

The Research Act

A Theoretical Introduction to Sociological Methods



Norman K. Denzin

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Continuous advance in science has been possible only when analysis of the objects of knowledge has supplied not elements of meanings as the objects have been conceived but elements abstracted from those meanings. That is, scientific advance implies a willingness to remain on terms of tolerant acceptance of the reality of what cannot be stated in the accepted doctrine of the time, but what must be stated in the form of contradiction with those accepted doctrines.

George Herbert Mead

In Creative Intelligence: Essays in the Pragmatic Attitude, p. 173.

The most important advice I can give the contemporary sociologist has nothing to do with the validity of my arguments. It is this: you do not have to believe anything about theory and methodology that is told you pretentiously and sanctimoniously by other sociologists—including myself. So much guff has gotten mixed with the truth that, if you cannot tell which is which, you had better reject it all. It will only get in your way. No one will go far wrong theoretically who remains in close touch with and seeks to understand a body of concrete phenomena.

George Caspar Homans

In Handbook of Modern Sociology, pp. 975–976.



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Preface

In this book I present a symbolic interactionist view of sociological theory and methodology. I take the view that theory and method must be brought closer together and that both must be interpreted from a common perspective if sociologists are to narrow the breach that presently exists between their theories and their methods. Accordingly, I have taken one theoretical perspective from the many currently available in the social sciences, and consistently evaluated theory and method from it. Chapter 1 of this book presents the symbolic interactionist and methodological perspective used in my analysis. I develop the case that theories and methods represent different ways of acting on the empirical environment. I indicate the role of symbolic interaction in the conduct of sociology and suggest that there must be a recognition of the “symbolic” before a science called sociology can be systematically built.

Chapters 2 and 3 apply this framework to the process of theory development. Elements of theory are examined and problems of developing theory are discussed. Various views of theorizing are presented, including a perspective consistent with the interactionist point of view, which I feel should be the desired approach to theory development. Chapters 4 and 5 continue the application of this framework to the problems the sociologist confronts as he moves from theory to empirical reality—the selection of sampling or observational units. In Chapter 4 I present a synthesis of statistical and theoretical sampling models. Rather than viewing the sampling problem as resolved either by rigorous statistical procedures or by more loosely conceived theoretical strategies, however, I suggest that all steps in the sampling process must be theoretically guided. When they are, sociologists may sample such theoretically relevant objects as time and its passage, situations and their variations, or persons in different forms of social organization. In addition, theoretical schemes may be sampled just as sociologists currently sample communities, or entire societies.

In Chapter 5 I offer a review of the major measurement and scaling techniques employed by sociologists. Several themes guide my discussion in this chapter. First, measurement is seen as an interaction in which the

sociologist attempts to identify reliably observations as instances of theoretical concepts. These identifications are problematic, and in this context I expand the notions of internal and external validity to include the interaction that occurs in “observational encounters.” Second, the costs and consequences of selecting one measurement strategy over another are examined as I treat the relevance of measurement decisions for subsequent data analyses. I propose an elaborated view of triangulation, or the combination of measurement strategies, as one strategy for resolving the inherent biases of one measurement technique. Finally, the differences between qualitative and quantitative are discussed, and I suggest that qualitative measurement strategies may serve as well as quantitative strategies in the development and testing of social theory.

Chapter 6 takes up the problems of interviewing and here I propose an interactionist conception, treating the interview as a special type of face-to-face encounter. Strategies of interviewing recalcitrant, deviant, hostile, and silent respondents are presented, as are the validity and reliability problems of such encounters. I indicate the appropriate role of interviews in the total observational process and conclude by suggesting that interviews may be studied for what they reveal about interactions between strangers.

Chapters 7 through 11 present what I regard as the five major methods of the sociologist: experiments, surveys, participant observation, life histories, and unobtrusive measures. These methods are assessed in the interactionist perspective. Validity and causation are treated and I propose that the sociologist has three chief means of formulating causal propositions: the experimental model of control, the survey-multivariate analysis model of design, and analytic induction. The use of these models at once raises and solves in different ways certain problems, which are indicated as I treat the strengths and weaknesses of the five methods.

Chapter 12 presents the logic for “multiple triangulation”—a framework for combining multiple theories, observers, data sources, and methods in single investigations. I take the position that sociologists must move beyond single-method, “atheoretical” investigations; sociological reality is such that no single method, theory, or observer can ever capture all that is relevant and important.

The book concludes with a logical and necessary treatment of the ethical, social, and political contingencies that always make sociological investigations less than ideal. There is more to the “doing of sociology” than the formal methods treated in this or any other methodological manual would suggest. The roles of differing pressure groups, ethics, values, and ideologies in the sociological research process are sketched, with special attention to the interpersonal demands of colleagues, granting agencies, clients, and subjects. A perennial dilemma of the sociologist arises from the fact that humans are studying other humans; hence, values, personalities and social pressures lie at the heart of sociology. The final chapter closes with a treatment of the ethical implications of sociological research. Various positions

on this issue are reviewed and, on the grounds that the sociologist's first commitment should be to his discipline, I take a stance that justifies "unannounced," disguised research methods. If we are not permitted to study things that people wish hidden, then sociology will remain a science of public conduct based on evidence and data given us by volunteers, a condition that runs counter to the dominant ethos of the scientific-intellectual enterprise.

Perhaps some readers will feel that I have ignored important recent developments in sociological methods. For instance, I do not treat the use of computers and high-speed data-processing techniques, because of a personal bias against such strategies, which too often become substitutes for the sociological imagination. They are tools that can, and under certain circumstances should, be used, but they are not substitutes for rigorous knowledge and use of sociological methods in the theory process. Nor have I treated recent developments in what some now call the comparative method. The methods treated in this book are well suited to the comparative method—the basic principles of methodology apply to any field setting, a native village in Africa, Cartagena, Colombia, or Chicago. To speak of a comparative method is to speak of all sociological methods. Finally, although I have drawn on materials from ethnomethodology in several of the chapters on the field method, there is no systematic presentation of this point of view, since, with few important exceptions, I see it as a special expression of principles from symbolic interaction theory.

It is my hope that the perspective of and the problems treated in this book will shed new light on old issues and lead the sociologist to better link his vague images of reality with research activities. I am suggesting that theory and method must interact so that each contributes to the growth of the other.



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To Evelyn, Johanna, and Rachel

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I am indebted to my publisher, Alexander J. Morin, for forcing me to revise and for ever reminding me that the act of doing sociology must be seen in symbolic interactionist terms. He has given me a sense of scholarship that I hope someday to attain. Howard S. Becker painfully endured the reading of revision upon revision and his suggestions have kept me mindful that one must learn to defend his positions without being at the same time overly polemical. Richard J. Hill subjected me to the greatest pains; his criticisms forced a major revision. The quality of my presentation owes much to his efforts, and to a reader who remained unidentified. Herbert Blumer's works and remarks have fundamentally shaped my conception of the sociological enterprise; it is my hope that this book does justice to his perspective. My students at the University of Illinois patiently sat through the many drafts of this book. I am grateful for their comments, grimaces and criticisms, as well as the comments of David A. Fabianic and Sheldon Stryker.

For placing the final product in readable form I am indebted to Regan and Lillian Smith. They undertook a rush job at the last moment and admirably performed under the pressures of a tight publication schedule.

I dedicate this book to my wife and two daughters who patiently put up with an absent husband and father. My wife's perspective has indelibly shaped my sociological stance. It is to her and my daughters that I am most deeply indebted.

All books, I suppose, are ultimately personal statements. I am grateful to the above persons for making this book more public in nature.



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Part I

The Interactionist Perspective and the Process of Theory Construction



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Nor is it enough to say that research and theory must be married if sociology is to bear legitimate fruit. They must not only exchange solemn vows—they must know how to carry on from there. Their reciprocal roles must be clearly defined.

Robert K. Merton, 1969, p. 171.

We can, and I think must, look upon human life as chiefly a vast interpretative process in which people, singly and collectively, guide themselves by defining the objects, events, and situations which they encounter. . . . Any scheme designed to analyze human group life in its general character has to fit this process of interpretation.

Herbert Blumer, 1956, p. 686.

Methodology—that vague word sociologists have come to associate with research—has occupied a peculiar role in the sociological enterprise. There are spokesmen who see little connection between methods, research activities, and the process of theorizing. For example, in a particularly perplexing passage, that contradicts the above quotation, Robert K. Merton states:

At the outset we should distinguish clearly between sociological theory, which has for its subject matter certain aspects and results of the interaction of men and therefore is substantive, and methodology, or the logic of scientific procedure. The problems of methodology transcend those found in any one discipline, dealing either with those common to groups of disciplines, or, in more generalized form, with those common to all scientific inquiry. Methodology is not peculiarly bound up with sociological problems, and

though there is a plenitude of methodological discussions in books and journals of sociology, they are not thereby rendered sociological in character. Sociologists, in company with all others who essay sociological work, must be methodologically wise; they must be aware of the design of investigations, the nature of inference, the requirements of a theoretic system. But such knowledge does not contain or imply the particular *content* of sociological theory. There is, in short, a clear and decisive difference between *knowing how to test* a battery of hypotheses and *knowing the theory* from which to derive hypotheses to be tested. It is my impression that current sociological training is more largely designed to make students understand the first than the second [1967, pp. 140–41].

Merton suggests that theory is of greater value than methodology. He further suggests that methods as such have little, if any, substantive-theoretical content. From Merton's perspective, methods are "atheoretical" tools suitable for any knowledgeable and skilled user.

This position, which potentially leads to a wide gap between theory and methodology, contrasts with Blumer's (1931, 1940, 1954, 1956), for as the Blumer quotation beginning this chapter implies, he calls for research and theoretical designs that accurately reflect and capture what he regards as the special features of human interaction. From his perspective the study of methodology demands a consistent theoretical perspective; theory and method must go hand in hand.

Other sociologists have implicitly subscribed to Blumer's position but have tended to use methods with little thought for either their theoretical implications or their differing ability to shed light on theory. Many sociologists now use only one method in their studies—thereby eschewing the potential value of other methodologies. Small-group theorists, for example, rely nearly entirely upon the experiment, while family sociologists primarily utilize the survey technique, and students of organizations overemphasize field strategies such as participant observation. This tendency has given rise to a rather parochial, specialty-bound use of research methods.

Closely related to this position is the tendency to develop within limited boundaries theories resting on special methodologies—what Merton (1967) terms middle-range sociology—and while it brings theory and method closer together, a specific commitment to special areas of inquiry seriously limits the far-ranging value of general or formal theory. To read of a tightly integrated theory of small-group interaction is pleasing because it is theory, but disappointing because it is not developed from a more abstract set of formulations. Small-group theory exists hand in hand with theories of the family, of political sociology, of delinquency, and so on, but seldom do these specialized theories with their localized methods come together in one large and more general theory.

A Point of View

I hold that methods are indeed of great theoretical relevance—that in fact every method has a different relevance for theory, and that significant advances in substantive sociological theory will occur only after sociologists adopt a consistent and viable framework for the dual analysis of theory and method. Each can best be assessed and evaluated in the same general framework, and to this end—out of personal preference—I have selected symbolic interactionism as my perspective (see Mead, 1934; Blumer, 1962, 1966, 1969; Kuhn, 1964). This selection is deliberate because in my judgment interactionism best fits the empirical nature of the social world. It would have been possible to use another theoretical stance—to apply, say, structural-functional theory to the issues treated in this book—but I am convinced that any other approach would lead to significantly different conclusions, even though it would also, at the same time, lend support to my thesis that methods can no longer be viewed as “atheoretical” tools. It should be apparent that each theory demands a special view of methods.

The Interrelationship of Theory and Method

The sociological enterprise may be said to rest on these elements: theory, methodology, research activity, and the sociological imagination. The function of *theory*, which I define as an integrated body of propositions, the derivation of which leads to explanation of some social phenomenon, is to give order and insight to research activities. *Methodology*, on the other hand, represents the principal ways the sociologist acts on his environment; his methods, be they experiments, surveys, or life histories, lead to different features of this reality, and it is through his methods that he makes his research public and reproducible by others. As the sociologist moves from his theories to the selection of methods, the emergence of that vague process called *research activity* can be seen. In this process the personal preferences of a scientist for one theory or method emerge. Furthermore, his selection of a given problem area (e.g., delinquency, the family, etc.) often represents a highly personal decision.

Order is given to theory, methodology, and research activity through the use of what Mills termed the *sociological imagination*.

The sociological imagination, I remind you, in considerable part consists of the capacity to shift from one perspective to another, and in the process to build up an adequate view of a total society and its components. It is this imagination, of course, that sets off the social scientist from the mere tech-

nician. Adequate technicians can be trained in a few years. The sociological imagination can also be cultivated; certainly it seldom occurs without a great deal of routine work. Yet there is an unexpected quality about it. . . . There is a playfulness of mind back of such combining as well as a truly fierce drive to make sense of the world, which the technician as such usually lacks. Perhaps he is too well trained, too precisely trained. Since one can be *trained* only in what is already known, training sometimes incapacitates one from learning new ways; it makes one rebel against what is bound to be at first loose and even sloppy. But you cling to such vague images and notions, if they are yours, and you must work them out. For it is in such forms that original ideas, if any, almost always first appear [1959, pp. 211–12].

The sociological imagination demands variability in the research process. The processes by which sociology is done should not be made too rigorous; an open mind is required. What some regard as doctrinaire will be challenged by others and, therefore, methodological and theoretical principles must always be evaluated in terms of the sociological imagination. Rather than applying just a set of methodological principles to research strategies—which leads to an even greater gap between theory and method—I combine a theoretical perspective with a series of methodological rules, with symbolic interactionism as the theoretical framework and taking certain key principles from the scientific method and applying them to both theory and method. My aim is first to show that each method takes on a different meaning when analyzed in the interactionist framework—and hence can be shown to have different relevance for that theory—and second, by employing notions from the scientific method, I indicate how these methods can best be put to use to fit the demands of interaction theory. Third, and returning to the central thesis, I will suggest that methods are not atheoretical tools, but rather means of acting on the environment and making that environment meaningful. This point of view will, I hope, permit sociologists to overcome what I view as errors of the past, and reduce the gap that presently exists between theory and method. It should also lead sociologists to cease using methods in rote and ritualistic fashion, and enable us to move away from middle-range and small-scope theories to what I will term formal theory (see Simmel, 1950). Finally, I hope that this perspective will assist sociology toward the goal of a mature science of human interaction.

The Interactionist Perspective

The interactionist's conception of human behavior assumes that behavior is self-directed and observable at two distinct levels—the symbolic and the interactional (or behavioral). By “self-directed,” I mean that humans can act toward themselves as they would toward any other object. As Blumer (1966) says, the human may “perceive himself, have conceptions of himself, communicate with himself, and act toward himself [p. 535].” This be-

havior, which Blumer calls “self-interaction,” permits humans to plan and to align their actions with others. Integral to this position is the proposition that man’s social world is not constituted of objects that have intrinsic meaning, but that the meaning of objects lies in man’s plans of action. Human experience is such that the process of defining objects is ever changing, subject to redefinitions, relocations, and realignments, and for conduct toward any object to be meaningful, the definition of the object must be consensual. That is, if I cannot persuade another sociologist to accept my definition of what a particular research method means, I shall be incapable of discussing my actions with him.

The interactionist assumes that humans are able to act because they have agreed on the meanings they will attach to the relevant objects in their environment. But before such consensus can occur, common symbolic languages must be present, and in sociology it is mandatory that agreement over basic terms be established before serious activity can begin. (Consequently it will be necessary to give precise definitions to the terms *theory*, *method*, *experiment*, *social survey*, *participant observation* and *validity*. The interactionist additionally assumes that man learns his basic symbols, his conceptions of self, and the definitions he attaches to his social objects through interaction with others. Man simultaneously carries on conversations with himself and with his significant others.

Methodological Considerations from Interaction Theory

Given these basics of the interactionist perspective, I can now propose a series of principles that this perspective demands of its methodologies. If human behavior is observable at two levels—the symbolic and the behavioral—then central to understanding such behavior are the range and variety of symbols and symbolic meanings shared, communicated, and manipulated by interacting selves in social situations. Society contributes two essential elements that reflect directly on concrete interactions: the symbols, or various languages provided and communicated through the socialization process; and the concrete behavioral settings in which behavior occurs.

An interactionist assumes that a complete analysis of human conduct will capture the symbolic meanings that emerge over time in interaction. But the sociologist must also capture variations in ongoing patterns of behavior that reflect these symbols, images, and conceptions of self. These symbols are manifold and complex, verbal and nonverbal, intended and unintended. Verbal utterance, nonverbal gesture, mode and style of dress, and manner of speech all provide clues to the symbolic meanings that become translated into and emerge out of interaction.

The *first methodological principle* is that symbols and interaction must be brought together before an investigation is complete. To focus only on

symbols, as an attitude questionnaire might, fails to record the emergent and novel relationships these symbols have with observable behavior. If I am studying the relationship between marijuana use and the strategies of concealing the drug in the presence of nonusers I will want to show that a marijuana user's attitude toward outsiders is reflected in his behavior in their presence. It would be insufficient to document only the fact that users do not like to get "high" when an outsider is present. Committed to the interactionist position, I must go further and demonstrate how this attitude is influenced by contact with nonusers.

Becker (1953, 1955, 1962) has provided such an analysis. In his interviews (1962, p. 597) it was discovered that among nonregular smokers fear of discovery took two forms: that nonusers would discover marijuana in one's possession; and that one would "be unable to hide the effects of the drug when he is 'high' with nonusers." This type of user adopts deliberate strategies to conceal the effects and presence of marijuana; he may even smoke infrequently because he cannot find a "safe" setting. Among regular users such fears are not present, although Becker indicated that as their interactional contacts change regular users may find it necessary to revert to only occasional use. One regular user who had married a nonuser eventually turned to irregular use. The following excerpt from Becker describes this pattern and demonstrates how the meanings attached to the social object (marijuana) actually emerged in patterns of interaction:

(This man had used marijuana quite intensively but his wife objected to it.) Of course, largely the reason I cut off was my wife. There were a few times when I'd feel like . . . didn't actually crave for it but would just like to have had some. (He was unable to continue using the drug except irregularly on those occasions when he was away from his wife's presence and control [1962, p. 598].)

A *second methodological principle* suggests that because symbols, meanings, and definitions are forged into self-definitions and attitudes, the reflective nature of selfhood must be captured. That is, the investigator must indicate how shifting definitions of self are reflected in ongoing patterns of behavior. He must, therefore, view human conduct from the point of view of those he is studying—"take the role of the acting other in concrete situations"—and this may range from learning the other's language to capturing his salient views of self. Returning to the example of the marijuana user, it would be necessary to learn the language of marijuana subcultures, which, as Becker shows, includes special words for getting "high" and has various categorizations for "outsiders."

Taking the role of the acting other permits the sociologist to escape the *fallacy of objectivism*; that is, the substitution of his own perspective for that of those he is studying. Too often the sociologist enters the field with preconceptions that prevent him from allowing those studies to tell it "as

they see it." A student of marijuana use, for example, may incorrectly generalize from his own experiences with it to the group of users he is studying. Often the investigator will find that the meanings he has learned to attach to an object have no relevance for the people he is observing. This error occurs frequently in areas of conduct undergoing rapid change; studies of racial interaction, political activity, fads and fashions, and even analyses of stratification hierarchies in bureaucracies may provide cases where the definitions of the sociologist bear only slight resemblances to the actual situation.

Everyday and Scientific Conceptions of Reality

I wish to maintain a distinction between the sociologist's conceptions of his subject's behavior and the motives and definitions that subjects ascribe to their own conduct. The way a subject explains his behavior is likely to differ from the way a sociologist would. Marijuana users, for example, do not employ such terms as "morality," "rationalization," "collusion," "social control," "subculture," "socialization," or "role behavior." Commenting on this fact Becker notes that the sociological view of the world is "abstract, relativistic and generalizing [1964, p. 273]." On the other hand, the everyday conception of reality that guides our subject's conduct is specific, tends not to be generalizing, and is based on special concepts that often lack any scientific validity.

These points suggest that it is insufficient merely to state that the sociologist must take the role of the acting other in his investigations, and that a distinction must be made between everyday conceptions of reality and scientific conceptions of that reality. An adherence to my second principle suggests that the sociologist first learns the everyday conceptions of this reality and then interprets that reality from the stance of his sociological theory. This is the strategy Becker employed in his analysis of the marijuana user. He began with a symbolic interactionist conception of human conduct, and applied it to behavior in the marijuana subculture. His concepts were shaped by the meanings given them by the user, but he retained their sociological meaning. The sociologist must operate between two worlds when he engages in research—the everyday world of his subjects and the world of his own sociological perspective. Sociological explanations ultimately given for a set of behaviors are not likely to be completely understood by those studied; even if they prove understandable, subjects may not agree with or accept them, perhaps because they have been placed in a category they do not like or because elements of their behavior they prefer hidden have been made public. An irreducible conflict will always exist between the sociological perspective and the perspective of everyday life (Becker, 1964). This is a fact the sociologist must recognize. I raise this problem at this point to indicate that a commitment to my second principle goes further than merely

taking the role of the other; sociologists must also place their interpretations within a sociological perspective.

Taking the role of the acting other leads to the *third methodological principle*: The investigator must simultaneously link man's symbols and conceptions of self with the social circles and relationships that furnish him with those symbols and conceptions. Too frequently failure to achieve this link leaves studies of human conduct at an individualistic level, and as a consequence the impact of broader social structures on subjects' conduct can be only indirectly inferred. This principle is not unique to the interactionist perspective, but derives ultimately from a conception of sociology that holds that the impact of social structure on groups and individuals must be examined.

Applying this principle to the study of marijuana use suggests that the investigator must demonstrate how an individual user's definitions of the object are related to his group's conceptions. The following excerpt from Becker's interview with a regular user satisfies this principle.

(You don't dig [like] alcohol then?) No, I don't dig it at all. (Why not?) I don't know. I just don't. Well, see, here's the thing. Before I was at the age where kids start drinking I was already getting on (using marijuana) and I saw the advantages of getting on, you know, I mean there was no sickness and it was much cheaper. That was one of the first things I learned, man. Why do you want to drink? Drinking is dumb, you know. It's so much cheaper to get on and you don't get sick, and it's not sloppy and takes less time. And it just grew to be the thing you know. So I got on before I drank, you know. . . .

(What do you mean that's one of the first things you learned?) Well, I mean, as I say, I was just starting to play jobs as a musician when I got on and I was also in a position to drink on the jobs, you know. And these guys just told me it was silly to drink. They didn't drink either [1962, p. 603].

This interview offers an excellent instance of how a person's attitude toward a social object represents a combination of his own attitudes and those of his social groups. My third principle is satisfied when personal and social perspectives are blended in a fashion similar to Becker's analysis. In Chapters 7 through 11 I show that the major methods of the sociologist meet this requirement in different ways.

The *fourth methodological principle* derives from the statement that any society provides its members with a variety of behavior settings within which interaction can occur. Research methods must therefore consider the "situated aspects" of human conduct—that is, whenever sociologists engage in observation, they must record the dynamics of their specific observational situations. Situations vary widely in terms of the norms governing conduct within them, and participants in any behavioral setting both create and interpret the rules that influence normal conduct within that situation. Record-

ing the situationality of human interaction would be less important if it were not that symbols, meanings, conceptions of self, and actions toward social objects all vary because of the situation. As shown by Becker's study of marijuana users, in "safe" situations among regular users, the marijuana smoker is likely to get "high" and feel no restraints in discussing the effects of the object on his conduct; in "unsafe" situations he will go to extremes of secrecy and concealment.

"Situating" an observation or a respondent may require no more than asking the respondent to answer questions in terms of the situations where he normally engages in the behavior under study. Stone (1954) achieved this goal in his study of female shoppers in a large urban locale; he explicitly situated his respondents by symbolically placing them within their favored shopping locale, thus permitting a designation and description of relevant activities on that basis.

Social selves, I am suggesting, are situated objects that reflect ongoing definitions of social situations. For this reason both the meanings attached to these situations and the types of selves and interactions that emerge within them must be examined. Stone's investigation treats the meanings attached to shopping situations and indirectly infers the types of selves that flow from them. Becker's study achieves both goals: the meaning or definitions of the situation and the self-attitudes of marijuana users in varying situations.

Implicit thus far has been the assumption that the forms and processes of interaction must be reflected in sociological methodologies. Since the emergent relationship between self-conceptions, definitions of social objects, and ongoing patterns of interaction must be recorded, analyzed, and explained, the *fifth methodological principle* is that research methods must be capable of reflecting both stable and processual behavioral forms. Speaking of models of causation, Becker makes the following argument for processual analyses of human behavior.

All causes do not operate at the same time, and we need a model which takes into account the fact that patterns of behavior *develop* in orderly sequence. In accounting for an individual's use of marijuana, as we shall see later, we must deal with a sequence of steps, of changes in the individual's behavior and perspectives, in order to understand the phenomenon. Each step requires explanation, and what may operate as a cause at one step in the sequence may be of negligible importance at another step. We need, for example, one kind of explanation of how a person comes to be in a situation where marijuana is easily available to him, and another kind of explanation of why, given the fact of its availability, he is willing to experiment with it in the first place. And we need still another explanation of why, having experimented with it, he continues to use it. In a sense, each explanation constitutes a necessary cause of the behavior. That is, no one could become a confirmed marijuana user without going through each step. He must have the drug available, experiment with it, and continue to use it.

The explanation of each step is thus part of the explanation of the resulting behavior [1963, p. 23].

As I turn to the individual methods of the sociologist it will become apparent that some are better suited than others for the above kinds of analyses, that surveys better measure static and stable forms of behavior while life histories and participant observation more adequately lend themselves to processual analyses.

The Role of Methods

The *sixth methodological principle* necessarily becomes more abstract and reflects directly on the role of methods in the entire sociological enterprise. It states that the very act of engaging in social research must be seen as a process of symbolic interaction, that being a scientist reflects a continual attempt to lift one's own idiosyncratic experiences to the level of the consensual and the shared meaning. It is in this context that the research method becomes the major means of acting on the symbolic environment and making those actions consensual in the broader community of sociologists.

When a sociologist adopts the surveys as a method of research he does so with the belief that when he reports his results other investigators will understand how he proceeded to gather his observations. The word *survey* designates a social object that has some degree of consensus among other sociologists. But more than this the word implies a vast variety of actions in which one will engage after he has adopted the method. Persons will be sampled, questionnaires will be constructed, responses will be coded, computers will be employed, and some form of statistical analysis will be presented. If, on the other hand, participant observation is chosen as a method, smaller samples will be selected, documents will be collected, informants will be selected, unstructured interviewing will be done, and descriptive statistical analyses will be presented.

If a situation can be imagined in which two sociologists adopt different methods of study, the impact of symbolic interaction on their conduct can be vividly seen. Suppose that the same empirical situation is selected—for example, a mental hospital. The first investigator adopts the survey as his method; the second, participant observation. Each will make different kinds of observations, engage in different analyses, ask different questions, and—as a result—may reach different conclusions. (Of course the fact that they adopted different methods is not the only reason they will reach different conclusions. Their personalities, their values, and their choices of different theories will also contribute to this result.)

Ultimately the sociologist's actions on the empirical world are achieved by the adoption of specific methodologies. His actions are translated into specific methods through lines of action that reflect his definitions of those

methods. At the heart of this interaction is the concept. The concept, in conjunction with the research method, enables the sociologist to carry on an interaction with his environment. Observers indicate to themselves what a concept and a method mean and symbolically act toward the designation of those meanings. Sociologists are continually reassessing their imputed object meanings—assessing them against their relationships to theories, their ability to be observed by others, and their ability to generate understanding and explanation of empirical reality.

This point can be illustrated by again turning to Becker's study of the marijuana user. Beginning with an interactionist conception of human conduct, Becker applied the generic principles from that perspective to the problem of how occupancy of a role in a subculture shapes a person's perceptions and activities. His theory suggested that an intimate knowledge of the subject's perspective must be learned, and to this end he adopted the open-ended interview and participant observation as methodological strategies. Beginning with this conception, Becker's main line of action was to approach marijuana users and to have them present their experiences as they saw them. The final result of his analysis was a series of research findings that modified a role theory and subcultural theory of deviant behavior. In formulating his research observations and conclusions, Becker continually assessed his findings against his conceptual framework; his methods and concepts continuously interacted with observations and theory—that is, symbolic interaction guided the process of his research and theory construction.

The scientist, then, designates units of reality to act upon, formulates definitions of those objects, adopts research methods to implement these lines of action, and assesses the fruitfulness of his activity by his ability to develop, test, or modify existing social theory. Thus, both his concept and his research methodology act as empirical *sensitizers* of scientific observation. Concepts and methods open new realms of observation, but concomitantly close others. Two important consequences follow: If each method leads to different features of empirical reality, then no single method can ever completely capture all the relevant features of that reality; consequently, sociologists must learn to employ multiple methods in the analysis of the same empirical events.

It can of course be argued that all research methods stand in an instrumental relationship to the scientific process. Methods become plans of action employed as sociologists move from theory to reality. They are the major means of organizing creative energy and operational activities toward concepts and theories and, as such, they at once release and direct activity, the success of which is assessed by the ability to satisfy the normal criteria of validity while establishing fruitful ties with theory.

Research methods serve to provide the scientist with data that later may be placed in deductive schemes of thought. By observing several discrete instances of a concept or a series of concepts, scientists are able to move

above the single instance to the more common problems that transcend immediate perceptions and observations. A failure to move beyond particularistic observations leaves the sociologist at the level of descriptive empiricism. He must establish articulations between his observations and some variety of theory. To the extent that Becker's investigation was related to a theoretical framework, he satisfied this demand. I can now claim another important role for methods in the scientific process: Methods are one of the major ways by which sociologists gather observations to test, modify, and develop theory.

In this sense, methods go hand in hand with the following less rigorous techniques of theory-work. It is reasonable to argue, I believe, that methods do not do all the relevant work for the sociologist. As stated earlier, underlying the use of methods must be a sociological imagination. It is necessary to recognize that such techniques as introspection, the use of imagined experiments, and the playful combination of contradictory concepts also serve as aids in the development of theory. Methods, because of their more public nature are too frequently given greater attention than these other techniques that are of equal relevance. (In Chapter 2 I will develop further the use of introspection and imagined experiments in the construction of social theory.)

The *seventh methodological principle* indicates that from the interactionist's perspective the proper use of concepts is at first sensitizing and only later operational; further, the proper theory becomes formal; and last, the proper causal proposition becomes universal and not statistical. By *sensitizing concepts* I refer to concepts that are not transformed immediately into *operational definitions* through an attitude scale or check list. An operational definition defines a concept by stating how it will be observed. Thus if I offer an *operational definition* for "intelligence," I might state that intelligence is the score received on an I.Q. test. But if I choose a *sensitizing approach* to measuring intelligence, I will leave it nonoperationalized until I enter the field and learn the processes representing it and the specific meanings attached to it by the persons observed. It might be found, for example, that in some settings intelligence is not measured by scores on a test but rather by knowledge and skills pertaining to important processes in the group under analysis. Among marijuana users intelligence might well be represented by an ability to conceal the effects of the drug in the presence of nonusers. Once I have established the meanings of a concept, I can then employ multiple research methods to measure its characteristics. Thus, closed-ended questions, direct participation in the group being studied, and analysis of written documents might be the main strategies of operationalizing a concept. Ultimately, all concepts must be operationalized—must be measured and observed. The sensitizing approach merely delays the point at which operationalization occurs.

Goffman's treatment of stigma provides an excellent example of what I mean by "sensitizing a concept." He began with a rather vague and loose

definition of stigma that he claimed was "an attribute that is deeply discrediting." Three types of this attribute were designated: abominations of the body or physical deformities, blemishes or character (mental disorder, homosexuality, addiction, alcoholism), and last, tribal stigma of race, nation, and religion. Moving beyond classification, he analyzed data collected in such traditional sociological specialities as social problems, ethnic relations, social disorganization, criminology, and deviance. From these areas, relevant commonalities were organized around the stigma theme. In summarizing this analysis he states:

I have argued that stigmatized persons have enough of their situations in life in common to warrant classifying all these persons together for purposes of analysis. An extraction has thus been made from the traditional fields of social problems. . . . These commonalities can be organized on the basis of a very few assumptions regarding human nature. What remains in each one of the traditional fields could then be reexamined for whatever is really special to it, thereby bringing analytical coherence to what is now purely historic and fortuitous unity. Knowing what fields like race relations, aging and mental health share, one could then go on to see, analytically, how they differ. Perhaps in each case the choice would be to retain the old substantive areas, but at least it would be clear that each is merely an area to which one should apply several perspectives, and that the development of any one of these coherent analytic perspectives is not likely to come from those who restrict their interest exclusively to one substantive area [1963, pp. 146–47].

Sensitizing a concept permits the sociologist to discover what is unique about each empirical instance of the concept while he uncovers what it displays in common across many different settings. Such a conception allows, indeed forces, the sociologist to pursue his interactionist view of reality to the empirical extreme.

The notion of formal as opposed to other types of theory will be further developed in chapters 2 and 3. At this point it is only necessary to indicate that such a stance relates directly to the assumption that universal explanations of human behavior can be developed. With Simmel (1950, pp. 3–25), I argue that human conduct presents itself in behavioral forms that differ only in content. The job of sociology is to discover the forms that universally display themselves in slightly different contexts. Simmel termed this the strategy of "formal sociology," an attempt to abstract from generically different phenomenon commonalities or similarities. The synthesis of these common threads into a coherent theoretical framework represents the development of "formal theory."

Society, for Simmel, existed only in forms of interaction:

More specifically, the interactions we have in mind when we talk of "society" are crystallized as definable, consistent structures such as the state

and the family, the guild and the church, social classes and organizations based on common interests.

But in addition to these, there exists an immeasurable number of less conscious forms of relationship and kinds of interaction. Taken singly, they may appear negligible. But since in actuality they are inserted into the comprehensive and, as it were, official social formations, they alone produce society as we know it. . . . Without the interspersed effects of countless minor syntheses, society would break up into a multitude of discontinuous systems. Sociation continuously emerges and ceases, emerges again. . . . That people look at one another and are jealous of one another; that they exchange letters or dine together; that irrespective of all tangible interests they strike one another as pleasant or unpleasant; that gratitude for altruistic acts makes for inseparable union; that one asks another man after a certain street, and that people dress and adorn themselves for one another—the whole gamut of relations that play from one person to another and that may be momentary or permanent, conscious or unconscious, ephemeral or of grave consequence (and from which these illustrations are quite causally drawn), all these incessantly tie men together. Here are the interactions among the atoms of society [1950, pp. 9–10].

The sociological task, for Simmel, became the isolation of these forms of interaction.

In its very generality, this method is apt to form a common basis for problem areas that previously, in the absence of their mutual contact, lacked a certain clarity. The universality of sociation, which makes for the reciprocal shaping of the individuals, has its correspondence in the singleness of the sociological way of cognition. The sociological approach yields possibilities of solution or of deeper study which may be derived from fields of knowledge continually quite different (perhaps) from the field of particular problem under investigation [1950, p. 14].

As examples of this strategy Simmel suggests that the student of mass crimes might profitably investigate the psychology of theater audiences. Similarly, the student of religion might examine labor unions for what they reveal about religious devotion, the student of political history, the history of art. The argument, I believe, is clear: A series of concepts and propositions from the interactionist perspective are thought to be sufficient to explain the wide ranges of human behavior—whatever the social or cultural context.

More contemporary spokesmen of this position include Goffman and Homans. Goffman proposes a “formal sociological” stance for the analysis of face-to-face interaction.

Throughout this paper it has been implied that underneath their differences in culture, people everywhere are the same. If persons have a universal human nature, they themselves are not to be looked to for an explanation of it. One must look rather to the fact that societies everywhere, if they are to

be societies, must mobilize their members as self-regulating participants in social encounters. One way of mobilizing the individual for this purpose is through ritual; he is taught to be perceptive, to have feelings attached to self and a self expressed through face, to have pride, honor and dignity, to have considerateness, to have tact and a certain amount of poise. . . . If a particular person or group or society seems to have a unique character of its own, it is because its standard set of human-nature elements is pitched and combined in a particular way. Instead of much pride, there may be little. Instead of abiding by rules, there may be much effort to break them safely. But if an encounter or undertaking is to be sustained as a viable system of interaction organized on ritual practices, then these variations must be held within certain bounds and nicely counterbalanced by corresponding modifications in some of the other rules and understandings. Similarly, the human nature of a particular set of persons may be specially designed for the special kind of undertakings in which they participate, but still each of these persons must have within him something of the balance of characteristics required of a usable participant in any ritually organized system of social activity [1967, pp. 44–45].

While the reader need not accept Goffman's theoretical perspective, its thrust is apparent—a small set of very abstract and general principles can explain all human behavior. Statements similar to Goffman's have been made by Homans, who has suggested that principles from economics and behavioral psychology can be employed to explain all of human conduct.

I believe that, in view of the deficiencies of functional theory, the only type of theory in sociology that stands any chance of becoming a general one is a psychological theory, in the sense that the deductive systems by which we explain social behavior would, if completed, contain among their highest-order propositions one or more of those I call psychological. The time may come when they will lose their place at the top, when they in turn will be shown to be derivable from still more general propositions such as those of physiology. But the time has not come yet, and psychological propositions remain our most general ones [1964, p. 968].

In the statements of Simmel, Goffman, and Homans there is an explicit commitment to formal sociological theory. Homans' theory would be based on propositions from psychology, Goffman's from functional theory and certain portions of symbolic interaction. In this context I can now *define* formal theory as any set of interrelated propositions based on a small set of concepts. Furthermore, these concepts will be ordered in such a way that some are more specific than others and hence capable of being derived from higher-order statements. Once this feature is achieved, *explanation* of the behavior indicated by those propositions shall be said to have occurred. A last feature of the formal theory, which distinguishes it from other types of theory, is the fact that it explicitly rests on empirical referents. Goffman's formulations are based on the observation that wherever face to face inter-

action occurs, participants will be observed employing strategies of tact, pride, defence, honor, and dignity. His highest-order proposition holds that all societies train their member-participants in the rituals of face-to-work because to do otherwise would leave that society without participants who could routinely engage in interaction. His lower-order propositions then include predictions concerning the balance between various types of rituals and their enactment in daily encounters.

While I have not extensively quoted from Homans, his highest-order proposition holds that "The more rewarding men find the results of an action, the more likely they are to take this action" [1964, p. 968]. It is Homans' belief that variations on this proposition will explain historical revolutions, daily interactions in work groups, and conduct within social organizations.

The work of these two spokesmen illustrates the use of formal theory as I have defined it. Contrast their perspective with that of Merton (1967 pp. 39–72), who believes that sociologists should develop middle-range theories of specific problem areas. Merton's formulation is too restrictive for our purposes; it leads to the endless proliferation of small-scope theories. (I shall develop this point in greater detail in the next chapter.) Grand theory represents the other alternative; it suggests that one very abstract and general theory can be developed to explain all of human behavior. Unfortunately, as it is currently practiced, grand theory has few, empirical referents. Formal theory, empirically grounded at all points, is preferable to a grand theory with a few empirical referents, or a series of middle-range theories, each of which have their own methods and specific domains.

Basic to formal theory will be universal interactive propositions that are assumed to apply to all instances of the phenomenon studied—at least until a negative case is discovered. By stating that these propositions will be interactive, I suggest that they will describe interrelationships between processes that mutually influence one another. In Becker's analysis of the marijuana user, an explicit reliance on interactive propositions of universal relevance can be seen.

The analysis is based on fifty intensive interviews with marijuana users from a variety of social backgrounds and present positions in society. The interviews focused on the history of the person's experience with the drug, seeking major changes in his attitude toward it and in his actual use of it and the reasons for these changes. Generalizations stating necessary conditions for the maintenance of use at each level were developed in initial interviews, and tested and revised in the light of each succeeding one. The stated conclusions hold true for all the cases collected and may tentatively be considered as true of all marijuana users in this society, at least until further evidence forces their revisions [1962, p. 592].

Becker's generalizations rest on the assumption that they apply to all persons who have ever used marijuana. More abstractly, his formula-

tions bear a relationship to a formal theory concerning symbolic interaction and the development of self-attitudes in a group setting. The earlier quoted passage describing the marijuana user who altered his using patterns after marrying a nonuser represents a description of an instance of interaction. The user's attitudes toward the object shifted and changed as he was forced to interact daily with a person who did not hold his definitions.

If the fact of human behavior is interaction, then sociological propositions must take an interactional form. In this sense Becker's analysis fits the criterion. The seventh principle, to summarize, is that methods must be constructed so that they contribute to formal theory while at the same time permitting sensitizing concept analysis and the discovery and verification of universal interactive propositions.

The Interactionist Principles in Review

I have shown that interaction theory suggests seven principles against which methods and sociological activity may be evaluated. These principles state:

1. Symbols and interactions must be combined before an investigation is complete.
2. The investigator must take the perspective or "role of the acting other" and view the world from his subjects' point of view—but in so doing he must maintain the distinction between everyday and scientific conceptions of reality.
3. The investigator must link his subjects' symbols and definitions with the social relationships and groups that provide those conceptions.
4. The behavior settings of interaction and scientific observation must be recorded.
5. Research methods must be capable of reflecting process or change as well as static behavioral forms.
6. Conducting research and being a sociologist is best viewed as an act of symbolic interaction. The personal preferences of the sociologist (e.g., his definitions of methods, his values and ideologies, etc.) serve to shape fundamentally his activity as an investigator, and the major way in which he acts on his environment is through his research methods.
7. The proper use of concepts becomes sensitizing and not operational; the proper theory becomes formal and not grand or middle-range; and the causal proposition more properly becomes interactional and universal in application.

These principles will reappear in my subsequent analysis of theory and method. They are somewhat peculiar to the interactionist perspective, but their use gives sociologists one consistent theoretically grounded point of

evaluation. I now turn to salient features from the scientific method which must be combined with the interactionist point of view. By wedding the two, a degree of rigor and precision may be added to the final set of evaluative principles. I assume, then, that any theory, and research methodology must meet the following additional preconditions.

Considerations of Validity, the Causal Proposition, and the Interactive Context of Observation

All research methods must provide answers to the problem of causal inference. A method must permit its user to gather data concerning time, order and covariance between variables, while allowing the discarding of rival causal factors. When it is claimed that one variable or process caused another, it must be shown that the causal variable occurs before that it is assumed to cause; that is, as the causal variable changes value, so too must the variable being caused. When investigators formulate causal statements they must recognize that other variables—rival causal factors—not directly measured or considered may be causing the variations observed. Before discussing the nature of rival causal factors, it is necessary to describe the causal proposition itself. Becker's analysis of marijuana use provides my illustration, and central to his theory is the point that:

The extent of an individual's use of marijuana is at least partly dependent on the degree to which conventional social controls fail to prevent his engaging in the activity. Apart from other possible necessary conditions, it may be said that marijuana use can occur at the various levels described only when the necessary events and shifts in conception of the activity have removed the individual from the influence of these controls and substituted for them the controls of the subcultural group [1962, p. 606].

This and previous excerpts from Becker's study provide the basis of the following propositions.

Proposition One: A potential marijuana user must have regular access to the drug and must learn the proper means of acting toward it before he will become a regular user.

Proposition Two: An individual will engage in marijuana use to the extent that he symbolically removes the effects of conventional social control mechanisms upon his behavior.

Proposition Three: If a regular user systematically comes in contact with nonusers, he will alter his drug-using patterns to use it only among members of the subculture, or he will adopt strategies of secrecy to conceal his use of the drug when in the presence of nonusers.

Proposition Four: The extent of use among nonusers will vary by the strength of the nonusers' negative attitudes. If these negative attitudes are strong and if the situation does not permit concealment, then nonregular use will occur.

Additional propositions could be developed from Becker's argument, but the above are sufficient to illustrate the essential features of the causal proposition. Becker's primary causal variable is the reduction in effect of normal mechanisms of social control. His basic dependent, or caused variable, is the degree and nature of drug use. Given this dependent variable, he proceeds to establish time order between the variables by showing that the degree of use varies by the attitudes of nonusers with whom the user is in contact; the degree of access to the drug; the attitudes the person develops toward the drug; and the situations available for its use. Regular and systematic use thus occurs when marijuana is available on a routine basis, when the user's interactional partners sanction and reinforce its use, and when the user has learned how to act toward the effects of the drug.

It is important to note that Becker has introduced additional causal factors into his final explanatory network. No single variable is assumed to cause marijuana use. The four factors outlined are his additional causal factors and he carefully points to the situations under which they will have the greatest impact. These propositions meet the normal criteria of the causal proposition: time order, covariance, and the partial consideration of rival, or additional causative factors.

Rival Causal Factors

While Becker's study treats additional causal factors that influence the extent of marijuana use, it does not give careful consideration to rival factors that may have distorted or, in fact, caused his observed causal relationship. Rival causal factors may emerge from the following: time and its passage; the situations of observation; characteristics of those observed; characteristics of the observer; and interaction among any of the preceding four elements. In short, the generic question asked when an observer focuses upon rival causal factors is whether or not the causal propositions he has formulated accurately represent the events under study, or whether aspects of the process of making those observations caused the differences. If the investigator concludes that rival factors have caused his observed differences, then he is unable to generalize his findings to other situations, and his research has failed to reach the goal of developing sound causal propositions.

Traditionally, rival causal factors have been treated as falling into two broad classes: either factors external to the observations themselves, or factors that arise from or during the observational process. External factors are termed conditions of *external validity* and internal factors are labeled

conditions of *internal validity*. External validity asks to what populations, settings, treatment variables, and measurement variables the causal propositions may be generalized (Campbell, 1963a: p. 214). Internal validity asks whether the assumed causal variables made a difference, or whether the observational process caused the difference. Under internal validity Campbell (1963a: p. 215) presents the following eight factors.

1. *History*: the other specific events occurring between the first and second measurement in addition to the experimental variable.
2. *Maturation*: processes within the respondent operating as a function of the passage of time *per se* (not specific to the particular events), including growing older, growing hungrier, growing tired, and the like.
3. *Testing*: the effects of taking a test upon the scores of a second testing.
4. *Instrumentation*: changes in the calibration of a measuring instrument or changes in the observers or scorers which may produce changes in the obtained measurements.
5. *Statistical regression*: regression operating when groups have been selected on the basis of their extreme scores.
6. *Selection*: Biases resulting in differential recruitment of respondents for the comparison groups.
7. *Experimental mortality*: the differential loss of respondents from the comparison groups.
8. *Selection-maturation interaction, etc.*: in certain of the multiple-group quasi-experimental designs, such as the nonequivalent control-group design, such interaction is confounded with (i.e., might be mistaken for) the effect of the experimental variable.

External validity is seen by Campbell (1963a, p. 215) as involving:

9. The *reactive* or *interaction effect* of *testing*, in which a pretest might increase or decrease the respondent's sensitivity or responsiveness to the experimental variable and thus make the results obtained for a pretested population unrepresentative of the effects of the experimental variable for the unpretested universe from which the experimental respondents were selected.
10. *Interaction* effects between *selection* bias and the *experimental variable*.
11. *Reactive effects of experimental arrangements*, which would preclude generalization about the effect of the experimental variable for persons being exposed to it in nonexperimental settings.
12. *Multiple-treatment interference*, a problem wherever multiple treatments are applied to the same respondents, and a particular problem for one-group designs involving equivalent time-samples or equivalent materials samples.

Campbell's concern is clear. Changes in observers, measuring instruments, and subjects during the course of a study can distort the events investigated. The passage of time as seen by subjects' maturation, or as evidenced in historical shifts and events, can introduce distorting influences, as can unique characteristics of respondents.

As far as it goes, this system is fine, but it lacks a theoretically grounded set of criteria for the evaluation of research methods. I turn to interaction theory for such a grounding. My earlier treatment of this theory suggests that for human interaction to occur, the following elements must be present: two or more persons able to take the role of the other; a situation for the interaction to occur in; and time to carry out that interaction. When these factors are present an interactional sequence is observed. Persons dining, making love, listening to a record, or negotiating a purchase are in such sequences.

Placed within this context, it is evident that social research becomes a type of symbolic interaction. Role-taking must occur, meaningful symbols must be present, situations have to be available, and time has to be allocated for the research (filling out a questionnaire, taking part in an experimental task). Any encounter between an investigator and a subject shall be termed an *observational encounter*. The interactants shall be called *observer* and *observed*. The situations will range from laboratories, offices, classrooms, living rooms—even automobiles. The time sequence may be brief, lasting no longer than an hour or two, or may extend into months and years, as it does in long-term field studies.

Every research method represents a special combination of these interactive elements. Some, like the experiment, are relatively short-term, include persons who remain unacquainted, and occur in structured situations. Others, like the life history and participant observation, take the investigator to diversely structured situations, rest on multiple identities, lead to the development of close relationships between observer and observed and, as noted, extend over long periods of time.

Each of the interactive elements (time, situations, characteristics of observers and observed) introduce into any study a special set of potentially distorting factors. Unless the observer is aware of his own characteristics, attends to unique features of time and its passage, and records the nature of the situations where his observations occur, he cannot validly develop sound causal propositions.

The Observer Observers vary by their interactive style, their self-concepts, their interpretations of the research project, and their ability to relate to those observed. I assume that whenever an observer gathers an observation he brings into that observational sequence a series of attributes that make his observations different from any other observer. Some experimenters, for example, are overattentive to the emotions of subjects—others are underattentive. Some survey interviewers insist on using first names—others are

more formal. The list of possible variations is endless, yet every stylistic difference can distort the processes under study.

The Observed No respondent is perfectly duplicated by another, so the unique characteristics of respondents and subjects may also introduce distortion into an observation. When it is realized that observers interact with those observed, it can be seen that the interaction between an interviewer and respondent, experimenter and subject, observer and informant, may itself create difference across observational encounters.

The Situation Because all behavior occurs in social situations, the settings of observation may become sources of invalidity. One interview may take place in a living room, another in an office, and still another in an automobile. Each of these settings is different from the other, each has different rules governing permissible, comfortable, and serious interaction. To the extent that there is variation, the behavior that occurs will also vary. Thus, situations of observation must be treated as a class of rival causal factors.

Time and Its Passage Interaction involves an orderly sequence of events that unfold over a temporal period. Every observational encounter must be seen as having its own unique temporal career. Some are long, others short; some are difficult, others flow smoothly. During the passage of time events extraneous to an observation may occur (this is what Campbell means by historical factors jeopardizing internal validity). Yet the passage of time also signals changes in observers and those observed. Self-concepts change, intents of the investigation may shift, and symbols may take on new meanings. The following statement by Geer illustrates the type of change that may occur in the first week of a field study.

Throughout the time the undergraduate study was being planned, I was bored by the thought of studying undergraduates. They looked painfully young to me. I considered their concerns childish and uninformed. I could not imagine becoming interested in their daily affairs—in classes, study, dating, and bull sessions. I had memories of my own college days in which I had appeared as a child: over-emotional, limited in understanding, with an incomprehensible taste for milk shakes and convertibles.

Remembering my attitudes as I began to sort out the thirty-four comments in the field notes on the prefreshmen, I expected to find evidence of this unfavorable adult bias toward adolescents. But on the third day in the field I am already taking the students' side. . . . Perhaps the rapid development of empathy for a disliked group does not surprise old hands at field work, since it seems to happen again and again. But it surprised me; I comment on it seven times in eight days [1964, pp. 328–29].

Campbell's dimensions of mortality, maturation, testing effects, and instrumentation also relate to time. In studies that require repeated observations, the mortality factor becomes crucial. Persons observed at time two may differ from those observed at time one. Loss of subjects, or even observers, can create distortion, and this can also happen with testing effects

in which the subject takes on a special attitude because of the interpretation given the first test, interview, or experimental session. Similarly, shifts in measuring instruments over time may occur.

Observers, situations, subjects, and measuring instruments, then, become social objects within the research design. They are objects whose meaning shifts because their meaning is created through the process of interaction. The sociologist must guard against these changes, if only by recording his interpretations of them.

Treatment of the Rival Factor

In subsequent chapters I shall show how each research method addresses the potential distortion created by the rival factors of time, situations, observers, and observed. Here I want only to indicate the possible strategies for their treatment. In formulating causal propositions the sociologist has three basic strategies of control and design. Under conditions of great rigor, as in experiments, he can explicitly design situations of observation where time order, covariance, and rival causal factors are manipulated. The use of the experimental model of inference is the strongest strategy for formulating the causal proposition and, by implication, is the strongest method of controlling rival causal factors.

Under conditions of less rigor, such as the social survey, the sociologist loses the control given by the experimental model and must resort to a method of analysis termed the multivariate method. Events remain uncontrolled, but the investigator constructs comparisons within his sample that parallel, as nearly as possible, the experimental model. He may compare persons with college education to those completing high school on attitudes toward sexual permissiveness, constructing two comparison groups that vary on the independent variable (education) and measuring the relationship of education to the dependent variable (sexual permissiveness). In his analysis the sociologist will attempt to treat rival causal factors by classifying them as events that were either antecedent to his main variables, or intervening between them. He will maintain the same classification in his attempt to establish covariance and time order for the principle variables.

An important consequence follows from this strategy. Because he lacks control over the temporal occurrence of his variables, the investigator must infer their relationship. He cannot control their occurrence as the experimenter does, and this places him one step below his experimental counterpart. Where the experimenter controls his variables, the survey analyst must infer their relationship.

The last model of inference the sociologist may utilize is analytic induction. Experimental control is again absent, but now the investigator follows the events he is studying through time. This is best represented in studies employing participant observation and life histories. Rather than snap-

shooting the relationship between variables as the survey method so often does, the user of analytic induction engages in long-term studies that permit the direct identification of time order, covariance, and rival causal factors. Yet he too lacks experimental control. But while experimental and survey models lead to causal propositions that treat proportions of events subsumed under a proposition (e.g., 90 per cent of those college-educated favor permissive sexual attitudes, while only 35 per cent of those high-school educated favor such permissiveness), analytic induction generates propositions that cover every case analyzed; it leads to universal-interactive propositions. The last important feature of analytic induction is its emphasis on negative cases that refute the investigator's propositions. In his search for universal propositions, the user of this method seeks cases that most severely test his theory, and until his propositions cover every case examined, his theory remains incomplete.

These three strategies (experimental method, multivariate analysis, analytic induction) represent the major means the sociologist has for examining causal propositions. They also represent the principal strategies of handling rival causal factors. The experimental model controls them, the survey method infers them, and analytic induction follows their occurrence over time.

Each research method employs one or more of these strategies, and each in its own way treats rival causal factors. Consequently, each method can be evaluated in terms of its ability to construct sound causal propositions. To the extent that one method permits greater control of situations, time, observers, and subjects than another, it is superior to the other.

The Logic of Triangulation

Unfortunately no single method ever adequately solves the problem of rival causal factors. For example, while experiments can establish valid causal propositions, the problem of observer reactivity (his presence in a laboratory) potentially creates a situation where subjects act as they think the experimenter wants them to. Following Webb, *et al.* (1966), I conclude that no single method will ever permit an investigator to develop causal propositions free of rival interpretations. Similarly, I conclude that no *single* method will ever meet the requirements of interaction theory. While participant observation, for example, permits the careful recording of situations and selves, it does not offer direct data on the wider spheres of influence acting on the self. I earlier stated (p. 13) that because each method reveals different aspects of empirical reality, multiple methods of observations must be employed. This is termed triangulation and I now offer as a final methodological rule the principle that multiple methods must be used in every investigation, since no method is ever free of rival causal factors, (and thus seldom leads to completely sound causal propositions), can ever

completely satisfy the demands of interaction theory, or can ever completely reveal all of the relevant features of empirical reality necessary for a theory's test or development. A similar conclusion is reached by Webb, *et al.*:

It is too much to ask of any single class that it eliminate all the rival hypotheses subsumed under the population-, content-, and reactive-effects groupings. As long as the research strategy is based on a single measurement class, some flanks will be exposed. . . . If no single measurement class is perfect, neither is any scientifically useless . . . for the most fertile search for validity comes from a combined series of different measures, each with its idiosyncratic weaknesses, each pointed to a single hypothesis. When a hypothesis can survive the confrontation of a series of complementary methods of testing, it contains a degree of validity unattainable by one tested within the more constricted framework of a single method. . . . There must be multiple operationalism [1966, pp. 173–74].

I will offer a definition of each method that rests on several discrete, but interrelated strategies and techniques. For example, I will show that participant observation is best seen as a method that combines survey data, descriptive statistical analysis, quasi-experimental variations, document analysis, and direct observation.

My last criterion under the category of validity, then, is the triangulation of methodologies. This proposes a new line of action for the sociologist as well as a new set of symbolic meanings for the research process generally. I concur with Webb, *et al.* (1966), who argue that in the present stage of social research it is no longer appropriate to conceive of single-method investigations. The combination of multiple methods—each with their own biases, true—into a single investigation will better enable the sociologist to forge valid propositions that carefully consider relevant rival causal factors.

Discovery versus Verification

Should the sociologist give more attention to verifying, or to modifying existing theory? Many sociologists have abandoned theory development for a concern with refining their methods and skills of verification; but methods are of value only to the extent that they lead to better social theory—and the basic concern of the sociologist should be the development of theory. My own concern with combining principles from interaction theory and causal analysis represents a dual interest in discovery and verification. Interaction theory tells the sociologist how to approach empirical reality; the principles of causal analysis tell him how to identify valid, correct, and reproducible observations. The two problems are inseparable, and until

sociology has a set of criteria that permits discovery and verification, the separation between theory and method will persist.

Notes

This and subsequent chapters draw heavily upon Glaser and Strauss' (1967) treatment of theory and research in contemporary sociology. In this book the case is made that sociologists have ceased to concern themselves with theory development, and that a concern with theory verification has increased the division between theory and method. Glaser and Strauss offer a series of very useful strategies for generating theory with qualitative research methods.

The symbolic interactionist perspective that guides my interpretation of theory and method is best presented in Mead (1934), Blumer (1969) and is well reviewed in Manis and Meltzer (1966), in Rose (1962), and in Stone and Farberman (1969).

My distinction between everyday and sociological conceptions of reality draws on Cicourel (1964, 1967), Garfinkel (1967), and Schutz (1963). What has recently been termed ethnomethodology by Garfinkel (1967) represents another attempt to maintain this distinction. The terms *phenomenology*, *ethnomethodology*, and *symbolic interactionism* represent different attempts to confront empirical reality from the perspective of those who are being studied.

Because formal theory occupies a central position in my perspective, the reader is advised to consult criticisms of the strategy. In addition to the Wolff (1950) collection of Simmel's work, the volumes by Coser (1965), Wolff (1959), and Spykman (1966) provide criticisms and comments on Simmel's method. An especially critical review of Simmel's concept of formal theory is contained in Sorokin's essay, which appears in Coser (1965, pp. 142–53).

My treatment of validity and the causal proposition draws on Hirschi and Selvin's (1967) critical review of research methods in the field of delinquency research. The reader should consult this work for an excellent application of the principles of causal analysis to substantive area research. In my attempt to rework Donald T. Campbell's conception of rival causal factors, I have modified the scheme of analysis presented in his 1963 treatment of experimental design (co-authored with Julian Stanley).

For fuller discussions of the three models of inference that may be employed in the resolution of causal problems, consult chapters 7, 8, and 9.

A problem not treated in this chapter, but one that is taken up later, is the use of sampling models in the selection of units of observation. Glaser and Strauss (1967, pp. 45–77) offer a useful but critical review of theoret-

ical and statistical sampling models. My position (see Chapter 4) is that a theoretically directed sampling model that stresses the random selection of observational units provides the most reasonable strategy for selecting units of observation, since it assures the investigator that his subjects accurately represent the populations they are drawn from, and unless the investigator can show that his subjects represent people in the broader population he is restricted in his ability to generalize findings to that population.

My discussion of rival causal factors suggested that Becker's analysis of marijuana users was deficient because it did not carefully treat rival factors. Specifically I suggest that his study fails to report the situations of observation, lacks any concrete description of those observed, gives little description of the measuring instruments employed, and leaves unexamined the temporal features of observation. The additional problems of subject maturation, mortality (loss of subjects), and observer changes are also untreated. Becker attempted to resolve these problems through the use of analytic induction, which dictated the examination of every deviant case. My position is that while analytic induction provides the proper perspective on causal analysis, it is deficient in its ability to treat rival causal factors. Hence, I conclude that Becker's study *may* offer universal propositions concerning marijuana use but we have no clear way of disproving his theory.

My discussion of theory verification versus theory generation draws on Glaser and Strauss (1967) but is heavily influenced by Herbert Blumer (1969), who suggests that sociologists have displaced the goals of their discipline by concerning themselves with methodological refinements at the expense of developing strategies for approaching and interpreting empirical reality. A view that favors verification may be found in Zetterberg (1965), who presents several strategies for testing existing theory. For other views of theory, consult Merton's (1967) discussion of middle-range and general social theory. I slighted middle-range theory in this chapter because I wanted to avoid confusion regarding formal theory. In Chapters 2 and 3 I treat other views of theory development and verification.

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