Some cautionary notes on sociological research: with special attention to survey studies *

Derek L. Phillips

This paper is written from the perspective of what has come to be known as „the sociology of sociology”, and offers some critical remarks about sociological research, especially survey studies. It is divided into several sections, dealing with (1) the dominance of the survey interview, and its relevance for the concerns of sociology; (2) explanation and prediction in sociological investigations; (3) the validity of questionnaire and interview data; (4) data-collection as a social process; (5) the consequences of bias and invalidity for data-collection in sociology; (6) problems of „research on research”; and (7) the sociologist as a human being. The intentions of the paper are to remind sociologists of the enormous difficulties involved in the study of other human beings, to argue that bias-free research is impossible, and to suggest that we pay more attention to our own experiences and treat what we already „know” as a problem.

Peter Berger has stated that the first wisdom of sociology is that „things are not what they seem” (1). This is certainly as true for the discipline of sociology itself as for the social world which sociologists take as their subject for study. In the past few years this has been recognized by an increasing number of sociologists who have begun to direct their attention to investigating and writing about what has come to be called „the sociology of sociology”. These writers have turned the sociological imagination upon sociology itself, examining, among other things, the infrastructure, the organization, the assumptions, and conduct of the sociological enterprise (2). By and large, however, they have focused their attention on sociological

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theorists and have tended to neglect questions concerning the research activities of sociologists.

Very little of a critical nature has been written about sociological research methods and techniques. The major work on these matters still remains Cicourel's book (3), although his concern with "methods and measurement" is considerably more abstract and theoretical than the views to be presented here.

It is my intention in this essay to offer some critical observations on issues and problems involving the collection of sociological data. From time to time, I will refer to previous publications of my own and utilize them when it seems appropriate. It is important to note that most of my discussion will refer to the practice of sociology in American society, both because I know it best and because I share with Gouldner the view that "American sociology today, is for all practical purposes, the model of Academic Sociology throughout the world" (4).

1. The Validity of Interview and Questionnaire Data

Although the exact extent of its domination is difficult to establish, it is clear that the survey interview (and the questionnaire) is the dominant data-collection procedure in sociological investigations (5). Despite the heavy use of this procedure, it is not at all clear how the data obtained are relevant to the professed concerns of most sociologists: action, interaction, social processes, institutions, and social structures. Survey investigators typically obtain reports of behavior and interaction from one individual, then from another, another, and so on. People are then grouped together on the basis of one or another variable and then discussed as if interaction, social processes, etc., had actually been observed. Thus the utilization of interviews and questionnaires in sociological research is often irrelevant and inappropriate to the topics being investigated. Even when they may be relevant, the results generally provide little in the way of explanatory and predictive power (6).

The above is coming to be acknowledged by more and more social scientists, but what is not so often acknowledged is the fact that the results obtained are frequently invalid and inaccurate as well. Since I have examined the evidence relevant to this assertion elsewhere (7), mention will be made here of only a few studies providing relevant data.

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a. Voting Studies. Parry and Crossly report that 23 per cent of the respondents on their study who said they had voted had actually not done so (8). Thirty per cent of those studied by Bell and Buchanan gave inaccurate replies to a question on voting; and Cahalan reports that 28 per cent of the respondents in his study exaggerated their vote in a Denver mayorality campaign (9).

b. Health Studies. In an investigation concerned with the accuracy of health information provided through interviews, Cannell and Fowler found numerous discrepancies between the reports people made to interviewers and the records of hospital: 58 per cent gave inaccurate reports concerning their length of stay in the hospital, 35 per cent were inaccurate with regard to diagnosis and 10 per cent were inaccurate in their reports as to whether or not surgery had been performed (10). And a recent study by Green concerned with the validity of the responses given and the actual behavior of respondents regarding the use of contraceptives, found that of all couples who had received contraceptives (according to clinic records) about one out of five husbands and one out of every three wives denied ever having obtained them (11).

c. Census Data. The United States Census serves as a source of data for a large number of sociological studies, but only rarely has there been evidence available as to accuracy of such data. An investigation by Hambright in 1969 reported on a study where a sample of death certificates was matched with the 1960 census reports, thus allowing for a comparison of response data for items asked on both records. Among the numerous inconsistencies were the following: even when people's ages were compared in terms of ten-year intervals, there was disagreement for 8.1 per cent of the white males and 11.4 per cent of white females, and 23.8 per cent for Negro males and 30.3 per cent for Negro females. Even larger in magnitude were the inconsistencies with respect to marital status. For example, the two sets of records failed to coincide in terms of the „single” status of 9.0 per cent of white respondents and 21.6 per cent of Negro respondents. With the category of „divorced”, the records were in disagreement with the reports of 26.5 per cent of the whites and 42.4 per cent of the Negroes (12). And a recent study by the Census Bureau itself has compared the occupational data obtained from respondents with that obtained from employers. Although only eight occupational groupings were used, 18 per cent of all persons classified by the 1960 census in a given occupation group really belonged in another one (13). Comment-
ing on these findings, Lebergott estimates that perhaps one-third of all persons were reported in the wrong occupation by the census (14).

d. **Social Desirability.** Although it is usually difficult to do so, people's verbal reports about such things as voting behavior, hospital utilization, age, and marital status, can be validated against outside criteria. But what about topics of sociological interest that cannot be validated in a similar fashion against some outside standard: for example, alienation, prejudice, work satisfaction, or mental health? Might they, too, not be subject to problems of invalidity?

In an attempt to answer that question, Kevin Clancy and I have been engaged in a series of investigations designed explicitly to deal with problems of bias and invalidity in survey studies (15). In our first study we found that, as was hypothesized, people's scores on a mental health inventory employed by numerous investigators were related to their assessments of the "trait desirability" of the symptoms constituting that inventory. Further, we found an inverse relationship between people's location in the social class hierarchy and their views as to the desirability of the items (16). In other words, trait desirability was related to both their social class position and their mental health scores — thus constituting a systematic bias in the relationship between social class position and mental health scores. Higher status persons saw the traits as undesirable and, in our opinion because of this, denied having them, while lower status persons saw them as (relatively) desirable and (more often) reported having such symptoms of mental illness. In our view, the frequently reported inverse relationship between social class position and mental illness results from the effects of a social desirability bias operating in survey studies.

In our second study we examined the effects of trait desirability on the relationship between people's sexual status (being male or female) and their reports of such things as happiness, religiosity, number of friends, prejudice, and frequency of doctor visits. Our most general finding in that study was that both the magnitude and the direction of the relationships between sexual status and the various measures were affected by the introduction of judgments of trait desirability into the analysis (17). That is, people's assessments as to the desirability of the items which they were asked about did affect the relationships between being male or female and people's responses to the various items. Furthermore, the independent effects of trait desirability on people's responses were considerably stronger than the independent effects of sexual status. This finding is the same as what we
found in our earlier study (18), where more of the total variance in respondents’ mental health scores was accounted for by the biasing factor of trait desirability than by social class status.

What our studies show, then, is that people’s willingness to report or admit certain feelings or information about themselves is affected by one biasing factor: their assessments as to the social desirability of such feelings or information. If people see happiness as something that is highly desirable, for instance, they are much more likely to report being very happy than if they view happiness as less desirable. This means that the validity of certain sociological measures is called into question, and so is the validity of the findings involving the measures. In fact, there seems to be good reason to concur with Hauser’s statement that: „It is at least a moot question as to whether, up to this point in the history of survey results, more misinformation than information has been gathered on many subjects” (19).

There is considerable evidence to support Hauser’s view. Yet most sociologists have chosen to ignore it, choosing instead to devote their efforts to collecting more data, to concentrating on problems of data-analysis, and to an ever-increasing infatuation with mathematics and statistics. Given the demonstrably faulty quality of our data, these efforts are a monstrous waste of time and effort.

2. Data-Collection as a Social Process

I have argued elsewhere that one of the major reasons that sociological researchers have been so unsuccessful in explaining any sizeable amount of variance, and one of the reasons that their measures and findings are often invalid, is that investigators fail to recognize that the collection of social scientific data constitutes a social process (20). Their failure to acknowledge this fact is somewhat surprising. For if as most sociologists assume, people’s behavior is partially a product of their needs, values, and expectations, it should not be unexpected that their behavior (responses) in interview situations will also be affected by these factors. The theoretical writings of Cicourel and Riecken and the substantive results of investigations by Rosenthal, Friedman and several other social psychologists all indicate that in social science research — as in many other social activities — people tend to organize their behavior in light of what they feel the „others” (interviewers, observers, laboratory experimenters) will expect is appropriate for someone like them in that kind of situation (21). Thus, Riecken speaks of the
subject's desire to "put his best foot forward" (22). And Rosenberg writes of "evaluation apprehension" which he defines as "an active, anxiety-toned concern that he (the laboratory subject) win a positive evaluation from the experimenter, or at least that he provide no grounds for a negative one" (23).

Even in the most carefully controlled of laboratory experiments, psychologists have been generally unsuccessful in eliminating the influence of such tendencies as those mentioned above. Clancy and I have been equally unsuccessful in our research. For despite the fact that we followed the usual precautions (assuring anonymity, stressing that there were no right or wrong answers, etc.) for eliminating the possible effects of the desirability bias, our respondents were very much affected by considerations of social desirability. What these results indicate is that taking the usual "precautions" (i.e. following the textbook recipes) will not eliminate the influence of people's desire to place themselves in a favorable light on many measures of interest to sociological investigators.

Let me now review briefly the argument of my earlier studies with Clancy, and also attempt to make explicit certain assumptions contained in our research. Our most basic assumption is that in all social science investigations differences among individuals on various measures reflect some mixture of: (1) "true" differences in the characteristic (attitude, quality, or whatever) which the investigator is attempting to measure, and (2) variations due to errors in the measurement process. It seems to me that there is a great deal of evidence showing this to be the case and I will, therefore, not defend that assumption here (24). A second assumption is that whenever an investigator (interviewer, experimenter, observer) confronts the objects of his research interest, these individuals' responses and behavior will be affected by considerations of what is "appropriate" for someone in that kind of situation. That is, we assume that in social science investigations people will tend to organize their behavior and responses in light of their "definitions of the situation" — as do people, of course, in other social situations. Again, the evidence is, I think, quite persuasive (25).

More questionable, however, are our assumptions regarding an individual's ability to figure out the investigator's intentions with regard to the study; his ability to distort his "true" beliefs, feelings, behavior, etc., and his motivations and intent for engaging in such distortions. We believe that with most self-report measures, the instrument's purposes are apparent to the individual who responds to an interviewer's questioning or fills out a questionnaire. He knows (or thinks he knows, or at least has a hypothesis about) that what is of concern to the investigator is his mental health, sexual
behavior, job satisfaction, religiosity, or whatever. He does not, we assert, merely answer questions, endorse statements, or provide data without considering the purpose of the investigation. Rosenberg who has studied what he calls „evaluation apprehension”, in psychology experiments, reports that: „Subjects will report — sometimes with uncertainty and sometimes with great clarity — that they were burdened or preoccupied with the question „What is the real purpose of this experiment?“ ” (26). Although I know of no comparable findings from interview and questionnaire studies, I think it likely that people are almost always concerned with the purpose of the investigation.

It is also my belief that with most self-report measures, indeed with most sociological measures, the implications of people's answers are rather apparent to them and that they can, and will, consciously control their responses. Again, Rosenberg's research is recommended as evidence of this, although I disagree with his tentative conclusion that „subjects will usually obscure from themselves the extent to which they regulate their responding so as to win favorable judgments from the experimenter“ (27). Rather, it is my view that the subjects are more likely to obscure from the investigator rather than from themselves. In short, I believe that — in most instances — people have an idea (accurate or not) as to what is being evaluated or measured and what is the „right”, „correct”, or „best” response to give under the circumstances, and that this will color their responses.

3. Consequences for Data-Collection (28)

What, then, are the consequences for the ideals of social scientific research of viewing the data-collection process as a social process? This question will be considered in regard to the following ideals of sociological research: controlled data-collection procedures, the replication of procedures, and the cumulation of results. In discussing these three ideals my concern will be mainly with survey studies and laboratory experiments, although the implications for other research techniques should be apparent.

a. Controlled Data-Collection. The principal methods of data-collection and analysis in survey studies and laboratory experiments have as their model the controlled experiment as found in the natural sciences. In the laboratory experiment, an „experimental” group is exposed to the independent variable of chief interest while the „control” group is not; the two
groups are then compared in terms of the effects on some specified dependent variable. Randomization in the assignment of subjects to the two groups controls the effects of confounding variables. With studies using interviews and questionnaires, however, the control of confounding or extraneous variables usually comes about in one of two ways: (1) the sample is selected in such a way that certain factors do not operate as variables; e.g., only men might be included in the sample, or only persons under age forty; (2) in the analysis of data, certain variables are „held constant”; e.g., the relationship between religious affiliation and voting preference might be examined within each of several social class divisions.

In the types of controlled empirical inquiries described above, the ideal is that no variables other than those of explicit theoretical and empirical interest will affect the results. This means that there should be no „uncontrolled” influences by other subjects or respondent variables, situational factors, or attributes of the data-collector. Obviously, perfect control, in the sense described here, is impossible. But what is important is that unintended variations in the data-collection procedure be of minor magnitude and that when unintended variations do occur, they not be systematic.

In some of the studies concerning bias and invalidity variations in the responses and behaviors of those being studied are not of minor magnitude, and are related to the investigator's attributes and to such other factors as „social desirability” in a systematic manner — thus affecting the validity of the results (29).

These studies suggest quite clearly, I think, that much (in my view, most) social science research falls considerably short of the ideal of controlled procedures of data-collection.

b. Replication of Procedures. Since we do not have controlled data-collection procedures, it is extremely difficult to achieve the goal of replication. While the social scientist may make explicit a description of such things as the content of the interview, the exact words that were read from the instructions by the experimenter, the way in which the sample was drawn, and the data analyzed, this does not constitute replication. For if another investigator were to try to replicate the first man's data-collection procedures, he would, among other things, have to obtain interviewers (in the case of survey studies) who possessed the same relevant qualities (in the same combinations or mixes regarding their age, sex, race, expectancies, etc.) as those who did the original interviewing. If the intent of replication is not to vary anything which makes a systematic difference in people's responses,
then replication in survey and experimental studies is much more difficult than is usually acknowledged. In fact, contrary to what is usually assumed, it may be more difficult to replicate a survey study than a study based on participant-observation in that conducting interviews with, say, 1000 respondents actually involves 1000 (or 999) replications of the same investigation (30).

Friedman's remarks on the replication of psychological experiments are relevant here. He notes that "psychologists do not, as a rule, report how many glances they exchanged with their subjects while reading the instructions, nor would they have any way of knowing if they decided to. Nor do they, as a rule, report the exact duration of each phase of each experimental session. Hence, working from the public description of the experiment, it is unlikely that another experimenter would replicate the exchange of glances variable or the time variable" (31). Should it be the case (as Friedman found) that the experimenter's glances are relevant to the subjects' responses and behavior, the failure to include these would constitute a significant omission in a replication study. When the data-collection procedures (including all response-relevant variables in the context of the investigation) are not controlled and not reported, they are not replicable.

c. Cumulation of Results. What can be said, then, about the third ideal: the cumulation of results? Obviously, studies which are based on the utilization of procedures which are neither controlled nor replicable will not allow for empirical results that can in some way be "added up" or built upon. The assumption by many social scientists that they can engage in research without influencing what they obtain in the way of data is without foundation. Despite the claims of many that our data-collecting is objective, our procedures public, our findings replicable and cumulative, the evidence indicates the contrary.

4. Some Thoughts Concerning "Research on Research"

I have described above the results of my research with Clancy concerning social desirability as a source of bias and invalidity in survey studies. They are part of an increasing, but still small, number of investigations that have examined the effects of various factors operating in the collection of social science data. There are studies of "modeling effects" (32), "experimenter expectancy" (33), and of various attributes of the experimenter and inter-
viewer on the responses and behavior of people in social science investigations. Among these are investigations showing the effects of their race (34), sex (35), age (36), religion (37), social status (38), and various personality characteristics (39), on the subjects of their research.

By and large, studies which have concerned themselves with the effects of various factors as sources of bias and invalidity have not reported the proportion of variances accounted for by these factors. Rosenthal, however, states that expectancy effects (the effects of experimenter expectancies on subjects' performances in laboratory research) account for at least as much variance in subjects' performances as do the effects of the main experimental variables (40). And Clancy and I found that the factor of „social desirability“ accounted for more variance than the variables of social class position and sexual status (41). Although I know of no relevant studies, I would hazard a guess that if we were to examine the combined effects of the numerous sources of bias that operate in various sociological and psychological studies we would find that they account for considerably more of the variance in the dependent variables of interest than do the major independent variables that are of theoretical and empirical interest.

Although sociologists have paid very little attention to problems of bias and invalidity in their research, those psychologists who have been most concerned with these problems have, in my opinion, been overly-optimistic with regard to eliminating bias. For example, Rosenberg has stated that „every investigation in this realm profits the succeeding one; error should fall away as we continue to „zero-in“ toward the goal of bias-free research” (42). In my view, most of the studies concerning the existence of bias in social science investigations are themselves subject to possible biasing influences. Consider, for example, Rosenberg’s studies of evaluation apprehension where he establishes the existence of such a biasing factor through the utilization of post-experiment interviews (43). That is, after the experiment the subjects are interviewed and many report that they were concerned with trying to figure out what the purposes of the experiment were. Rosenberg accepts these post-experimental interviews as valid reports and fails to acknowledge that this, too, is a kind of evaluation apprehension. For even after the completion of the experiment itself, the subject may feel that he is being „evaluated“ through his responses to the interviewer’s questions. Consequently, he may report that he was concerned, because that is what he believes a „good subject“ should be. On what basis can we assume that an individual’s laboratory behavior will be affected by bias while his interview behavior will not be affected?
This is not to say that laboratory researchers are totally ignorant of this problem, for they definitely are not. Orne, for example, states that "the postexperimental interview must be conducted with considerable tact and skill, creating a situation where the subject is able to communicate freely what he truly believes . . ." (44). This, of course, is also the goal of the survey interview. It is, as I have tried to indicate, a considerable problem. For statements like that of Orne's presume the kind of knowledge that our investigations are designed to discover.

My research with Clancy, where we attempted to determine the influence of "social desirability" as a biasing factor in survey studies is subject to a similar criticism. In our investigations we employed a procedure resembling that developed by Edwards and employed previously by Dohrenwend (45). We asked each of the respondents to rate each of the items (in one study, the twenty-two items comprising a mental health inventory; in the other, such items as being "religious", "non-prejudiced", etc.) on a nine-point desirability scale. We then accepted these ratings as accurate reflections of people's perceptions of the desirability of the various items, and argued that these "accurate" ratings were responsible for people giving "inaccurate" responses to the questions about mental health symptoms, religiosity, prejudice, etc.

But, clearly, there are other possibilities. Both the desirability ratings and the responses to the various questions may be inaccurate reflections of people's "true" feelings, both may be accurate reflections, or the desirability ratings may be inaccurate while the responses to the questions are accurate. We discussed these possibilities in our earlier papers and argued that there were good theoretical reasons for preferring our line of reasoning (that the measures of social desirability were accurate, and the other responses biased by social desirability) to those implied in the alternate possibilities. The point, however, is that it is difficult to demonstrate empirically the correctness of our position. This is a result of the fact that our attempts to discover sources of bias and invalidity in survey studies are, logically, subject to the same weaknesses as the measures whose validity we have called into question.

It seems to me that there are two principle reasons for doing "research on research", for attempting to discover the effects of various aspects of the data-collection process on the validity of social scientists' measures and, consequently, on the validity of their results. The first of these is to discover the sources of error and invalidity so that they can be controlled or eliminated. This clearly is the goal of most investigators who have studied bias, as is shown by the statement of writers like Rosenberg who speak of "bias-
free" research. The second, which is now my position (but not that of my collaborator, Kevin Clancy), is to demonstrate the inadequacy of contemporary social science research and the impossibility of bias-free research in situations where human beings collect data from active, thinking people like themselves. For to continue to conceive of people (respondents, subjects) as "objects" who can be subjected to the control of the researcher is, in a sense, to conceive of them as "things". I think Gouldner expresses this point clearly when he reminds us that "There is not as great a difference between the sociologist and those he studies as the sociologist seems to think, even with respect to an intellectual interest in knowing social worlds. Those being studied are also avid students of human relations; they too have their social theories and conduct their investigations" (46).

5. The Sociologist as Human Being

It is my view that there is a dominant paradigm in sociology that defines, to a certain extent, the manner in which a sociologist looks at the social world and conducts his research. But this is not to suggest that the sociologist is influenced only by the dictates of the scientific community. It seems clear, for instance, that the sociologist's own values, interests, preferences, skills, personal troubles, and biases always intrude to some extent in the selection of the best procedures for collecting data pertaining to his topic of investigation. It is inevitable that sociologists will choose not only problems that concern them but will also select investigative procedures that will maximize the possibility of their finding what they are looking for or in having their hypotheses supported. For surely (as Gouldner, among others, has so clearly stated), sociology is by no stretch of the imagination "value-free" (47). That is, not only do an investigator's values influence the problems that he selects for study but also his methods for studying them and the sources of data which he employs.

By this time I think it is widely recognized that all social scientists select for study the problems that interest them. No one should have ever expected it to be otherwise. But there is less recognition that their methods and sources of data are often chosen on other than the strictest of scientific grounds, although this is coming to be widely accepted by younger social scientists. For example, the historian Staughton Lynd comments on his own early work as follows:

On the one hand it still makes sense to me that, like any other social
scientist, the historian should formulate hypotheses and then test them against a restricted range of data, such as what happened in one area, or in one man’s life. On the other hand I am now more conscious that I selected a range of data that I could be relatively certain would substantiate a thesis that I hoped was true. I studied opposition to the United States Constitution in Dutchess County, New York, because Dutchess County has a history of landlord-tenant conflict very likely to be connected with how groups aligned themselves for or against the ratification of the Constitution. The bias involved in my selection of Dutchess County did not necessarily invalidate my findings, but it raised serious questions as to their general ability. I believe this is how bias characteristically operates in the work of other historians, too: not in deliberate mishandling of evidence, but in selection of research design (48).

I suspect that what Lynd indicates about data utilized to test his own thesis is equally true for the vast majority of social science investigations. But, as I noted earlier, this is coming to be acknowledged by ever-increasing numbers of social scientists.

However, there is another aspect of the investigator’s influence that remains widely ignored and, in fact, has only recently begun to be given the attention which it deserves. I refer here to the investigator’s implicit theories of behavior of men, and assumptions that guide, and, to a large extent, probably determine the outcome of his research activities.

Although many seem reluctant to admit it, sociologists — like all other men — view the behavior of themselves and others in terms of certain assumptions about men, about society, and about men in interaction with one another. This means that social researchers will organize their research and their writings in terms of such prior assumptions. It could not, of course, be otherwise. For like those whom he studies and writes about, the sociologist is influenced by his own experiences. Some sociologists undoubtedly will argue that they make no assumptions about men and society, or that their assumptions are „supported by empirical evidence”. In my view, this is nonsense. Whether we see men as self-centered or altruistic, whether we see them as possessing „free-will” or not, and what we see as their basic „needs”, for example, are not determined by reference to principles of social behavior and interaction. The reason for this reflects a fundamental problem in sociology; what we know about social behavior (and, indeed, most social phenomena) is dependent on our methods for studying it, while our methods for studying it are dependent upon what we know about social behavior.
This suggests, then, that in order to know more about social behavior and interaction, we need better methods; and to obtain better methods, we need to know more about behavior and interaction. This constitutes a kind of vicious circle which we must break out of if the social sciences are to move beyond their present stage of development. I will return to this point shortly, and discuss it further.

Clearly, the fact that we have neither adequate methodologies nor basic principles of social behavior has not prevented sociologists from theorizing about social phenomena nor from engaging in empirical research. One reason for this is that all sociological investigators are guided in their work by some view of men, both with regard to their nature and to the ways they operate as social actors. As I noted, these views are seldom made explicit — although they can easily be seen in the myriad of „interpretations” which different sociologists offer in an attempt to „make sense” of the results of various studies. Given the task of offering an explanation for some pattern of association between two variables, it is likely that there would be little agreement among the explanations which different sociologists would put forth.

This being the case, it is not at all surprising that there should be great differences among sociologists in the factors chosen to explain various social phenomena. Some of these differences are due, of course, to different orientations among different sub-areas of sociology, which are often concerned with different concrete subject matter. Even when the phenomena to be explained are similar, sociological practitioners will select different causal or determining factors. This is, perhaps, to be expected in that the various sub-areas have a long history of different concerns, emphases, and traditions. But even within a particular sub-specialty, there are a tremendous number of factors chosen to explain the phenomena of interest. In sociological research, the range of possible „variables” is so great — including both structural and social psychological variables — that investigators can choose from a seemingly infinite universe of possibilities. And since there are no formal scientific criteria to govern the selection of some rather than others, the choice is often made on the basis of the individual investigator’s own feelings, hunches, preferences, or whatever. In short, his own experience will dictate what he chooses as the variables of empirical interest — although he may pay „lip-service” to the literature, and to various models and theoretical perspectives.

I would like now to return to a point that I made earlier. My reference is to the kind of „vicious circle” quality of the relationship of interdepen-
dence that exists between theory and methods: in order to know more about social behavior and interaction, we need better methods and data; while to acquire better methods and data, we need to know more about behavior and interaction. So, for example, to eliminate error from our research, we require certain kinds of knowledge about behavior and interaction that will tell us where and why errors may arise. In short, we require agreed-upon principles of social behavior and interaction. At present, these principles are lacking. The objective of utilizing various methods in doing research is to discover such principles. But research is a social activity and, therefore, is affected by the very principles which we have not yet discovered or are not yet agreed upon. And so it goes, in a vicious circle.

I have puzzled over this dilemma at length, in an attempt to make some judgement as to where our priority lies — with theory or with methods and research. My conclusion is neither the pluralistic view that we need both, nor Coleman's view that „innovations in methods are urgently required” (49) but, rather, that in most sciences the major changes and advances come, not through new research technique or new data, but from new ways of looking at existing data (50).

Thus the answer — if there is an answer — lies with new theories and new conceptual schemes, with radical conceptual breaks or discontinuities. But in reaching this conclusion I have reformulated both the notion of „data” that I have used throughout this paper, and the idea of „theory” as it is usually thought of in sociology. With regard to data, I do not mean that we must look more closely at only (or necessarily) the kinds of data that are purposively collected in empirical investigations employing interviews and questionnaires, or other research „techniques”. Rather, I mean that we must try to look at the world through our own eyes and not through our „scientific” instruments that frequently cloud our vision or blind us entirely. By theory I do not mean the activities engaged in by writers like Parsons, Merton, Homans or Zetterberg, but rather, the primordial conception of theorizing that originated with the pre-Socratics: the idea of theorizing as a self-conscious and reflexive activity (51). In a very profound sense, we must take ourselves and our own experiences more seriously than we do at present. But we must go further than this. We must become more self-conscious about the ways in which we organize and use our ideas. We must consider what we „know” and treat that as a problem (52).

Starting from a somewhat different vantage point, Gouldner has reached a similar conclusion (53). And although he has expressed his view with
clarity and passion, there are those who ask to be shown "how". What they are asking for, I believe, is a set of directions as to how to think and how to live their lives. Alas, there are no ready answers to such questions. But sociologists who fail to consider the questions are clearly not alive to life. Perhaps they are asleep. If so, it is our obligation to awaken them.

Notes


22. Riecken, op. cit.
24. See, for example, Cannell and Fowler, op. cit; Friedman, op. cit; Phillips, op. cit; Rosenberg, op. cit; Rosenthal, op. cit.
27. Ibid, p. 19.
28. The discussion in this section is taken from Phillips, op. cit.
29. See Friedman, op. cit; Phillips, op. cit.; Phillips and Clancy, op. cit.; Gene F. Summers and Andre D. Hammonds, "Effects of Racial Characteristics of Investigators on Self-Enumerated Responses to a Negro Prejudice Scale", Social Forces, 44, 1966; A. Binder, D. McConnell, and Nancy A. Sjoholm, "Verbal Condition as a Function of Experimenter Characteristics", Journal of Abnormal and Social Psychology, 55, 1957; Mark Benney, David Riesman, and Shirley A. Star, "Age and Sex in the Interview", American Journal of Sociology, 62, 1956. Although I believed at one time (as in Phillips, op. cit.) that the kinds of results provided by the above studies could be informative and useful in their own right as evidence of the factors operating in human interaction, I now feel that the findings cannot be generalized beyond the studies in which they have been discussed.
30. For further evidence of this, see Phillips, op. cit.
31. Friedman, op. cit. p. 150.
33. Rosenthal, _op. cit._
35. Binder _et al._, _op. cit._
36. Benney _et al._, _op. cit._
42. Rosenberg, _op. cit._ p. 348.
43. Ibid.
44. Martin T. Orne, „Demand Characteristics and the Concept of Quasi-Controls“, in Rosenthal and Rosnow, _op. cit._ p. 154.
46. Gouldner, _op. cit._ p. 496.
52. This may appear vague and cryptic in the extreme. I will deal more fully with these matters in future papers.