating situations or infrequent and private social role behaviors). Similarly, investigators may choose to sample rather than control antecedent conditions when this strategy better serves their theoretical needs. Of course, describing and sampling is no less an action than directly controlling; it is simply a different kind of action. The investigator *characterizes* populations or situations (e.g., ghetto dwellers, high density rooms, successful women, etc.) and then observes some theoretically relevant behavior of his/her choosing. Controlling or sampling, researchers must act to define, manipulate, and/or measure any antecedent conditions relevant to their purposes.

## Imposing Units of Measurement

A second dimension of the researcher's action and decision concerns the extent to which s/he imposes restrictions on the range or spectrum of behavioral responses that will be considered data. To what extent does the investigator determine or limit the form of measured and recorded responses. At the extremes, social scientists may limit measured responses to the check marks on a preformed scale. On the other hand, the researcher's use of open and unstructured interviews may allow respondents to relate a wide variety of verbal and nonverbal information that may later become part of the analyzable data.

When investigators structure units of measurement they are assured of detecting predetermined relevant information and they are assured of having that information in the form most useful to them (i.e., observations pick-up all relevant dimensions of predefined behavior, measurements are scaled as the researcher desires). When investigators avoid imposing units of measurement, observational information may not occur in expected forms. Participants may utilize unexpected behavioral and verbal responses styles. This may be viewed as relevant and informative in and of itself. The point is, both investigative actions serve to shape the relationship between researcher and participant and the form of subsequent data.

Roger Barker (1969) has described two types of relationship between the investigator and those being studied. The psychologist as "operator" (O Type Data) and the psychologist as "transducer" (T Type Data). O Data involves the active participation of the investigator in regulating input or affecting the phenomena (controlling antecedent conditions), followed by the psychologist's "translating" the observed outcome (imposing units of measure). Researchers collecting T Data, on the other hand, attempt to avoid operations on the phenomena and instead concentrate on the translation, or coding, of observed responses. In both cases, however, Barker describes the psychologist as highly involved in translating, coding, or imposing units of measurement. The ethological tradition has concentrated more attention on this issue of translating observed phenomena. Traditionally, the ethological orientation has stressed the importance of detailed description avoiding researcher imposed units of measure in order to facilitate the accurate representation of the behavior observed (Marler and Hamilton, 1966; Schneirla, 1951). Hutt and Hutt (1970) drew a clear distinction between "ethological" and "ecological" (Barker, 1965; Wright, 1967) approaches around this very issue. Their distinction clearly highlights the fact that researchers within the two orientations, who are in agreement about eschewing control of antecedent conditions, vary in the degree to which they see themselves as imposing observational units of measurement. From Hutt and Hutt's point of view, investigators using ethological approaches adhere more strictly to behavioral description, at least initially disallowing imposed categorizations or organization of behaviors (p. 22-25).

Other research traditions have also emphasized the implications of using methods of measurement which impose a priori restrictions on the recording of phenomena into data. Interviewers as well as participant observers may vary in the degree to which they impose units of measure. The same issue has been considered by those concerned with the relative advantages and limitations of unstructured procedures and participant observation (Dean et al., 1969; Vidich and Shapiro. 1955). Several authors have pointed out that within almost any research project the observer may fluctuate from less structured approaches to frequency counts of operationally defined behaviors, in the latter case imposing categories or units on the stream of behavior (Lofland, 1971; Becker, 1958).

The methods of Kendon and Ferber (1973) in observing human greetings may be used as a clear example of decisions to avoid constraining measurement units with a priori impositions. After thoroughly documenting the physical environment, context, and nature of the population under study, these investigators used film and video-tape recording to observe the greeting behavior of some middle-class adults at a private outdoor party. Cameras were kept at the edges of the party site. Guests were filmed from the time they approached on the walkway until they parted from the host or hostess. Initially, the only restrictions on filmed observations were the definition of the phenomenon of interest. Cameras focused on greetings from the time the guests first came within sight of the host-hostess and earlier arrivals. It may also be noted here that this research strategy may be seen as exerting very little control over antecedent conditions. The party was a real event. The investigators had no control over the existing relationship of guests to each other, time of arrival, order of persons in meeting each other, etc.

Kendon and Ferber's investigative strategy of avoiding imposed response categories and controlled situations resulted in both gains and losses of pertinent information. Any questions or verbal probes would have elicited responses partly shaped by the structure of their questions. On the other hand, avoiding such impositions resulted in the loss of any access to internal experiences associated with the greeting behaviors that were the subject of inquiry.

The research paradigm used by Byrn (1969) to investigate the relationship of similarity and attraction between persons may be used to illustrate researcher actions designed to highly control behavioral antecedents and impose rigid units of measurement. Each respondent is given a short attitude questionnaire on some topic. He/She is then shown another copy of this questionnaire, supposedly completed by a second individual. The responses on this second questionnaire vary in the degree to which they are "similar" to the responses of the respondent (independent variable). Finally, liking or attraction of the respondent toward this hypothetical stranger is measured by two questions (How much do you think you would like/dislike this person? and Would you enjoy/dislike working with this person?). The respondent places an "X" on a seven point scale, indicating the degree of "attraction" (dependent variable). The antecedent condition (similarity of respondent and hypothetical stranger) is under the total control of the investigator as communicated through the presented questionnaire responses. Likewise, the investigator imposes rigid units of measurement, allowing participants to express "attraction," but only within the structure of their answers to two scaled questionnaire items.

Of course, researchers that impose units of measurement may utilize sampling rather than control of antecedent conditions in forming their investigative strategies. For example, Byrn's procedures may be contrasted with other research cited by him concerning the same theoretical issues. Several researchers measured the attitudes and personality traits of married couples and friends in an attempt to assess the degree to which these persons (presumably attracted to each other) were similar on these dimensions (Schiller, 1932; Kirkpatrick and Stone, 1935; Schooley, 1951; Richardson, 1940). The measured variable, similarity, was once again obtained by imposing limitations on the participants' response style. Each completed a set of scaled questionnaires (high imposition of units). On the other hand, the investigators had no control over the antecedent conditions of attraction in the relationships.

Controlling antecedent conditions and imposing units of measurement are two independent dimensions of researcher action in relationship with participants. Moreover, decisions and choices along these dimensions of control and measurement shape the information available for subsequent analysis. One way or the other the investigator must act, and the consequences of his/her actions will help define the relationship with participants, the shape of resultant data, and therefore the range of rational interpretations available.

## Concealing the Investigative Intentions

There is by this time a rather extensive literature concerning the reactivity of persons to being observed, measurement as a change agent, evaluation apprehension and related topics (Orne, 1962; Rosenberg, 1969; Rosenzweig, 1933; Campbell and Stanley, 1963; Cook and Campbell, 1975; Argyris, 1968; and others). Scientists have concentrated much attention on "subject reactivity" as a threat to "veridical scientific investigation." Their approaches have varied depending on orientation, beliefs, assumptions, judgments and the particular objectives of the researchers carrying out the project. It is important to note however that their concerns have focused on creating the "correct" and "effective" methods of empirical investigation. Without referring to "validity issues" or "subject reactivity" it is still possible to characterize researcher relationships with respondents by the extent to which the research enterprise is made explicit and salient to those participating. To what extent does the investigator disguise, hide, or submerge the agendas, intentions and objectives of his/her relationship with respondents.

Discussions of "unobtrusiveness" in research have been concerned, in general, with strategies for lowering the saliency of "observation," "investigation," and "theoretical hypotheses" within the researcher-participant relationship. Webb et al. (1966) discussed this in terms of removing the instruments of observation from the participants' view. The hidden camera, hidden counter, or natural archival study would be examples of such a tactical approach. This orientation represents the most common understanding of unobtrusiveness. These same concerns have been reflected in the common researcher strategy of "deception" and concealment of hypotheses in social psychological experimentation (Foreward, et al., 1976). While the participants often know they are in a psychological experiment, complex procedures and "cover stories" are used to obscure the purposes and hypotheses of the study. It is, of course, equally true of many participant observers that they do not disclose all their intentions or preliminary hypotheses to those being studied. Similarly, survey researchers and interviewers will maintain an openness about individual questions while concealing broader research intentions, theoretical perspectives and hypotheses.

Of course, concealing research goals and intentions from participants is a two-edged sword. While researchers may gain some assurance of "noncontamination" (e.g., ignorance of researchers' hypotheses and expected results), concealment is also recognized as alienating the researcher from the perceptions, beliefs and intentions of those being studied. Participant observers will often reveal the full nature of their research enterprise to selected informants in the interests of eliciting useful and unexpectedly pertinent information. Recently, social psychologists have shown renewed interest in role-playing techniques as a means of gaining more insight into social phenomena. These techniques often include relatively full explicitness about research interests and agendas (e.g., Mixon, 1971; Baron, 1976). Mixon, for example, asked participants to engage with him in a role playing replication of the classic Milgram (1963) obedience study. Each person played all roles (subject, learner, experimenter, and bystander). Through eliciting self-reflections on personal experience and systematically varying elements of each role, he was able to shed new light on the interpretation and human meaning of the "obedience" phenomenon as it was originally reported by Milgram.

Both concealing and nonconcealing researcher strategies constitute decision/actions that shape relationships and constrain subsequent conclusions. Concealment tends to alienate the researcher from participants' phenomenal experiences and interpretations of the situations observed or constructed by the investigator. Revealing intention and theoretical objectives raises questions about participants' motives of self-presentation, and their compliance/ resistance vis a vis the researchers' favored hypotheses.

Once again, it is clear that researchers must act, and at the same time any actions on their part serve to shape and constrain conclusions and interpretations.

Controlling Research Contexts

Researchers may not only act to control the forms of observation/measurement and the introduction of antecedent events, but also some elements of the context or milieu within which these research relationships are pursued. To what extent does the researcher control or on the other hand describe the physical and social backdrop for the investigation of the focal theoretical issues? Who creates the circumstances within which data are collected? Who controls the dimensions of legitimate activity, the researcher or the participants? The classic laboratory study represents an attempt by the investigator to manipulate theoretical variables, measurement procedures *and* the surrounding context of the research relationship. The subject engages in the researcher's tasks, responds to the researcher's questions within the researcher's controlled domain, designed to serve only the purposes of the researcher. Field experimenters may control research variables of interest and the form of observations while allowing the surrounding context to continue unperturbed. Sherif and his colleagues (1961) for example, took advantage of the routines at a boys' summer camp to systematically intervene in games and camp tasks and observe the consequent patterns of cooperation and competition among the campers. However, they did not control the surrounding context of the summer camp experience. The researchers' enterprise was embedded within a context defined by others and serving to achieve other purposes.

Popular mythology within the social scientific community has muddled the discussion of context. Field research (a category typically used for discussions of contextual issues in research) has been discussed and/or endorsed by numerous authors making a variety of points: Brunswik (1955) representative design; Barker (1965) and Wright (1967) ecological psychology; Weick (1968) systematic observational methods; Sanford (1970) action research; McGuire (1967, 1973) changing orientations in social psychology, just to mention a few. Such a state of affairs could easily confuse the concept as it is defined here. Contextual control refers to the activities and choices of a researcher which are designed to control the milieu *within* which he/she focuses on behavioral variables of theoretical interest. As I have implied above, the decision to control the research context is independent of decisions about controlling theoretically defined antecedent variables or the extent to which measurement units are imposed by the investigator. The laboratory investigator creates a controlled context in order to establish "uncontaminated" and "simplified" surroundings within which to manipulate and measure behaviors of interest. Other researchers (such as Sherif et al., in the Robbers Cave Experiment, or social program evaluators, Campbell, 1969) may control chosen conditions but measure "effects" *within* uncontrolled contexts that serve purposes other than those of the researcher (boys camps are for fun, and participants in social programs are pursuing their own lives not the interests of science).

The distinction between "theoretical variables" and "context" in social research is determined by the researcher's perceptions of "figure and ground." And in the same way that the well-known visual illusion may shift from "vase" to "human profiles" depending on how you look at it, "theoretical variables" and "context" may reverse themselves depending on the researcher's point of view. This is of course true whether the researcher chooses to control or describe the context which surrounds the theoretical phenomenon. For many years the "experimenter" was simply part of the context ("Procedures were carried out" as the studies often read) of social psychological experiments. By focusing on the experimenter and "experimenter effects," Rosenthal (1966) and others reversed the perceptual field such that the experimenter was considered a "critical variable."

This figure-ground reversal created reverberating effects within the field permanently altering the way researchers discuss, conduct and report experimental studies. In a similar fashion, the current rhetorical vogue advocating emphasis on social psychological research with more representative populations was at least partially a result of increasing concerns that the use of college sophomores in research was not simply contextual background but perhaps a critical determinant of research results. All social researchers act to define focal phenomena and explanatory concepts as distinct from surrounding contexts. Empirical researchers, with these perceptual frames in mind, conduct their research relationships through controlling and/or describing their conceptions of the contextual surrounding vis a vis their chosen theoretical phenomena.

### Defining the Rules by Which Phenomena Are Translated Into Data

From the researcher's perspective the empirical investigation is an exercise in structured observation for the purpose of generating information. Verbal description and physical procedures that clearly define concepts and actions allow the researcher's experiences to become publicly accessible and interpretable. Like anyone else, scientists are most clearly understood when they can communicate the meaning of their concepts, definitions and perceptions with as little ambiguity as possible. There are a variety of researcher strategies for establishing clear definitions in empirical research. All of these may be understood as various ways of defining the rules and procedures by which phenomena are conceptualized and translated into data.

*Operational description*, as a strategy for empirical definition, while rarely used in its pure form, still enjoys wide acceptance among social scientists as a standard of aspiration. Concepts must be defined utilizing discretely observable behaviors and explicit rules of combination (e.g., certain behaviors are defined as "aggressive," the more of these behaviors that occur [summative combinatorial rule] the higher the person is in "aggressiveness"). High amounts of palmer sweating and skin conductivity (GSR), for example, may be defined as data indicating that a person is physiologically "aroused." However, behind this definition are the *rules* for translating the phenomena (palmer sweating) into data (GSR measures). The rules in this case are constituted by the description of the electronic measuring instrument and the procedures for its proper use. Knowing the rules (the equipment specifications and procedures) any investigator wishing to use the same operational definition of "arousal" (palmer sweating and GSR) could be assured that s/he is considering the same empirical concept and phenomenon as the first researcher. A concept is empirically defined solely through the operational rules for translating the phenomenon into data. Other examples of the operationalist approach may clarify this point: attitudes are defined as individuals' scores on *properly constructed* attitude scales; intelligence is an individual's score on a *properly constructed* ("reliable" and "valid") intelligence test. And the rules of "proper construction" are defined in terms of standard attitude scaling and psychometric procedures. As an ideal there are clearly prescribed procedures and actions (reliability tests, item stability checks, predictive validity tests, convergent validity tests, and so on) that constitute the *rules for translating* (scale construction) these phenomena into "data."

While operational description is still a common aspiration, in practice many social scientists use definitional strategies that are a good deal less explicit and rigidly constructed. The two major categories may be characterized as intuitive and introspective descriptions.

Kaplan (1964) defines intuition as "(1) preconscious, and (2) outside the inference schema for which we have readily available reconstructions" (p. 14) While *intuitive description* is based on observables, although perhaps less well explicated than in the operationalist strategy, the rules for combining observables to define a construct are left undescribed or loosely described. Wright (1967) emphasizes that definitions and descriptions in this model rely on "everyday knowledge and perception" (pp. 24-25), common sense. He cites an example which may clarify the distinction between operationalism and what I am calling intuition. From Wright's point of view this record is incomplete:

> John's fist swung upward through an arc of 30 degrees and landed on Henry's chin.

He suggests this as a better record:

> John hit Henry with apparent intent to hurt him.

The basis of intuitive description is clearly within publicly observable behaviors. But the observer might be hard pressed to describe the rules by which the construct "intent to hurt" was intuited. This represents a non-public, unexplicated process of translation.

Winch (1958) offers a similar example in describing a cat which is seriously hurt.

> We say the cat "writhes" about. Suppose I describe his very complex movements in purely mechanical terms, using a set of space-time coordinates. This is, in a sense, a description of what is going on as much as is the statement that the cat is writhing in pain. But one statement could not be substituted for the other. The statement which includes the concept of writhing says something which

> no statement of the other sort, however detailed, could approximate to. The concept of writhing belongs to a quite different framework from that of the concepts of movement in terms of space-time coordinates. (p. 73-74)

"Writhing" is an intuitive statement, based on observable events but going beyond these events through inference rules that are difficult if not impossible to explicate.

*Introspective description* refers to use of the observer's subjective experiences as sources of data. These experiences are, of course, not publicly observable and the rules for their construction relatively unaccessible. Perhaps the clearest example of the use of private experience is the clinician's attention to his/her feelings toward a client at a particular time as information about what the client is expressing. Such references are common in case study reports. Introspection also appears as one of the strongest arguments for participant observation as a rich data collection strategy. Perhaps the greatest contribution of this method for social science is its emphasis on the significant data obtainable by observers attending to their own responses in the participant role (McCall and Simmons, 1969; Lofland, 1971). For example, Howard Becker (1963), in his study of jazz musicians, had inside information, special knowledge unattainable except for his experience as a participant in that group. This information was not publicly available and the rules by which he constructed perceptions were inexplicit.

Social scientists may choose to use operationalist, intuitive and/or introspective strategies for defining concepts, behaviors and activities within the empirical investigation. Donald Campbell (1974) has argued that even highly structured laboratory research, ostensibly relying only on operational rules for definition, contains significant elements of the intuitive process. And likewise, participant observation reasearch may sometimes rely heavily on operational definitions more suitable for quantifiable analysis (Becker, 1969). But one way or the other, "description" must involve both a phenomenal event and some action by the investigator. The investigator must set the rules by which these phenomena will be translated into data.

The empiricist perspective constructs an understanding of the research enterprise and the dimensions of social scientific activity within its own framework of aspirations: "accurate description," "valid conclusions" and "universal laws." These aspirations and the consequent construal of research activity as "the logic of discovery" have tended to deemphasize the human decisions and actions that constitute the investigative endeavor. The purposes of this chapter have been to reorient attention to decision making and actions that occur when researchers interact with other people in the name of science. Through these actions the investigator shapes the structure and form of his/her relationship with the subjects of empirical inquiry.

I have argued previously (Chapter II) that social science is necessarily the study of both social concepts and social behavior. Moreover, social concepts arise out of relationships among people. Social inquiry is itself a social event and takes place within meaningful relationships. This definition of social science and social reality is of course circular; and that is precisely the dilemma of the human condition. Our understanding of ourselves is largely constituted by who we are and the form of our human relationships. If social scientists wish to pursue inquiry into the forms of social phenomena then they must come to grips with the implications of their own active participation.

I would like to argue that as social scientists we constrain our perceptions through our social perspectives and that these constraints are manifest in the way we interact with those we study. Researcher actions and decisions within empirical investigations shape research relationships and therefore the scientific conclusions that emerge from them. Through the institutions of social science, research reports, popular science, applied research and consultation, social scientists participate in the larger social drama. That is, our social perspectives inform our participation in the constitution of the same social reality that is the object of our inquiry.

# CHAPTER IV

## RELATIONSHIPS, COMMON SENSE, AND USEFULNESS

When researchers make scientific decisions and engage in investigative actions they necessarily shape a particular relationship with those who are the subjects of their inquiry. And because this must be true, the empirical enterprise cannot be fully understood without considering the implications of these researcher actions and relationships. Research methods are indeed more than mere "tools" for discovery. They are researcher actions within a relationship with those being researched. And a refocusing on these relationships and their implications for social science begins to clarify new aspirations for an empirical enterprise without empiricism.

To say that social scientific research is constituted by researcher actions in relationship with those researched is not in itself a criticism of the endeavor. Rather, by highlighting scientist participation in the empirical enterprise I am simply proposing that social scientific methods cannot separate the social scientist from human relationships and human responsibility. Like all other human activities empirical research and social theory are participatory, collective and social undertakings. By drawing out the dimensions and implications of our research relationships, it may be possible to engage in them more self-consciously, and more reflectively. In so doing, social science will have to relinquish its position as the priesthood of facts. As an alternative, however, social scientists may begin to form another relationship of social responsibility and social participation in good faith.

The following discussion will consider the actions which shape empirical relationships as forms of constraint on the social reality that may be understood within them. It will be argued that these relationships necessarily manifest fundamental social perspectives, and often these perspectives are implicated in a social and normative way of life. The ways in which empirical research relationships are constrained will be viewed as a manifestation of the way the researcher implicitly construes social reality. The way in which social existence is understood helps define the boundaries of social living, the alternatives within conflicts, the shape of options and opportunities, and the rules for success and failure, normality and deviance. The social perspectives and the social reality that constrains the research relationships also constrains the potential uses of subsequent researcher conclusions, theories, and "facts." It is then pertinent to ask, what kind of relationship is occurring in the research activity? What is the role of control and compliance, selection and acceptance? What is the role of concealed intentions in social scientific inquiry? And finally, when researchers "make sense," "name the reality," of empirical relationships, who's "sense" is this, who's "reality," and within which way of life?

Researcher Actions and Researcher Relationships: Control and Selection

A reconsideration of the dimensions of researcher action casts new light on some old and rather tired dichotomies. Characterizations such as behavioral vs phenomenal, experimental vs qualitative, laboratory vs naturalistic and others have been batted about as alternative strategies for competent investigation of social phenomena. They have been debated for their utility in gleaning the "facts" from a complex stream of social behavior. While it is clear that the presumptions of such controversies are misguided, there are important distinctions within these debates and the distinctions roughly correspond to two modes of action used by researchers as they shape their relationships within the empirical inquiry: control and selection.

Elements of *researcher control* are easily enough recognized. For the purest cases, the laboratory experiment is brought to mind. Highly controlled stimuli are presented to participants (controlling antecedent conditions) and the responses are measured by structured task performances or rating scales (imposing units of measurement). The experiment takes place within a researcher controlled setting (controlling contexts) and responses are operationally defined, precisely and mathematically constructed, according to rules chosen by the researcher (operational definitions). There are, of course, *elements of selection* involved as well. The researcher chooses to select randomly from a population of college students, married couples, local gay communities, U.S. citizens, and so on depending on the researchers' theoretical interests.

A somewhat different approach corresponds to research strategies often described as ethnographic, participant-observation or qualitative techniques. The researcher will join with those being observed in an ongoing setting, controlling neither antecedents nor contexts. The investigation may describe "causes" and contexts as they are described by participants, using their units of measurement and utilizing their explicit and/or inexplicit intuitive and subjective styles of combining information about experiences. But here too the *researcher selects* and samples a population of his or her own definition (e.g., neurotic clients, young medical interns, jazz musicians, aggressive individuals, etc.) according to particular chosen theoretical interests.

One way or another, of course, the researcher acts, decides and participates in the research relationship. All empirical inquiries contain elements of researcher control and/or selection in varying degress. The debates over which strategies are most effective tend to melt into irrelevance, however, as one considers the character of the relationships that follow from these scientific actions and decisions.

## Elements of Control and Selection: The Professional Relationship

Research relationships in which the researcher exerts control can only proceed smoothly to the extent that the participants are willing to cooperate. Highly controlled relationships are relationships of asymetric power and compliance. Subjects must be willing to attend to the researcher's stimuli, accept the researcher's tasks, and express themselves through the categories and units presented by the researcher. When researchers enter into relationships in which they hold the power of control, and when participants cooperate and comply, then we may say that they are engaged in a legitimate professional relationship.

Elements of professionalism have been recognized by others as inherent forms of relationship in experimental research. Ingle (1977) offered a persuasive review pulling together the dimensions of "professionalism" and "legitimate power." Interviews with prospective experimental subjects have indicated that they expect experimenters to display competence, efficiency, and concern for the subjects' physical and psychological well-being, making sure not to embarrass, harm, or disrespect them (Epstein et al., 1973; Schulman and Berman, 1975).

> In effect, subjects are willing to contribute their time and effort if the experimenter is a decent human being and a competent professional. These same demands would, no doubt, also be made of physicians, lawyers, ministers, accountants, etc. Each of these individuals would be expected to be competent in his field, to respect his client, and to adhere to the norm of privileged communication. Hence, when a subject is asked to rate the appropriateness of incompetence, violations of confidentiality, or lack of respect, he indicates that they are highly inappropriate. (Epstein et al., 1973, p. 218)

Epstein et al.'s understanding of the experimental relationship as a "professional" relationship is based on well-known concepts of "legitimate power." Professionals, and researchers as a case in point, derive their legitimate power from broad cultural value which give "experts" and "specialists" the right to influence others within prescribed contexts (Collins and Raven, 1969; French and Raven, 1959; Ingle, 1977).

In order for the researcher to establish a legitimate professional relationship s/he must be recognized as a legitimate professional and an expert with certain privileges. Moreover, the research relationship tends to breakdown if, (1) subjects do not recognize the legitimacy of the relationship at the outset, or (2) the researcher is perceived as having violated the bounds of professionalism through incompetence or disrespect of the participant. That is to say, in order for the researcher to successfully establish a relationship of control and professionalism, s/he must *select* participants from a population of indivduals who will comply because they are willing to recognize the legitimacy of the relationship and perceive the tasks and forms of interaction as competent, respectful, and reasonable.

It is instructive to consider the elements of researcher control and professionalism, and participants' compliance as relationships of tacit agreement. When highly controlled research goes well, all parties agree to interact within a particular relationship and set of constructs. They have agreed that the relationships and constructs do not seriously violate propriety or their mutual sense of reasonableness and common sense. This agreement and shared point of view constitutes the basis of their professional relationship and the shape of resultant empirical results. The necessary correspondence between researcher and participant points of view in controlled research is easily illustrated through consideration of tacit agreements and "error variance" in some typical experimental work.

Consider a typical experiment, concerned with human responses to noxious experiences under varying conditions of avoidability and information about its onset. More generally, it was an experiment addressing issues of behavioral response under varying conditions of respondent control.

> Briefly, as subjects awaited an electric shock, not knowing when it would occur, they were free to switch back and forth between two channels of a tape recorder. One channel provided a warning signal 5 seconds before the shock was to occur; the other channel provided only background music. If subjects chose to listen for the warning signal they could press a button upon hearing it, and perhaps avoid the electric shock. The effectiveness of the avoidance response varied across trials, being either 100%, 66%, 33%, or 0% effective. (Averill et al., 1977, p. 396)

The researcher instructed each subject that they might control the shocks by attending to the tone and pressing an avoidance response button. The research questions concerned the amount of time that subjects would listen to the tone (rather than irrelevant music) thereby increasing their chances of controlling shocks through their vigilance to the warning signal.

The researchers, in controlling and shaping the experimental tasks, created a situation in which subjects' potential control over shocks varied from 0% to

100% if they pushed the avoidance button. That is, the researchers *invited the subjects to so construe their control* over shocks. But this is not the only way to understand the situation. This perspective directs attention to the "probability of shock" as the focus of defined control. If fewer shocks occur subsequent to avoidance responses then that constitutes potential control. However, this situation could be considered to be one in which the relevant referent was not only the probability of shock but also the research relationship in a larger context. That is, the experimenter *allowed* the subjects certain probabilities of shock if the avoidance button were pushed, and the experimenter could change or withdraw that condition. The experimenter, rather than the established shock probabilities, could quite reasonably be considered the focus of any definition of control in this situation. It is clear that anyone so construing the experimental situation would, having already agreed to participate, perceive little meaningful variability in his/her potential for control. The experimenter maintained control and the designated probability of shocks would be considered relatively trivial in comparison to this one overriding fact.

The entire experiment was based on a shared point of view, that researcher bestowed "control" and a focus on shock probability constituted a sensible domain of control. Without that tacit agreement the meticulously varied experimental conditions had no sense. It is interesting to note in this regard that most subjects utilized very instrumental and vigilant coping styles, attending to the warning tones and doing so more frequnetly as the effectiveness of the avoidance button was increased by the experimenter from 0% to 100% effective. However, 15 of the 80 subjects showed very inconsistent attention to the warning tone, for example, attending when there was 0% or 33% effectiveness in avoidance attempts then not attending when the avoidance button was 100% effective. Post-experimental interviews revealed that,

> Some of these subjects evidently were trying to demonstrate their control over the entire situation by doing what was counterintuitive. Others switched coping strategies for the sake of variety, or in order to test the stated probabilities. (Averill et al., 1977, p. 411)

These "inconsistent" subjects were considered "error varience" in this research, that is their behavior was unexplainable. It is quite possible that these participants were those for whom the researchers' concept of "control" was not sensible or meaningful. They looked at this relationship and perhaps other "control" relevant relationships in their lives from another point of view.

There are several important points to be made about this particular experiment and others like it. First, it is quite competent and well done within the empiricist perspective. In fact, the authors were unusual in their attention even to those who constituted the "inconsistent responders," recognizing that something more than "error" or "random" variability might be operating. It is most important to note the fundamental role of the relationship between the researcher and researched in constituting the outcome of the study. They were in agreement (on the whole) about the legitimacy of the relationship (researcher control), and on the sensibleness of the implicit definition of control represented by the task. Without this agreement the participants' responses degenerated into "inconsistency" or "randomness." Moreover, the alternative concept of control, outlined above, that focuses on the experimenter-subject relationship and a wider contextual definition of control *cannot* be studied within researcher-controlled empirical strategies. The relationship within the setting constrained the conclusions, viewing one perspective on control and its constitutive behavioral implications while being totally blind to alternative conceptions. It is also important to recognize that it is only within a social population where the experimenters' concepts represent common sense for the majority that the professional relationship that legitimates control can be maintained. For most, this task seemed not so outrageous as to imply incompetence or disrespect. Compliance with the experimenter would therefore seem reasonable. For the minority, resistence to a familiar form of nonsense simply elicited a passive participation and tolerance.

It is not necessary to restrict examples of researcher control and professional relationship to the extreme cases of experimental procedures in order to illustrate inherent constraints or empirical outcomes. A recent article by Rubin and Mitchell (1976) focused explicitly on "Couples Research as Couples Counseling." Although their research only exerted control through posing researcher-selected questions and scaled-response options (imposing units of measurement), subsequent reports revealed strong effects as a result of their inquiries within a legitimate professional relationship. Couples responded in a number of ways to the researchers' questions. Nearly 50% of those randomly sampled stated that answering the questionnaire had affected their relationship with their partner in one way or another. Nearly 50% claimed no effects from their participation, and a minority expressed offense at the form of the questions.

According to Rubin and Mitchell's data the participants reported that the questionnaire, the issues raised in it, and the implicit definitions implied by a list of "important issues" affected both the participants "process of definition" and "disclosure" within couples. That is to say, many couples took the researchers' concepts of intimacy seriously enough to prompt personal redefinition, discussion and disclosure.

Consider the role of legitimate professionalism and compliance in shaping any conclusions from their research. For those who felt their relationships were unaffected by answering the questionnaire it may be assumed that the implicit definitions contained within the questions were already shared (e.g., issues of equality and relative expectations within couples) as common-sensical, relevant and important to intimacy.

On the other hand, a minority were offended by the form and possibly the substance of the questions.

> I am no longer able to place or determine statistical variances on my emotional relationships. Please don't bother me. (p. 22)

And, of course, aside from those who explicitly expressed offense, others may be contained within the 17% of women and 25% of the men who refused to respond to follow-up questionnaires.

In the middle, between those who found the questionnaire to operate within common sense and reasonableness and those who were offended and felt degraded, were a large group who had not initially construed intimacy in the ways it was understood in the questionnaire. They were, however, willing to alter their definitions, engage in more disclosure, and ultimately reshape their personal relationship as a consequence of exposure to the researchers' questions and concepts. Some typical comments by respondents clearly reflect the power of asking questions within a legitimate professional relationship.

> I had to think about each inherently cryptic question. (p. 24)

> . . . some of the questions were really soul searching. (p. 19)

> Your study did bring out a difference in our outlooks which proved more important than I had realized at the time. (p. 21)

> Perhaps you might consider opening a course on this idea for couples planning marriage. It would give you a good idea of yourself, your compatibility with your partner, and plans for working together on each of your needs and finding your goals. (p. 25)

It seems clear that the results of this survey were necessarily constrained by the legitimacy of the relationship between the researchers and the respondents. The findings reflect "facts" about couples who either shared the researchers perspectives on intimacy or were willing to respond to researcher expertness on the issues by redefining their own concepts of relevant issues, dimensions of intimacy and the importance of self-disclosure. Those who found the implicit concepts of intimacy personally demeaning either rejected the legitimacy of the researcher actively or passively through non-response. The processes of researcher control and legitimacy, and selection of respondents who would recognize and respond within this context necessarily constrained any "facts" that could result from the research. Once again the form of the research relationship, the actions by the researchers and the perceived legitimacy of the relationship, constituted the constraints on resultant conclusions about the "nature" of intimate relationships.

Researchers may exert various forms of control over participants to the extent that they can maintain their positions as legitimate professionals. And maintaining this legitimacy depends on mutual agreements with the subjects of inquiry that they have acted with competence, reasonableness and personal respect. Research conclusions within such a relationship are inherently constrained by mutual concepts of legitimacy, competence, common-sense, and propriety. It is the general misapprehension of these mutually constitutive relationships within empirical reality that has allowed researchers to remain unaware of their own actions in conversing and shaping existing social constructs. But, of course, researcher relationships are not only constrained in the acts of control described above. Alternatively conceived, non-controlling research relationships contain within them the same potential for unrecognized researcher action.

## Elements of Selection and Acceptance: Peer Relationships

For a variety of reasons some social scientists have turned away from controlled research and toward methods variously described as ethnology, participant observation, or qualitative observation. Typically they choose research relationships that avoid controlling situations and possible antecedents to behavior, avoid controlling contexts within which observations are made, and attempt to describe using the same units of measurement and intuitive-subjective concepts that are used by those that are the subjects of the inquiry. To the extent that research strategies may be characterized in this way it may be said that the researcher and participants are involved in a relationship of legitimacy through acceptance, a relationship of peers. The researcher is attempting to join the subjects of inquiry to see the world and social experience through their eyes.

And the research can only proceed within this relationship to the extent that the observer assumes the perspectives and points of view of those observed and in some respects is accepted as an insider within their way of life.

The principle mode of researcher action within this strategy resides in the selection of the subjects of inquiry. It is self-evident that in order to participate and join in a way of construing the social world the researcher must select and define a social aggregate that is understood to be an internally coherent entity. However, simply referring to this researcher action as "selection," does not do justice to the complexity of such an act and the ways in which selection is both characterization and description.

In any but the most isolated societies individuals can be characterized, and do in fact characterize themselves, in a variety of different ways. Individuals who may be doctors, are also citizens, children, consumers, democrats, Black Americans, stockholders, New Yorkers, veterans, lovers, and so on. To select a group of "doctors" or "consumers" is an act of characterization by the researcher. And this act will constrain and define what it means to "join" and assume the perspective of that social group. It, of course, begs the question to ask how the individual would characterize him/herself. Given persons can easily see themselves as members of all the groups listed above. Or an individual might never characterize him/herself in a way that most "outsiders" would (e.g., "terrorists" "guerilla-bandits," "criminals," "traitors," "degenerates"). Which role might be a primary self-definition depends on personal biography, historical and cultural times, and even transitory settings (e.g., at home vs in the doctor's office).

So the researcher characterizes and defines a group of individuals then attempts to establish a form of rapport with them that will allow participation in viewing social experience through their eyes. And constraints on what may be observed and the form of research conclusions will reside in part within the complexities of selection that define the individuals who will be considered the participants in this social perspective. There is, of course, a paradox existent within the acts of selection-characterization on the one hand and the assumed researcher relationship of participant-peer on the other. Any reflection, explanation or description of the population under study must inevitably take a perspective that justifies or explains the selection itself from the perspective of an outsider, for those who share some other outsider point of view. True insiders or participants do not explain their participation in such terms except perhaps when interacting with an outsider. A prisoner's explanation of why he/she is an insider (as it were) will take on a different texture depending on whether the audience is other prisoners or some group of sociologists. Insiders are not likely to describe themselves by looking through the eyes of others. They are not likely to describe their demographic make-up, social historical context or position in society as a function of socio-economic level. These are the descriptive terms, perspectives and points of view of an outsider. Moreover to fail to maintain such a perspective runs the risk of "going native." An observer is considered to have "gone native" at that point where s/he may begin to:

> . . . incorporate the role into his self-conceptions and achieve self-expression in the role, but find he has so violated his observer role that it is almost impossible to report his findings. Consequently, the field worker needs cooling-off periods during and after complete participation, at which time he can "be himself" and look back on his field behavior dispassionately and sociologically. (Gold, 1969, p. 34)

Of course, "dispassionate" and "sociological" is exactly what true insiders are *not*. At the same time this is a necessary perspective for anyone who wishes to report "findings."

The distinction between insider perspectives and the point of view of outsiders reporting with access to inside information is constituted by differences in point of view. It can sometimes be seen by looking separately at participant-observation field notes, reported descriptions of the experience in the role of participant, and the wider contextual perspective that describes the research population. These three elements of the research project can chronicle immersion, "going native" and regaining of the "dispassionate" outsider perspective.

The field notes from Wheeler (1978) describe her experiences working as an attendant at a large state school for the mentally retarded. The field notes contain, both in substance and linguistic style, the strong intuitive feel of confusion, frustration, resentment and resignation, mixed with sociological detached observation. As the notes were transformed into a report, the topical organization moved the impression further along the continuum toward "outsider's report from insider information." However, the report still seemed to describe from the attendant's perspective, interpreting incidents as they would and focusing on those issues that attendants felt were important, for the reasons they felt they were important. The final section of the report placed the events and experiences within a larger societal context of job structure, social change, economics, and ideology. While it might be argued that this analysis was sympathetic or even consistent with the attendants' perspective, it nonetheless contained terms, explanations, and contextual descriptions that were not the ways in which they would describe themselves. Where the attendants tended to see problems arising from within the administration (its incompetency, insenitivity, personal idiosyncracies, etc.), the report tended to place the whole relationship within a social structural context. And in the end this larger sociological perspective consumed all others as the dominating perspective or explanation.

This description of the Wheeler report is not a criticism. In fact, these three rather distinct points of view may be considered the mark of competent participant observation, maintaining the balance between "going native" and completely detached insensitivity. The point is, if she had assumed only the attendants' point of view, then her descriptions and explanations would have been constrained within that perspective. By alternatively assuming an analysis of attendants within a sociological perspective, other explanations and perspectives were raised. Neither the attendant nor the sociological description is more correct; they are two perspectives that describe and explain in quite different ways. Moreover, they are incompatible, and imply different understandings of the attendant experience. It is not possible to hold, at the same time, that the circumstances within the work experience are caused by "incompetent administrators" who are blameworthy *and* that the causes lie in the wider context of a "social structure" that creates and maintains asylums as degrading institutions for all involved. The *researcher must choose or risk explanations incompatable with reason from any point of view.*

Researchers who select and characterize also participate in constraining the dimensions of their conclusions and explanations. Abandoning the professional relationship of highly controlled research does not exorcise the relational quality of empirical research. It simply alters the form of that relationship. Control and compliance may no longer be at issue, but the observer must still grapply with the dilemma of the insider vs outsider relationship that is inherent in the act of selection and characterization. In the end the researcher must make a commitment to a coherent point of view that will be inconsistent with other coherent perspectives and social commitments. It is quite appropriate to use the term commitment, for as we will see later different perspectives help constitute distinct commitments to quite different ways of life.

## Concealment of Intentions and the Denial of Relationship

Concealment and deemphasis of research intentions plays a prominent role in both controlling and non-controlling research strategies. Experimentalists conceal their hypotheses, explicit conceptions of the experimental situation and even the identity of the interventions and measurements. Those who attempt to assume peer-relationships may likewise obscure or deemphasize their perspectives, hypotheses and the nature of their observer role. Whatever the official reasons understood by the researcher, it seems to me that this concealment serves the purpose of obscuring the full meaningfulness of the relationships, and the ultimate power of the researcher as scientist to "name the reality" from his or her own perspective. Concealing or deemphasizing researcher intentions, relationships, and commitments serves to maintain the researcher's legitimacy of control and/or the appearance of participating, accepting and respecting the perspectives of those being researched.

I think this point can be made most readily by considering alternatives to some of the research cited earlier. Consider what this research might have looked like had the interactions with the participants been such that the relationships between researcher and participants was highlighted instead of concealed or deemphasized. In the control and shock avoidance study (Averill et al., 1977) the experimenter might have reminded the subjects that while some would consider that pushing the avoidance button when the 100% effective signal was on would allow them to control and avoid the shock, others might consider the fact that the researcher could, now that they had agreed to participate, change the conditions as he/she wished. I suspect that reorienting subjects in this way might have swelled the number of those who did *not* respond with "instrumental vigilance" to the warning in order to push the avoidance button. Emphasizing another view on the researcher-subject relationship would tend to undermine the initial construal of the situation and the legitimacy of the researcher to construe reality for the subjects.

In the Rubin and Mitchell survey of intimate couples they asked,

> How likely would you say it is that you and ______________ will eventually marry each other [10 point scale, from 0-10% through 91%-100%]. (p. 18)

This was one of those questions discussed by the authors as stimulating discussion and disclosure, and ultimately affecting the relationship within couples. Of course, by asking this question the "experts" endorsed its importance and relevance to couple intimacy. And within that part of the sample that accepted them as legitimate professionals this seemed to have been an issue they had not fully considered but were willing to consider as relevant to defining their personal relationships. Suppose the researchers had posed the question within another framework: 'Many noted psychologists have concluded that discussions of the probability of marriage should be *avoided* in intimate relationships except when or if one member wishes to propose in definite terms. Other experts believe it is important to discuss this issue early in any relationship. Given this controversy we invite you to skip the following question, or answer it, as you wish." Such a statement would undermine the legitimacy of this question for anyone who did not already believe it important. Without the full endorsement of the "professional" it seems likely that fewer couples would have been persuaded to consider this an issue of importance worth discussing. And those who chose to answer the question might be quite different from those who chose under such conditions to agree with the "don't push the future" oriented "experts."

Finally, it is clear that Wheeler (1978) could not, while assuming the role of an attendant, persist in describing the circumstances of herself and her co-workers in social structural terms in the face of the real attendants' personal, and characterological explanations. This would have immediately identified her as an outsider, an "intellectual type," "another kid from the college who thinks she knows everything." To so construe the situation would be to deny the attendants' reality of "uppity administrators," "insensitive superintendents," and so on. It would have been, in a very real sense, offensive.

It seems clear that concealing researcher intentions also conceals the implicit relationship of the researcher with the subjects of inquiry. And this is, of course, crucial for an empiricist social science which must only operate in the domain of facts, not agreements, behaviors, not intentions. In this regard it is not only important to conceal researcher intentions and relationship issues from participants who may not wish to hold these as legitimate, it is equally important that the researcher not discover that stable and reliable findings depend upon implicit agreements and shared concepts. So it would be equally problematic if subjects were to mumble during the Averill experiment, "sure, I agree with the experimenter" or "I'll go along and play this little experimental reality." It was in fact perceived as a problem when respondents informed the researchers of how much they had altered their intimate relationships because of the researchers' "expertness" in asking "important" questions. These occurrences are seen as problems because they direct attention to the *participation of scientists and subjects together constructing realities through relationships.*

This would seem an appropriate time to consider the "ultimate" strategy for avoiding the "problem of confounding" researcher-subject relationships with "true effects." Of course, I am referring to the completely impersonal unobtrusive research strategy. The researcher and the participant do not interact face to face. The participant does not know what the researcher's intentions, hypotheses, or measurements involve. Archival research, some forms of completely obscured observation, evaluation research, and secondary reanalyses, fall under this general strategy.

It should be clear, however, that even research detached from face-to-face interaction does not remove the social scientist from his/her relationship with the subjects of inquiry. The researcher's perspective or point of view is still contained in the dimensions of sampling and characterization of people, situations, and measurement indices. And the point of views expressed through these decisions and actions place the researcher in relationship viz-a-viz the self-perceptions and social understanding existant with those being researched and other concerned parties. It is through a point of view, from one relational perspective, that the scientist "names the reality."

The experimentalist, committed to the importance of systematic control and "empirical validity," may select situations that have been characterized as "serendipitous experiments" as an opportunity for unobtrusive research. But then s/he has the same problem as in any situation of control. The participants in such a situation tacitly or explicitly accept the intervention as "inescapable" or "legitimate" within the bounds of "reasonableness." And the observer must accept their concepts of social reality or choose to characterize them within some other alternative social conception. The Sherif et al. (1961) Robbers Cave Experiment might be considered unobtrusive research. The boys camp was a real situation, natural enough, and the researchers interacted through camp personnel or in the convincing role of camp counselors. The conclusions from this study of cooperation and competition are nonetheless constrained by the "legitimacy of camp personnel to *unilaterally* create and define situations, segregate campers, and define "tasks" and "crises." This research would seem to allow generalizations to other populations of persons who, like these children, are willing to accept such a relationship as legitimate. The institution of the Draft Lottery in the early 1970s would seem to have been a terrific opportunity to examine some issues of high and low control, with random assignment of young men to the drafted low control or unselected high control group. But of course such a characterization depends on the accepted "legitimacy" of the lottery itself. Canada and federal prisons were full of "error variance" and "non-responders" in this regard. And conclusion would necessarily rest on one's point of view about the concept of "control." Is "allowed" control, being unselected, the same as "seized" control, choosing to refuse the draft. The concepts mentioned above are moral and normative and suggest different points of view and ways of life. Given perspectives shared by a group of people constrain their options, opportunities and the dimensions of common sense. Perspectives chosen by researchers likewise define their commitments and their understanding of "systematic" pattern, and "random variability."

Concealment, detachment or unobtrusiveness may remove the researcher from personal interaction, but it does not remove him or her from standing in some relationship with those being studied. In the end the crucial issues of this relationship involve naming the reality, and the constitution of a way of life. The actions and decisions taken by any researcher are actions within the framework of descriptions and characterizations from some point of view, and face-to-face relationships that are constituted by legitimacy within a way of life. Social science may not then gather the social facts. Social scientists participate in either viewing or re-viewing social reality, and in so doing make themselves useful within distinct points of view and social commitments. That is to say, our actions and decisions as researchers not only constrain the empirical conclusions but also help define the way of life, social realities and commitments within which the research may become useful.

## Relationships, Common Sense and Usefulness

The relationships, dimensions of concepts and common sense, and the construal of social reality that underly social research constrain the empirical conclusions and at the same time help define the uses to which this research may be put. The shared concepts of legitimacy and common sense between researchers and respondents allows systematic patterns of behavior to be observed. At the same time, these findings are useful, informative and reasonable only within a way of life that shares a similar conception of legitimacy, common sense, and social reality.

Walton (1975) describes a highly successful project to create a "new plant culture" in a General Foods pet food plant using principles and theories largely borrowed from social psychology. The project originated from consultations with management concerned with the problem of "employee alienation."

> There were severe symptoms of employee alienation at the existing pet food factory in Kankakee. Employee indifference and inattention while manning the continuous-process technology led to plant shutdowns, product waste, and costly recycling. Numerous acts of sabotage and violence were both costly for the business and disintegrating for the plant society. We wanted to design a plant where these attitudes states would not occur. Thus, we wanted to avoid features of the existing plant which promoted alienation. (p. 140)

The stated intent and theory of the "innovative" work design was to reduce alienation and increase the workers' sense of participation, involvement, commitment and responsibility in the work environment. This was accomplished, according to Walton, through the design of the employees' relationship with tasks, fellow workers, and the management. Changing the relationships and responsbilities was seen as the major cause of the "impressive" improvement in productivity and "quality of work life." Employees worked on whole tasks in small groups rather than working on repetitive single tasks on an assembly line. Assignments of individuals to sets of tasks were subject to decisions within work teams. Status differentiation among employees was minimized, and dull and repetitive jobs were automated or subcontracted. Visible signs of status differences between employees and management such as separate parking lots, separate building entrances and differences in decor were eliminated. There were no time clocks. As many decisions as possible were allowed to be made at the lowest feasible level. Pay increases were geared toward rewarding employees for mastering an increasing number of the essential production tasks.

A somewhat different reading of Walton's description suggests an alternative interpretation of the intervention and some noteworthy parallels between the process of "changing" the plant culture and the process of "discovery" in highly controlled research. The key elements of similarity reside in the common and essential relationship of professionalism: power and compliance, legitimacy, and the sharing of perspectives. Although it is not highlighted in his report, Walton's strategy for creating a more productive and "less alienating" plant culture can be seen as relying heavily on (1) selecting a population of workers with a *compatible perspective* on self-fulfillment, control, and power, (2) *concealing* and de-emphasizing *alternative points of view.*

Walton does note that employees were highly selected and that this was important to the overall structure and success of the project.

> We projected evolving expectations of American workers which would increasingly come in conflict with the demands, conditions, and rewards of conventional organizations. For example, expectations were rising with respect to challenge and personal growth, patterns of mutual influence and egalitarian treatment, and openness in interpersonal relations . . . thus we set out to incorporate in the new plant features that would be responsive to evolving employee expectations and would provide a high quality of work life, enlist unusual human involvement, and result in high productivity. (p. 140)

> . . . we used an extensive recruiting procedure which encouraged the screening and self-selection processes to select employees more likely to respond positively to the organization we had designed. (p. 141)

> Therefore, an early step was for team members to visit sites where there were ongoing experiments with non-conventional plant organizations and talk with supervisory and worker participants. (pp. 148-149)

> . . . the persons selected into the system were judged to be relatively amenable to the re-education we had in mind. (p. 154)

The prospective employees were selected then for those who would come to understand the new organizational structure, and its potential for "personal growth" and "mutual influence" from the same point of view as the designer and the management. That is, workers were selected who would define the context and dimension of *"influence"* within small (7-17) worker groups. Workers were selected who understood common parking lots and uniform decor as "egalitarianness" (sic) in the work setting.

This perspective on influence and control is roughly correspondent to the concept of control assumed by the experimenter and most subjects in the study by Averill et al. (1977). Control and influencability was defined within the structure of immediate tasks (ability to decide which of five tasks to do, probability of avoiding shock). As in the experiment some latitude of doubt about this perspective could be overwhelmed through the power of the relationship. For the employer this power resided in their power to legitimately grant or withhold a job. For the experimenter the mantle of science lent credibility where sensibility was not immediately apparent.

There are, of course, other perspectives on "control," "influence" and "self-fulfillment" which represent incompatible points of view vis a vis the "innovative" plant organization. As in the experiment a perspective on personal control that focuses on *the process of achieving control in a larger context* rather than simply the proximal outcomes would not be compatible within this work setting. What was "error variance" or an "inconsistent responder" in the experiment becomes an employee with a "bad attitude" within the structure of this plant organization. The subject focuses on the power of the experimenter to manipulate his/her experience in the experiment; the "bad" employee focuses on the power that still resides with the management. The fact that they now carry a black lunch box, just like his, is not interpreted as a consequential sign of "egalitarianness" or worker control within this perspective.

The management of course wished to achieve not just "significantly different" results; they wanted to increase productivity and enlarge their profits. They could not simply rely on random sampling to select a group that was predominantly consistent with their perspective. So they carefully selected. Moreover, the plant was not a short-term experiment; it was a long-term enterprise, and they needed to insure that the necessary agreements in perspective that supported the higher productivity persisted. To make matters worse there is a highly organized body of workers that espouse an alternative point of view. Labor unions are notoriously hostile to the perspective that control is defined by "allowed control" and "management benevolence." The answer to this problem in the Walton project appears only once, buried in a nine-column table under "Favorable Business Conditions" necessary for success.

> No power groups will exist within the organization that create an anti-management posture. (p. 152)

It is perhaps not surprising that research conducted within the relationship of legitimate professionalism, control and compliance, and shared perspective contains some striking resemblance to other conerns in the wider society. In both cases the interests of the controllers are best served by manipulating implicit agreements about social concepts. The scientist can maintain the illusion of "accruing facts" detached from human relationships. The businessman can maintain control and power by operating within a social reality that conceals alternatives to the status quo. Stability and continuity in business practices and the body of knowledge both depend on *the understanding that these social agreements are the "nature of things."*

The congruity between current conceptions of social science and the range of usefulness is not limited to circumstances that are so clearly related to issues of control. In recent years Jacobo Varela has gained some notoriety through his applications of social psychological theory to "social problems." Appropriately enough, he describes his methods of application as "Social Technology" (1977, 1975, 1971).

Varela is very explicit about the fact that his concept of social intervention is directly tied to and an extension of the experimental paradigm.

> It has often been remarked that one problem with the social sciences that they have tried to follow the physical sciences too slavishly. My contention is that they have not followed them enough. (p. 915, 1977)

To follow the physcial sciences more slavishly is to strive for power over "variables," controlled manipulation of procedures, and the achievement of predictable behavior. Indeed, "social technology" seems an apt characterization for the extension of this approach to applied social issues.

The social technological approach is illustrated easily through the case study of a young woman who turned to alcohol and marijuana after her rejection by an art school admissions committee.

> Beatrice (Rosa's friend) easily diagnosed Rosa's abuse of alcohol and drugs as coinciding with a curt rejection note she received to her application for eagerly sought admission to an art school. The shock of rejection was interpreted by Rosa as a rejection of her artistic values and abilities. (p. 916)

Varela goes on to describe Rosa's difficulties in terms of an approach-avoidance problem concerning the desire to pursue art and the fear of further rejection. He chose to assist Beatrice in an attitude change intervention to return Rosa to her previous involvement in art. This was expected to conflict with and reduce her abusive indulgence in alcohol and drugs. The intervention carried out by Varela and Beatrice was described in terms of Sherif and Hovland (1961) latitude of rejection scales, Likert ratings (1932), cognitive dissonance (Festinger, 1957) and reactance theory (Brehm, 1966).

The manipulation involved Beatrice engaging Rosa in an orchestrated conversation (unknown to Rosa, of course) using various verbal ploys to induce her to assert increasingly positive statements consistent with resuming her art work. By first eliciting verbal commitment to the least negatively valenced statement (rated by Beatrice as something Rosa would not initially endorse) and moving slowly towards statements more consonant with resuming art work, Rosa would change her attitude and the intervention would succeed. Beatrice's rating of statements which Rosa would and would not initially agree with and the progression of orchestrated conversation are listed below.

| Statement Number | Likert Attitude Rating | Statement |
|---|---|---|
| 1 | +8 | I've always loved art. |
| 2 | -1 | I did right in not following my mother's advice when she disapproved of my painting. |
| 3 | -2 | There are always obstacles in the lives of artists. |
| 4 | -3 | Artists should not necessarily follow the dictates of the critics. |
| 5 | -4 | Art school staff do not have the time nor are they infallible in judging applicant portfolios. |
| 6 | -5 | East Art School's opinion is just one more negative opinion that most artists face. |
| 7 | -6 | I think I'll reapply for next term. |
| 8 | -7 | I'll send them some of my new art. |
| 9 | -8 | Give me materials. I'm starting to paint now. (Varela, 1977, p. 917) |

According to Varela's report, Beatrice was highly successful in manipulating Rosa to make these successive assertions and in encouraging Rosa to resume her art work.

Quoting Beatrice,

> She worked all afternoon, thoroughly immersed in the feelings of the beings in her drawings. More and more of the old enthusiasm returned. At one point she looked at herself surrounded by the materials and new creations and said: "This isn't what we had planned for the afternoon. What did you do to me?" I

> said: "Rosa I didn't do a thing. You did it yourself." (p. 918)

And Varela describes the success of his "technological" intervention:

> It is important to note that the attitude change design worked because at heart Rosa wanted to get back to art but couldn't on her own. Three weeks later, Beatrice reported that Rosa had returned to her art classes, was preparing another application for school, had finished seven drawings, gone on two photography trips, and was preparing a drawing for an etching for her etching class. What is more important, however, is that she had virtually abandoned alcohol and pot except for social occasions. (p. 918)

Indeed, Varela did recreate the essential elements of the experimental and controlled methodology in this applied situation. All the elements seem to be present. Beatrice, a trusted friend and *legitimately powerful person* with Rosa, clearly and accurately assessed Rosa's social reality and perspective on her art school rejection. The *underlying perspective shared by Beatrice and Rosa* comes out clearest in the scaled Likert statements. Despite some intermediate protestations to the contrary (statements 4, 5, 6 and even those equivocate) the bottom-line reality for Rosa was that the opinion and endorsement of the art school was an important indication of her competence as an artist and her personal worthiness. Statements 7, 8, and 9 reassert that the important issue is to "get back to work" so next time with "new art work" she will be "up to standards." Varela reaffirms this perspective by the importance he places on reapplication as an index of success with Rosa.

But, of course, there are alternative perspectives that could be taken. There are other resolutions, from other points of view, that could allow Rosa to return to her loved art work, discontinue her abuse of drugs, and resolve her self-doubt: refocusing on her personal satisfaction gained from artistic activities, a rejection of the legitimacy of the school's opinions, disconnecting others' opinions of her artistic talents from her assessment of self-worth, and so on. But, of course, in these cases, involving more fundamental changes, the social psychological literature is not so informative and the implicit goal of rapid return to the status quo is violated. These alternatives remained invisible partly because they were concealed behind the tacit agreements about social reality, and the belief that the way things are is the way things must be, all this shared by Varela, Beatrice and Rosa.

It is clear that fundamental tacit agreements play an important role within relationships of control and compliance. In the experiment, the factory, and the intimate relationship agreements about the legitimacy of the power-compliance relationship (expert, financial, affectual) and the commonsensical nature of shared points of view helped conceal any fundamental alternatives. But it is also true that fundamental commitments to a social perspective operate outside of power-compliance relationships, even outside of face to face interactions altogether. As discussed earlier (Chapter II) commitments to a point of view may become invisible by virtue of their very ubiquitousness. Our socially constructed concepts become the "nature of things" because they help constitute our personal lives and make sense of the wider social milieu. Be that as it may, these perspectives are elements of social commitment and they do constrain the ways in which we understand the world. It is because we hold these perspectives as internalized reality that they may become invisible. And it is this fact that underlies the actions and decisions of ethnological and participant observation techniques, and the acceptance-peer relationships they attempt to assume. Whether an observer accepts the "reality" of those observed or subsumes this reality within a wider contextual analysis the empirical conclusions will be constrained by the inherent researcher decisions and perspectives. And whatever relationship is assumed, the point of view will constrain the real world actions and uses that are reasonable within it.

The bottom line of any description and characterization can be understood as "naming the reality." And some current literature offers very dramatic illustrations of the ways described realities fit within some commitments and not others, within some ways of life but not others. Ian Lubec (1977a, 1977b) offers an example of alternative conceptions of "aggression" and their imbeddedness in contrasting world views. The central themes revolve around the criteria and dimensions of human action that define violence and aggression and the interpretive values placed on these acts. It is clear for example that most social psychological research and common conceptions of violence are understood within an interpersonal and dyadic context. Violent acts include hitting, shooting, shocking, etc. Partly because of some methodological traditions and partly because of widely held implicit understandings, other activities that might be considered aggressive and violent are not easily included within theoretical and empirical frameworks: polluting, job termination, discriminatory hiring and firing, I.Q. testing, deameaning media characterization of ethnic groups, etc. It is clear, however, that within another framework these less direct, but nonetheless consequential, actions could be considered acts of extreme violence and aggression. Then, of course, if one assumes this point of view a number of other concepts must be readjusted. What is the evaluation now of sabotage by individuals who have been "aggressed" against? What is the punishment for discriminatory hiring which relegates minorities to positions of joblessness and poverty? Should capital punishment be considered as an appropriate punishment for premeditated pollution, and so on?

Lubec also points out that when violence and aggression are understood within a dyadic perspective and the definition is restricted to immediate and direct actions it seems more reasonable to place a negative value on all such acts. This negative valuation and a focus on reducing aggression has characterized most theorizing in social psychology. However, there are other perspectives. The largely ignored work of Sorel (1908) took a wider view on the definition of aggression and consequently placed a different value on some violent actions. To the point, Sorel considered violence on the part of oppressed groups to be both natural and understandable in the face of long suffered conditions and preceding acts of violence perpetrated by ruling classes.

> Sorel's approach held that violence was functional, good, a normal part of the socio-political process of antagonistic class conflict, and should be encouraged in heroic, apocalyptic struggles. (Lubec, 1977b, p. 1)

The point here, of course, is not to endorse one point of view or the other. Rather, it is most important to recognize that these two perspectives on aggression and violence, (1) represent two different and incompatible perspectives, (2) each perspective constrains the holders' understanding of the social world, (3) each understanding of the social world carries within it implied actions and activities with real world consequences, and (4) in so far as one is committed to a perspective s/he is also committed to a way of life.

Other commentaries illustrate these same points considering other phenomena. Ryan's (1972) discussion of the implicit perspective of "Blaming the Victim" traced the relationship of "person centered" explanations to social programs that maintain poverty while appearing to support reform. Friere (1970) discussed the ways in which the oppressed poor have been led to maintain their positions as underdogs by their tacit acceptance of the wider society's concepts and explanations of their condition. A recent review of the literature on Black and white doll preference among Black children (Banks, 1976) supported conclusions that suggested "white child ethnocentrism" as a reasonable reinterpretation of data that had been used to argue that Black children maintained a "poor self image." The widely held interpretation was part of the support for the degrading concept of Black children as living in "cultural deprivation." Lobov (1969) studying the verbal abilities of Black ghetto children made a similar point from researching speech patterns within a different empirical relationship.

It is not necessary at this point to continue the chronicling of research relationships and perspectives that help constitute empirical conclusions. The point I wish to make here is a more general one. Within our empirical techniques, the activities of inquiry, are contained the perspectives that constrain conclusion and interpretations. And these constraints and commitments are also part of our commitments to a way of life and a view of social reality. Most important perhaps, is the understanding that these commitments and their implications *cannot* be reduced to "empirical questions." For example, in the case of "concepts of control" one perspective is "better" than the other depending on one's commitments to a way of life. The men and women in the factory, understanding control within a very local framework, were reported to be very pleased with the quality of their work life. To adopt a point of view that understood themselves as powerless vis a vis the management would involve personal costs and benefits and could only be weighed according to values and commitments about how they wished to live. They could lose their jobs; but they could also gain a fellowship with a larger group of organized workers. They could lose an opportunity to work in pleasant surroundings under conditions of "powerlessness" on the chance, in the long run, of establishing truly "egalitarion" relationships of worker and management negotiated work conditions. These are conflicts about what one values, how one wants to live, and commitments to the integrity of world views. These are not empirical questions. Clearly, a similar analysis is possible for Rosa and others.

Social scientists have become so absorbed in the pursuit of "facts," "validity" and the use of "methodological tools" of discovery that they have lost sight of their participations in a social reality. This understanding of the enterprise has obscured the ways in which particular perspectives have been reified and conserved as if they were the nature of things. This conservatism is reflected in the potential use of "scientific" methods and theories in applied settings. Coming to an understanding of the ways in which a social scientist must necessarily participate in a set of perspectives and commitments within the context of empirical relationships casts the social scientific enterprise in an entirely different light.

If it is considered essential that researchers report sampling procedures, sample sizes, statistical analyses, measurement procedure and so on, then it would seem at least equally important that scientists attempt to explicate the dimensions of social perspectives that also define and constrain the meaningfulness of their data. "Rigorous," "legitimate," and "competent" science then could be redefined to require this form of self-reflection and self-consciousness. I am speaking, of course, of social scientists and the social scientific enterprise in general taking responsibility for actions and decisions that reflect social commitments.

I have argued for a reorientation, turning from the aspirations of empiricism, facts, and validity to the pursuit of self-consciousness and good faith in social scientific inquiry. In so doing it is clear that I have dwelt more on anomalies, problems, and contradictions than I have on solutions. And indeed, the ideal of pursuing self-consciousness in social science does raise more problems and questions than it does ready solutions. Fundamental assumptions and perspectives tend to be difficult to bring into awareness. They exist like the air; it is everywhere and therefore invisible. Or our perspectives and social realities are so private that they never see the light of day and therefore do not encounter existent contradictions, with their potential for raising consciousness about alternatives. I will not resolve these dilemmas. I can only suggest some activities of inquiry that would seem consistent with this new empirical enterprise. In the next Chapter I will discuss the relationship of self-consciousness to the concept of good faith in social science.

# CHAPTER V

# GOOD FAITH SOCIAL SCIENCE: FROM PRIESTHOOD TO POLITICS

In the preceding chapters I have considered empirical methods as researcher actions and researcher shaped relationships with the subjects of inquiry. I have argued that such a view of empirical methods refocuses attention on the ways that empirical conclusions are constrained by the perspectives of the researcher and the implicit agreements within empirical relationships. Given this analysis I would like to suggest the beginnings of an enterprise for the social sciences that can transcend the limitations of empiricism.

The mechanical rules of "objective science" have obscured the participatory role of scientists in their relationships with those studied and the society that might put their theories and interpretations to practical use. The alternative to this orientation focuses not on "facts" but "perspectives" and replaces the aspirations of "validity" with the goals of "social responsibility." While I would not pretend to define an alternative science it is possible to suggest the outlines of this alternative. A participatory science can no longer stand aloof as the priesthood of objectivity. It must face the challenges of participation in good faith. And in so far as the coins of controversy are the commitments to ways of life and human values, participation will necessarily take the form of politics.

## Good Faith Social Science

The relational qualities of social inquiry have been obscured by the long standing focus on empirical methods as "tools" for establishing "social facts." Empirical strategies have been applied and understood as independent of the researcher and the researched. And this view has allowed the socially legitimated relationships and implicit agreements that support the researcher-participant interactions to remain invisible constituents of scientific results, conclusions and interpretations. These social relationships, based on the currency of legitimate agreements and invisible alternatives, have been reified as truths and basic knowledge. Social science has not come to grips with the inherently participatory nature of social reality and its constitutive empirical regularities. Because social scientists have not recognized their own participation, because social facts have been construed as separate and detached, empirical social inquiry has tended to sanctify the status quo, to conserve and anoint existant social reality as the "natural order."

Many of the concepts addressed by social science are essentially contestable. However, to the extent that normative social concepts are understood as social facts they may remain uncontested. Concepts of "control and personal influence," "aggression and violence," "mental health," "sex roles," "social power," and "compliance" may be socially defined and understood in a variety of ways. To the extent that social scientists focus on the behavioral regularities of existent conceptions and reify these empirical regularities as social facts, then they participate in a set of conservative commitments.

The behavioral regularities of subjects under varying conditions of "potential control" as they emerged from the relationships within the Averill et al. study (1977), and the "successful" restructuring of a plant society by Walton and his managerial colleagues do not reflect the facts and applications of "control" and behavioral response, but rather the limits of instrumental action within existent social concepts. Those with alternative understandings of control and personal influence that did not fit within the conceptual status quo were viewed as "inconsistent responders" and "employees with bad attitudes," respectively. Empirical research that defined aggression and violence in interpersonal and dyadic terms finds behavioral regularities among the majority of participants who share this view. More detached and indirect forms of aggression remain uninvestigated and excluded from the currently held concepts of violence. Those who recognize the "reality" of remote aggression and respond with counter-aggression are understood in normative terms as "deviants," "abnormal," and within the current conceptions, "irrational." The forms of rational and normative real world responses within this view are obvious; they are equally obviously defined by the normative legitimate reality, not by the "facts."

Social scientific inquiry does not have access to the "facts," only to various social conceptions and their constuitive actions and behaviors. Because this has not been well understood the interpretations of empirical inquiry have been limited by the study of random samples and the normative realities shared by the majority of those sampled. Perhaps unwittingly, this form of research, with its dependence on criteria of validity associated with "means and central tendencies," has tended to identify and theorize within the domain of a normative conceptual reality. If a social science of facts is impossible, an "unwitting" social science is certainly unacceptable. Any alternative must include some understanding of its participation and implication within the social commitments that are also the subject of inquiry. A legitimate social science must pursue self-consciousness and explicitness about these commitments that help constrain empirical results and interpretations. The pursuit of such self-consciousness can be the foundation of a social science in Good Faith.

The central aspirations of a good faith social science revolves around issues of self-consciousness. If socially defined concepts help constitute social realities they cannot be bannished from social inquiry. And if social scientists, like everyone else, must view social phenomena from within a point of view then they are necessarily participants as well as observers.

> The difficulty of the enterprise becomes more apparent when I realize that my perspective itself has emerged out of my personal history of social transactions. I share it with selected others with whom I interact, and its structure helps to constitute the fabric of our relationships. These others are the ones I habitually turn to to check the adequacy of my concepts, the plausibility of my beliefs, the propriety of my values. And yet, since our shared perspective has developed out of shared social experiences, my habitual procedure hardly encourages the self-consciousness I need to develop. (Connolly, 1973, p. 27)

Facing the problems and challenges of explicating these social perspectives as they define the constraints, meaningfulness, and uses of empirically derived results and interpretations will be the measure of good faith for social scientific enterprises within a wider societal context.

Self-consciousness will not be fully achieved through simple admonitions to "know thyself," or well intentioned researcher introspection. Awareness of fundamental social agreements and perspectives that constitute commitments to a way of life are revealed through their confrontation with alternative perspectives and commitments.

> By confronting unfamiliar presumptions in opposing theories I am able to render my own tacit views more explicit. By striving to perceive the world from other "angles of vision" I begin to grasp more explicitly the habits of classification I employ. I begin to see that my disagreements with others do not only or always constitute simple disagreements of fact; they also reflect a variable weighting of the "same facts" and subtle differences in the way we slice and organize experience. The suggested approach and promised results of this enterprise are lucidly summarized by Stuart Hampshire:
>
> The habits of self-conscious criticism may modify the habits of behavior. But the habits of criticism are themselves only revised by further criticism and comparison, and by communication with minds that are outside the circle of convention and custom within which he is confined. (Connolly, 1973, p. 27 and Hampshire, 1959, p. 208)

Ian Lubek's (1977a, 1977b) analysis of social psychological approaches to aggression research revealed the limits of the concepts by posing alternative definitions with their own internal rationale and social perspective. Sampson (1977) traced the underlying commitments to "self-sufficient individualism" in American psychological research by posing alternative values that suggested alternate conceptions of "mental health," and "sex roles," and the possibility of other socially legitimate realities. When alternatives such as these are raised they suggest another way of life and a different set of commitments for empirical researchers.

A social science in good faith must embrace criticisms and discourses such as those noted above as an *integral part* of the *empirical enterprise*. Such critiques are not ancillary, nor are they simply interesting historical footnotes; they represent disputes about the meanings and social uses of empirical research and social commitments. Clearly, suggestions that research on "control and social influence" has restricted its conceptions to current normative social understandings constitutes a challenge to the legitimacy of the concept and the legitimacy of the way of life that is constrained by it. Similarly, any analysis of fundamental conceptions and their representations within social research raises the possibility of alternatives and the possibility of other legitimate commitments. Such disputes about legitimacy and social commitment are not empirical disputes in the traditional sense. These disputes are essentially political.

From Priesthood to Politics

The social scientific aspirations of "objective fact" and "basic knowledge" have allowed social scientists to engage in social relations as members of a priesthood. These objectives have been seen as transcendant over other human enterprises, ultimately good for all. This priestly position has fashioned the form and substance of discourse and dispute. When the aspiration is to seek "truth" then the alternative must be seen as "ignorance." "Scientific methods" are understood as the tools for gathering facts and *all* facts are seen as valuable elements of knowledge, part of the "natural order." Base human values and issues of political relevance are thought to reside only in "the uses of knowledge," a matter quite apart from science. Human values that have been seen to invade activities of the priesthood have been addressed as alien bodies from the human realm, to be exorcised from the ritual and the sanctity of the temple. In this way the priesthood of science has established the legitimacy of its activities through a marriage with the ideals and aspirations of the enterprise. And by tying the human activity of social research to the ideals of truth, the disputes and discourse about legitimate activity have been truncated and limited in scope.

It is clear by now that this priesthood, like others, is intimately involved in issues of human value. It is clear that the activities of empirical research are constituted by human perspectives, and commitments to particular ways of life. And the ideals of "truth" and "knowledge" that have exempted these activities from scrutiny by the unanointed nonscientist have only served to conceal their participation in the negotiation of social realities.

In the last chapter, I illustrated the ways in which research relationships, the understanding of social concepts and the potential uses of research findings are mutually constituted. Moreover, the social commitments that are represented within these relationships are essentially contestable. They may represent ways of life that from another point of view are seen to be oppressive or otherwise unacceptable. The contests that must be engaged are not contests about facts; social facts reside *within* a way of life. The contests are about the preferred structure of social relationships themselves. Such disputes are inherently political. Insofar as the social scientific enterprise is implicated within these disputes, participation in good faith will require social scientists to face unsettling questions concerning social responsibility, the limits of legitimate scientific activity, censorship, and political action.

Because social perspectives help constitute ways of life, the commitments within social scientific research are potentially implicated in wider social disputes. It would not seem reasonable to expect labor unionists to show great enthusiasm for social research which conceives of "control and influence" within a definitional framework of "asymetrical legitimate power" and "allowed choices and options." Such an understanding generates information about the manipulability of individuals within this limited conception, instrumental responses that can be elicited under the "illusion of control," and so on. Research of this sort allows consultants such as Walton to describe his factory workplace interventions as "applied social psychology" as the application of "basic knowledge." Many people understand themselves to be victims of "institutional aggression and violence." Such an understanding may allow sabotage, theft, and revolt as reasonable defensive measures against such societal oppression. Why should such persons from the classes of the oppressed support research which defines aggression in individualistic terms suggesting societal interventions to "ameliorate" individual aggression? Within this conception they will be understood as abnormal, sick, maladjusted, and deviant. Attention is focused on their "aggressiveness" outside of the context of their victimization. These disagreements represent political disagreements and political negotiations and solutions are suggested. Such political disputes have already resulted in confrontations concerning Arthur Jensen's work on intelligence and race (Jensen, 1969; Block and Dworkin, 1976) and testing in general. I believe that there are other traditions of research, such as those mentioned above, that are no less politically relevant. They have escaped dispute to the degree that their inherent commitments and particular applications have remained concealed and misapprehended as "basic and applied knowledge."

The political complexities of a social scientific enterprise may be most like those of the news media in a democractic society. The news media also have the problem of reporting from a point of view, of selecting "newsworthy" stories, asking "revealing" questions, using "responsibility" in their choice of stories to run, etc. The perspectives, descriptive terms, and social concepts used by news reporters are also essentially contestable. A comparison of news reporting across papers (*New York Times*, *Rolling Stone*, *Pravda*, *The Real Paper* in Boston, etc.) clearly reveals the difference in perspectives, and commitment to ways of life that are reflected in what stories are considered important and what the "facts" are seen to be. And, of course, the news media are completely enmeshed in the political process; they have been seen as the "watchdogs of government," "responsible informants for society," "tools of Wallstreet," "supporters of an illegal war," and so on. And both the public at large and machinery of government have been engaged in conflict over the appropriate activities and roles of news reporting in a democratic society: can a reporter protect his/her sources; should they be prosecuted for receiving classified information; when may they be sued for liable or false reporting; how does the public respond to "irresponsible" reporting, political bias, etc. Newspersons like scientists claim to report only the "facts." I would suggest that "scientific facts" are subject to the same problems as "newsfacts." Moreover, it would seem reasonable to place "scientific facts" within the same discourse of societal considerations that has constituted our perennial attempts to define the proper relationship of the news media and the democratic process.

When newspaper chains become too large and hold too large a monopoly there is fear of "one-sided reporting" and consideration of forcing the sale of some papers. When newspapers are viewed as "irresponsible" there is discussion of censorship, as in the case of reportage on terrorism, hijackings, etc. The public's confidence waxes and wanes as papers are perceived as biased or unfair (consider the changes in opinions about the

*Washington Post* from the time of the first Watergate stories to the announcement by President Ford that he wished to "restore confidence in government").

I do not wish to characterize the news media in America as having achieved a state of "balanced" reporting and "objectivity" by virtue of its participation in the political process. But I would suggest that the questions that are relevant for the enterprise of generating "newsfacts" are also relevant for the enterprise of generating "science facts." The scientific enterprise too can indulge in "irresponsible" or "unacceptable" activities. Editorial policies can be viewed as ideologically biased and as a threat to "the people's right to know" from some alternative point of view. It becomes legitimate to reconsider the relationships of "basic researchers" with the uses of their research in consulting. To whom is the research useful, and whose interests are served? Should tax money be spent on research which is viewed as perpetuating social concepts contributive to the existing class structure, and so on?

Raising the inherent political issues that constitute the social scientific enterprise is indeed an unsettling business. It raises the specter of the full range of political actions: ideologically informed funding decisions, editorial policies and hiring policies, social ridicule, censorship, etc. These are actions and issues that have traditionally been seen as anathema to the social scientific enterprise. I would only suggest that they have by tradition remained implicit, not absent. The description of politics in science has been undertaken by others, including pressures to conform in research style (especially highly controlled experimentation in social psychology), choice of publication outlet, and fund raising from available granting sources (Lubec, 1977; Bevan, 1970; Greenberg, 1977; and others in Lubec). Social scientists have served the interests of business (Baritz, 1960) and advertising (Ewen, 1976), with more interest in selling than consumer protection, more interest in production than worker rights. By raising the political issues and commitments to the level of explicit discourse, science may run the risk of more extreme forms of dispute and conflict. But then the alternative to conflict must be resignation or obfuscation.

The priesthood of science has created the illusion of detachment in an "ivory tower" of pure knowledge. As social scientists begin to reexamine and debate the social commitments inherent in their research relationships and theoretical formulations this illusion of detachment will dissolve. I have suggested elsewhere that the focus on "methods as tools" has led to the unwitting support of existant social perspectives and the conservation of existant social relationships. This is, of course, not a new idea. Kvale (1973) has discussed the ways in which behaviorism serves the purposes of maintaining relationships within a highly technological and industrial state. Friere (1970) has argued that existant social understandings have helped maintain the poor and oppressed classes in a state of hopelessness and noncompetition with ruling classes. Person-centered explanations (Ryan, 1971) and humanistic psychology (Ratner, 1971) have been viewed as liberal social ideology in the service of existant class structure and the diversion of attention from the possibility of more radical explanations and prescriptions. There have, of course, been others who have made the same point: under the guise of objectivity, social scientific findings have tended to lend support and legitimacy to the conservation of existant social arrangements. And those who have vested interests in such conservatism have found empiricist social science to be a comfortable companion.

It should be clear that raising the level of discourse in social science, questioning the commitments, uses, and perspectives of research relationships and conclusions will inevitably place the discussants in a more adversarial posture vis a vis existant power holders in society. Paulo Friere was exiled from Brazil for his efforts in helping the poor liberate themselves from their docile acceptance of degrading self-definitions and social agreements about the inevitability of their class position. Walton was hired to increase production; he would not have been retained to "raise consciousness" among workers. Social scientists who discuss and legitimize social concepts that might threaten the existant social order should expect to see their stock drop with those invested in the status quo. The empiricists' implicit reification of the social order as the "nature of things" is well suited to those whose power and profits rely on existant, often implicit, social agreements. Self-consciousness about social commitments, essential to a good-faith social science, will suggest alternative arrangements and the possibility of social changes that may alter the distribution of influence and power. This discourse will serve to heat up the public debate, bring the legitimacy of the social scientific enterprise into question, and in general make life a good deal more problematic for social scientists.

I will not presume to answer the serious questions I have raised about the legitimacy of various social scientific concepts and relationships in the research enterprise. That is not to say I have no opinion. However, I do have less faith in moralizing than in the potential of a more moral way of life. That is to say, I would prefer to focus, for the moment, on the broader issues. If social science is to participate in good faith within a society, then it is imperative that we go beyond the discussion of validity and inference to face the real issues and political realities of our enterprise. The discourse on social responsibility, legitimacy, obligation, academic freedom, public access and so on belongs in the political arena, not in the false sanctity of the temple of science.

## Good Faith Social Science and the Activity of Inquiry

I have argued throughout this dissertation that the enterprise of social science should be understood within the fabric of wider social commitments. The research questions that are asked, the relationships within the investigative activity, methodological actions and decisions, the self-images of those in the social scientific enterprise and the dimensions of existant social realities are all *internally* related. That is to say, social scientists and the profession of social science are full participants in the negotiations and renegotiations that help constitute social ways of life. From this point of view the most important issues facing individual scientists and the profession concern the fashioning of our own way of life. I have proposed that the aspirations of social science be turned toward the pursuit of self-consciousness and the open negotiation of social reality. I have proposed a Good Faith Social Science.

This argument has attempted to re-view the activities of inquiry, and in the process arrive at a new understanding of rigorous social science and social responsibility. Within the present perspective these two concepts merge. Rigorous science must attempt to explicate the constituent social commitments that define the meaningfulness of conclusions and interpretations. Moreover, such self-consciousness emerges from open critique and discourse between those who identify with discrepant and conflicting points of view. Consciousness raising occurs when the rationality and self-interests of one perspective are confronted with allegations of irrationality and perhaps illegitamacy from some other point of view. The process by which self-consciousness is pursued also becomes the process of open negotiation and social responsibility. Social responsibility then is no longer understood as the activity of social scientists when they are being "good citizens." Theoretical self-consciousness and social responsibility merge within the concept of rigorous social science and the social scientific way of life.

Postscript

I began this dissertation by referring to a sense of crisis in social psychology and other social scientific endeavors. I have focused attention on the ways in which philosophical empiricism, as it is understood in the social sciences, has alienated researchers from their inherent embeddedness in social life. I have addressed both the crisis and the potentials for resolving this crisis by arguing for the commonality of social science with other social ways of life. For social scientists as well as others, we are at the same time recipients, actors, and inquirers within our existing social reality. No amount of mental or methodological gymnastics will allow us to escape this essential human condition. I have argued that an understanding of the ways in which social scientists share this condition sheds light on the nature of the current crisis of confidence and at the same time suggests the directions for an alternative social scientific enterprise. Empirical researchers must come to grips with the realities of the participant-inquirer role, a role in which rigorous research, theoretical self-consciousness, political conflict, and social responsibility tend to merge into what I have called "good faith social science."

By focusing on the basic issues of the commonality of social science and social life other relevant concerns have not been addressed. If social research may not be independent of the social-historical context in which it occurs, is it also true that social research may not be distinguished from other social enterprises? Does social scientific research contain any *fundamentally* different characteristics that distinguish it from news reporting, literature, or political advocacy? What claims can be made for the character of systematic empirical inquiry? I have not addressed these issues. Reestablishing social science as an inherently social activity loomed increasingly larger and as logically prior to these questions. I must admit that at this point the distinctions between social scientific inquiry, news-reporting, literature, politics, and so on seem relatively trivial to me in the face of their essential commonalities. Be that as it may, I believe that consideration of the possible distinctions [i.e., scientific methods] would appear to be a reasonable pursuit given the groundwork laid in the present discussion. However, if such distinctions are to be made, if social science is to lay claim to special achievements or potentialities, then these claims of distinction must reside within a recognition that social inquiry is a fundamentally human social activity.

# B I B L I O G R A P H Y


Altman, I. *The environment and social behavior.* Monterey, Calif.: Brooks/Cole Publishing Co., 1975.

Argyris, C. Some unintended consequences of rigorous research. *Psychological Bulletin*, 1968, *70*, 185-197.

Armistead, N. (Ed.). *Reconstructing social psychology.* Baltimore, Md.: Penguin Books Ltd., 1974.

Averill, J. R., O'Brien, L., & Dewitt, G. W. The influence of response effectiveness on the preference for warning and on psychophysiological stress reactions. *Journal of Personality*, 1977, *45*(3), 395-418.

Banks, C. W. White preference in Blacks: A paradigm in search of a phenomenon. *Psychological Bulletin*, 1976, *83*(6), 1179-1186.

Baritz, L. *Servants of power: A history of the social sciences in American industry.* Middletown, Conn.: Greenwood, 1960.

Barker, R. G. Explorations in ecological psychology. *American Psychologist*, 1965, *20*, 1-14.

Barker, R. G. *Ecological psychology: Concepts and methods for studying the environment of human behavior.* Palo Alto: Stanford University Press, 1968.

Barker, R. G. Wanted: A eco-behavioral science. In E. P. Willems and H. L. Raush (Eds.), *Naturalistic viewpoints in psychological research.* New York: Holt, Rinehart and Winston, 1969.

Baron, R. M. Role playing and experimental research: The identification of appropriate domains of power. *Personality and Social Psychology Bulletin*, 1977, *3* (3), 505-513.

Baron, R. M., Mandel, D. R., Adams, C. A., & Griffen, L. M. Effects of social density in university residential environments. *Journal of Personality and Social Psychology*, 1976, *34*, 434-446.

Bass, B. M. The substance and the shadow. *American Psychologist*, 1974, *29*(2), 870-886.

Becker, H. S. Problems of inference and proof in participant observation. *Smerican Sociological Review*, 1968, *23*, 652-660.

Becker, H. S. *Outsiders: Studies in the sociology of deviance.* New York: Macmillan (Free Press), 1963.

Becker, H. S. Problems of inference and proof in participant observation. In G. J. McCall and J. L. Simmons (Eds.), *Issues in participant observation: A text and reader.* Reading, Mass.: Addison-Wesley, 1969.

Berger, P. L., & Luckmann, T. *The social construction of reality: A treatise in the sociology of knowledge.* New York: Doubleday, 1967.

Bevan, W. Psychology, the university and the real world around us. *American Psychologist*, 1970, *25*, 442-449.

Block, N. J., & Dworkin, G. (Eds.). *The IQ controversey.* New York: Pantheon Books, 1976.

Blumer, H. *Symbolic interactionism.* Englewood Cliffs, N.J.: Prentice-Hall, 1969.

Brandt, R. M. *Studying behavior in natural settings.* New York: Holt, Rinehart and Winston, 1972.

Brehm, J. W. *A Theory of psychological reactance.* New York: Academic Press, 1966.

Bronfenbrenner, U. *Experimental human ecology: A reorientation to theory and research in socialization.* Presidential Address to Division of Personality and Social Psychology, 82nd Annual Convention of the American Psychological Association, 1974.

Bronfenbrenner, U. Toward an experimental ecology of human development. *American Psychologist*, 1977, *32*(7), 513-531.

Brunswik, E. Representative design and probabilistic theory in a functional psychology. *Psychological Review*, 1955, *62*(3), 193-217.

Buss, A. R. The emerging field of the sociology of psychological knowledge. *American Psychologist*, 1975, *30*, 988-1002.

Byrne, D. Attitudes and attraction. In L. Berkowitz (Ed.), *Advances in experimental social psychology*, Vol. 4, New York: Academic Press, 1969.

Campbell, D. T. Reforms as experiments. *American Psychologist*, 1969, *24*, 409-429.

Campbell, D. T. *Qualitative knowing in action research.* Kurt Lewin Award Address, Society for the Psychological Study of Social Issues, meeting held with the American Psychological Association, New Orleans, 1974.

Campbell, D. T., & Stanley, J. C. *Experimental and quasi-experimental designs for research*, Chicago: Rand McNally, 1963.

Collins, B. E., & Raven, B. H. Group structure: Attraction, coalitions, communication and power. In G. Lindzey and E. Aronson (Eds.), *The handbook of social psychology*, Vol. 4, Reading, Mass.: Addison-Wesley, 1969.

Connolly, W. E. Theoretical self-consciousness. *Polity*, Fall 1973.

Connolly, W. E. *The terms of political discourse.* Lexington, Mass.: D.C. Heath and Company, 1974.

Connolly, W. E. Appearance and reality on politics. Unpublished paper, University of Massachusetts, 1977.

Connolly, W. E. Personal communications, Fall 1977.

Cook, T. D., & Campbell, D. T. The design and conduct of quasi-experiments and true experiments in field settings. In M. D. Dunnette (Ed.), *Handbook of industrial and organizational research.* Chicago: Rand McNally, 1975.

Crano, W. D., & Brewer, M. B. *Principles of research in social psychology.* New York: McGraw-Hill, 1973.

Cronbach, L. J. Beyond the two disciplines of scientific psychology. *American Psychologist*, 1975, *30*(2), 116-127.

Dean, J. P., Eichorn, R. L., & Dean, L. R. Limitations and advantages of unstructured methods. In G. T. McCall and J. L. Simmons (Eds.), *Issues in participant observation.* Reading, Mass.: Addison-Wesley, 1969.

DiRenzo, G. (Ed.). *Concepts, theory, and explanation in the behavioral sciences.* New York: Random House, 1966.

Elms, A. C. The crisis of confidence in social psychology. *American Psychologist*, 1975, *30*(10), 967-975.

Epstein, Y. M., Suedfeld, P., & Silverstein, S. J. The experimental contract: Subjects' expectations of and reactions to some behaviors of experimenters. *American Psychologist*, 1973, *28*, 212-221.

Ewen, S. *Captains of consciousness: Advertising and the social roots of the consumer culture.* New York: McGraw-Hill, 1976.

Feldman, C. F., Hass, H. W., & Wilbur, A. Controls, conceptualization, and the interrelationship between experimental and correlational research. *American Psychologist*, 1970, *25*, 633-635.

Festinger, L. *A theory of cognitive dissonance.* Stanford, Calif.: Stanford University Press, 1957.

Festinger, L., & Katz, D. (Eds.). *Research methods in the behavioral sciences.* New York: Dryden Press, 1953.

Foreward, J., Canter, R., & Kirsch, N. Role-enactment and deception methodologies: Alternative paradigms? *American Psychologist*, 1976, *31*(8), 595-604.

Fredericksen, N. Toward a taxonomy of situations. *American Psychologist*, 1972, *27*, 114-123.

French, J. R., & Raven, B. H. The bases of social power. In D. Cartwright (Ed.), *Studies in social power.* Ann Arbor: University of Michigan, Institute for Social Research, 1959.

Friere, P. *Pedagogy of the oppressed.* Translated by M. B. Ramos. New York: Seabury Press, 1970.

Gadlin, H. Private lives and public order: A critical review of the history of intimate relations in the United States. In G. K. Levinger and H. L. Raush (Eds.), *Close relationships: perspectives on the meaning of intimacy.* Amherst, Mass.: University of Massachusetts Press, 1977.

Gadlin, H., & Ingle, G. Through the one-way mirror: The limits of experimental self-reflection. *American Psychologist*, 1975, *30*, 1003-1010.

Gallie, W. B. Essentially contested concepts. In M. Black (Ed.), *The importance of language.* Englewood Cliffs, N.J.: Prentice-Hall, 1962.

Gergen, K. J. Social psychology as history. *Journal of Personality and Social Psychology,* 1973, *26,* 309-320.

Gergen, K. J. Experimentation in social psychology: A reappraisal. Unpublished paper, Swarthmore College, 1975.

Gergen, K. J. Social psychology, science and history. *Personality and Social Psychology Bulletin,* 1976, *2* (4), 373-383.

Gold, R. L. Roles in sociological field observations. In G. J. McCall and J. L. Simmons (Eds.), *Issues in participant observation: A text and reader.* Reading, Mass.: Addison-Wesley, 1969.

Greenberg, D. S. *The politics of pure science.* New York: New American Library, 1971.

Gump, P. V., & Kounin, J. S. Issues raised by ecological and classical research efforts. *Merrill Palmer Quarterly,* 1960, *6* (3), 145-152.

Hampshire, S. *Thought and action.* London: Chatto and Windus, 1970.

Harré, H., & Secord, P. F. *The explanation of social behavior.* Totowa, N.J.: Littlefield, Adams & Co., 1972.

Hendricks, C. Social psychology and history: An analysis of the defense of traditional science. Unpublished paper, Kent State University, 1974.

Higbee, K. L.,& Wells, M. G. Some research trends in social psychology during the 1960s. *American Psychologist,* 1978, *27,* 963-166.

Hutt, S. J., & Hutt, C. *Direct observation and measurement of behavior.* Springfield, Ill.: Charles C. Thomas, Publisher, 1970.

Ingle, G. Bringing social psychology up to data: E and S have a relationship. Unpublished paper, University of Massachusetts, 1977.

Jensen, A. R. How much can we boost I.Q. and scholastic achievement? *Harvard Educational Review,* Winter 1969.

Jones, E. E., & Gerard, H. B. *Foundations of social psychology.* New York: Wiley and Sons, 1969.

Kaplan, A. *The conduct of inquiry: Methodology for behavioral science.* Scranton, Penn.: Chandler Publishing Co., 1964.

Kendon, A., & Ferber, A. A description of some human greetings. In J. H. Crook and R. P. Michael (Eds.), *Comparative ecology and behavior of primates*, New York: Academic Press, 1973.

Kerlinger, F. N. *Foundations of behavioral research: Educational and psychological inquiry.* New York: Holt, Rinehart and Winston, 1964.

Kirkpatrick, C., & Stone, S. Attitude measurement and the comparison of generations. *Journal of Applied Psychology*, 1935, *19*, 564-582.

Knight, R. C., Zimring, C. M., & Kent, M. J. Normalization as a social-physical system. In M. J. Bednar (Ed.), *Barrier-free environments.* Stroudsburg, Penn.: Dowden, Hutchinson and Ross, 1977.

Koch, S. (Ed.). *Psychology: A study of a science.* Vols. 1-3. New York: McGraw-Hill, 1959.

Koch, S. (Ed.). *Psychology: A study of a science.* Vol. 4. New York: McGraw-Hill, 1962.

Koch, S. (Ed.). *Psychology: A study of a science.* Vols. 5-6. New York: McGraw-Hill, 1963.

Koch, S. *Vagrant confessions of an asystematic psychologist: An intellectual autobiography.* Paper presented at the meeting of the American Psychological Association, San Francisco, 1977.

Kovesi, J. *Moral notions.* London: Routledge and Kegan Paul, 1967.

Krech, D., Crutchfield, R. S., & Ballackey, E. L. *Individual in society.* New York: McGraw-Hill, 1962.

Kuhn, T. S. *The structure of scientific revolutions.* Chicago: University of Chicago Press, 1962.

Kvale, S. The technological paradigm of psychological research. *Journal of Phenomenological Psychology*, 1973, *5*, 143-159.

Lichtman, R. Symbolic interactionism and social reality: Some marxist queries. *Berkeley Journal of Sociology*, 1970, *15*, 75-94.

Likert, R. A technique for the measure of attitudes. *Archives of psychology*, 1932, No. 140.

Lobov, W. The logic of nonstandard English. *Georgetown Monographs on Language and Linguistics*, 1969, *Vol. 22*.

Lofland, J. *Analyzing social settings: A guide to qualitative observation and analysis*. Belmont, Calif.: Wadsworth Publishing Co., 1971.

Lubec, I. Towards a social psychological analysis of research on aggression in social psychology. Unpublished paper, University of Guelph, Canada, 1977.

Lubec, I. The psychological establishment: Pressures to preserve paradigms, publish rather than perish, win funds, and influence students. Unpublished papers, University of Guelph, Canada, 1977.

Lubec, I. Apocalypse versus frustration: An historical appraisal of the relative contributions of three approaches to the study of aggression in social psychology. Unpublished paper, University of Guelph, 1977.

Manis, M. Comment on Gergen's "Social psychology as history." *Personality and Social Psychology Bulletin*, 1975, *1*, 450-455.

McCall, G. J., & Simmons, J. L. (Eds.). *Issues in participant observation: A text and reader*. Reading, Mass.: Addison-Wesley, 1969.

McGuire, W. J. Some impending reorientations in social psychology: Some thoughts provoked by Kenneth Ring. *Journal of Experimental Social Psychology*, 1967, *3*(2), 124-139.

McGuire, W. J. The yin andyang of progress in social psychology: Seven koan. *Journal of Personality and Social Psychology*, 1973, *26*(3), 446-456.

Milgram, S. Some conditions of obedience and disobedience to authority. *Human Relations*, 1965, *18*, 57-76.

Mixon, D. Behavior analysis treating subjects as actors rather than organisms. *Journal for the Theory of Social Behavior*, 1971, *1*(1), 19-32.

Orne, M. T. On the social psychology of the psychological experiment: With particular reference to demand characteristics and their implications. *American Psychologist*, 1962, *17*, 776-783.

Ratner, C. Totalitarianism and individualism in psychology. *Telos*, 1971, *7*, 50-72.

Richardson, H. M. Community of values as a factor in friendships of college and adult women. *Journal of Social Psychology*, 1940, *11*, 303-312.

Rosenberg, M. J. The conditions and consequences of evaluation apprehension. In R. Rosenthal and R. Rosnow (Eds.), *Artifact in behavioral research*. New York: Academic Press, 1969.

Rosenthal, R. *Experimenter effects in behavioral research.* New York: Appleton-Century-Crofts, 1966.

Rosenzweig, S. The experimental situation as a psychological problem. *Psychological Review*, 1933, *40*, 337-354.

Rubin, Z., & Mitchell, C. Couples research as couples counseling: Some unintended effects of studying close relationships. *American Psychologist*, 1976, *31*, 17-25.

Runkel, P. J., & McGrath, J. E. *Research in human behavior: A systematic guide to method.* New York: Holt, Rinehart and Winston, 1972.

Ryan, W. *Blaming the victim.* New York: Random House, 1972.

Sampson, E. E. Psychology and the American ideal. *Journal of Personality and Social Psychology*, 1977, *35*(11), 767-782.

Sanford, N. What happened to action research. *Journal of Social Issues*, 1976, *26*(4), 3-23.

Sartre, J. P. *Being and nothingness* (1956). Translated by H. E. Barnes. New York: Washington Square Press, 1973.

Schiller, M. A quantitative analysis of marriage selection in a small group. *Journal of Social Psychology*, 1932, *3*, 297-319.

Schlenker, B. R. Social psychology and science. *Journal of Personality and Social Psychology*, 1974, *29*(1), 1-15.

Schooley, M. Personality resemblances among married couples. *Journal of Abnormal and Social Psychology*, 1951, *46*, 190-207.

Schulman, A. D., & Berman, H. J. Role expectations about subjects and experimenters in psychological research. *Journal of Personality and Social Psychology*, 1975, *32*, 368-380.

Sells, S. B. Ecology and the science of psychology. In R. H. Moos and P. M. Insel (Eds.), *Issues in social ecology: Human milieus*. Palo Alto: National Press, 1974.

Selltiz, C., Jahoda, M., Deutsch, M., & Cook, S. W. (Eds.). *Research methods in social relations*. New York: Henry Holt Co., 1951.

Sherif, M., Harvey, O., White, B., Hood, W., and Sherif, C. *Intergroup conflict and cooperation: The robbers cave experiment*. Norman, Okla.: Institute of Group Relations, University of Oklahoma, 1961.

Sherif, M., & Hovland, C. I. *Social judgment*. New York: Wiley, 1956.

Sorel, G. *Reflections on violence* (1968). Translated by T. E. Hulme and J. Roth. New York: Collier Books, 1961.

Thorngate, W. Process invariance: Another red herring. *Personality and Social Psychology Bulletin*, 1975, *1*, 485-488.

Tunnell, G. B. Three dimensions of naturalness: An expanded definition of field research. *Psychological Bulletin*, 1977, *84*(3), 426-437.

Turner, R. (Ed.). *Ethnomethodology*. Middlesex, England: Penguin Books Ltd., 1974.

Varela, J. A. *Psychological solutions to social problems: An introduction to social technology*. New York: Academic Press, 1971.

Varela, J. A. Can social psychology be applied? In M. Deutsch and H. Hornstein (Eds.), *Applying social psychology: Implications for research, practice and training*. New York: John Wiley & Sons, 1975.

Varela, J. A. Social technology. *American Psychologist*, 1977, *32*(11), 914-923.

Vidich, A. J., & Shapiro, G. A comparison of participant observation and survey data. *American Sociological Review*, 1955, *20*, 28-33.

Walton, R. E. Using social psychology to create a new plant culture. In M. Deutsch and H. Hornstein (Eds.), *Applying social psychology: Implications for research, practice and training*. New York: John Wiley & Sons, 1975.

Webb, E. J., Campbell, D. T., Schwartz, R. D., & Sechrest, L. *Unobtrusive measures: Nonreactive research in the social sciences*. Chicago: Rand McNally, 1966.

Weick, K. E. Systematic observational methods. In G. Lindzey and E. Aronson (Eds.), *The handbook of social psychology*, Vol. 2, Reading, Mass.: Addison-Wesley, 1968.

Wheeler, H. C. "These kids aren't babies—They're grown-ups!": The socialization of the work world of attendants in a changing institution. In R. C. Knight, C. M. Zimring, W. H. Weitzer, & H. C. Wheeler, *Opportunity for control and the built environment: The ELEMR Project*. Amherst, Mass.: Institute for Man and Environment, 1978.

Willems, E. P. Planning a rationale for naturalistic research. In E. P. Willems & H. L. Raush (Ed.s), *Naturalistic viewpoints in psychological research*. New York: Holt, Rinehart, and Winston, 1969.

Willems, E. P. Behavioral ecology. In D. Stokols (Ed.), *Perspectives on environment and behavior*. New York: Plenum Press, 1977.

Winch, P. *The idea of a social science and its relation to philosophy*. London: Redwood Press, 1958.

Wright, H. F. *Recording and analyzing child behavior*. New York: Harper and Row, 1967.