Three

Formulating Research Problems

Research problems are questions that indicate gaps in the scope or the certainty of our knowledge. They point either to problematic phenomena, observed events that are puzzling in terms of our currently accepted ideas, or to problematic theories, current ideas that are challenged by new hypotheses. This chapter first looks at the role of such questions in the research process, and especially the ongoing debate among social scientists as to when and how problems should be formulated. Second, we consider methodology's effect on defining problems, and how the multimethod approach can be used to focus research more sharply upon the substance of research problems. Finally, we consider the role of theory in problem formulation, and how the multimethod approach integrates theory and research more closely in posing these research questions.

The Role of Research Problems in the Research Process

The problems of everyday life are difficulties to be avoided, if possible. Research problems are eagerly sought after. The difference is that research problems represent opportunities as well as trouble spots. Because

scientific knowledge is provisional, all empirical findings and theories are in principle problematic and are, therefore, subject to further investigation. But in addition to seeking more exact confirmations of existing claims to knowledge, research has the equally important goal of generating new claims. Problem formulation is the logical first step toward this goal. As Northrop (1966) writes, "Inquiry starts only when something is unsatisfactory, when traditional beliefs are inadequate or in question, when the facts necessary to resolve one's uncertainties are not known, when the likely relevant hypotheses are not even imagined. What one has at the beginning of inquiry is merely the problem" (p. 17).

The formulation of research problems also has an important social function. As Merton, Broom, and Cottrell (1959) suggest, researchers must justify the demands for attention and other scarce resources that research makes: "In conferring upon the scientist the right to claim that a question deserves the concerted attention of others as well as himself, the social institution of science exacts the obligation that he justify the claim" (p. xix). Achieving significant research results is perhaps the most powerful justification for such claims, but this type of justification can be offered only after the fact, and only in the event that the research is successful. A compelling research problem, by contrast, must marshal support in advance of research and, if it is sufficiently compelling, can even sustain that support through the sometimes fruitless periods that researchers experience.

However, despite research problems' logical priority in inquiry, and their importance as a priori justifications, a problem's formulation, as John Dewey stresses, is in fact a "progressive" matter. Dewey means that problem formulations are themselves problematic and so require continual attention to assure that the questions being asked will direct research toward the desired end: "If we assume, prematurely, that the problem involved is definite and clear, subsequent inquiry proceeds on the wrong track. Hence the question arises; How is the formation of a genuine problem so controlled that further inquiries will move toward a solution?" (quoted by Northrop, 1966, p. 13).

When and How to Formulate Problems: A Debate

It sometimes seems that there is little about which social scientists agree, and the most effective procedure for formulating research problems is no exception. In particular, there has been considerable debate over whether or not it is important to define problems explicitly in

advance of research and to show how they are linked to prior work. Many social scientists hold that research problems should be formulated by carefully analyzing as much of the relevant research literature as possible, formally stating the problem and the major hypotheses that the literature suggests, and only then collecting the data. Their intention is to give research a clear and firm justification and to encourage hypothesis testing. This will ensure that each new study does its utmost to add in an orderly fashion to the sum of knowledge. However, there are many other social scientists who are equally convinced that this style of formulating problems tends to stifle questions and prevent discoveries that a more open-ended approach might stimulate.

This latter group argues instead for letting problems and hypotheses emerge throughout the research process, pushed forth by new empirical observations that encourage the researcher to ask new questions and build new theories. For example, Schatzman and Strauss (1973) write:

> The automatic use of formally stated hypotheses, and of statements of "the problem" may make it easier to program action, but it will also limit the kinds of experience that he (the researcher) will tolerate and deal with. In original research there is less likely to be a conceptual closure to inquiry, for as the work of discovery continues and new kinds of data are conceptualized, new problems and hypotheses will emerge. Consequently far from putting a closure on his new experience the researcher will modify his problem and hypotheses—if indeed he ever stated them explicitly—arrange to handle new ones simultaneously with the old, or do so in serial order. This is how the relationship between the observer and the observed object is altered, and how it becomes possible for new questions to be asked and answered through research. (pp. 12–13)

Stating the problem early and in a highly structured form may indeed lock the researcher into a fixed stance with respect to the situation being observed, and it may also block the emergence of new ideas that might be stimulated by new experience. But open-endedness may have costs as well. For instance, Huber (1973) argues that letting the emergent features of each new research situation continually exert pressure to redefine problems and hypotheses tends to bias the emerging theory in the direction of the status quo. It gives undue weight to the particular situation being studied at the moment, diverts attention from the problems posed by other theories, and interferes with theory-testing because the same data obviously cannot be used both to form and to test an hypothesis. In this

view, prestated problems and hypotheses do much more than make it "easier to program action" (as Schatzman and Strauss [1973] suggest). They discipline research in the interest of testing theory, accumulating knowledge, and achieving a theoretical standpoint independent of the time and place in which researchers presently find themselves.

Overcoming Methodological Constraints on Problem Formulation

Both sides in the foregoing debate clearly have merit. However, in practice the decision as to when and how research problems should be defined usually depends less upon the perceived merits of one or the other of these procedures than upon the research style selected. Methods differ in their abilities to predict the kinds, quantities, and quality of the data that may be available in any given instance. For example, survey researchers or experimentalists can usually say with more certainty than fieldworkers whether or not the data pertinent to a particular research problem can be readily collected. Fieldwork offers the possibility of many data sources, but it is usually hard to say in advance which data will actually be obtainable. Similarly, Selltiz, Jahoda, Deutsch, and Cook (1959) note the need to take a "wait-and-see" attitude in the use of nonreactive data sources such as statistical records: "The use of such data demands a capacity to ask many different questions related to a research problem. . . . The guiding principle for the use of available statistics consists in keeping oneself flexible with respect to the form in which the research questions are asked" (p. 318).

Furthermore, as we will discuss in greater detail in Chapter 4, an empirical search for problems is considerably less expensive with some methods than others. Exploratory experiments and surveys are certainly feasible, but pilot field studies and searches through archives generally cost less, except perhaps for the researcher whose personal expenditure of time and energy usually "fund" such studies. Moreover, discoveries arise in different ways for different methods. Fieldworkers and nonreactive researchers are more likely to make discoveries as a result of finding new data sources and examining new situations; while survey researchers and experimentalists are more likely to make discoveries through innovations in techniques of study design, sampling, or data analysis, which can generate unexpected (serendipitous) findings by more precise tests of hypotheses.

Different research styles thus exert different constraints on formulating problems: open-ended constraints in response to the immediate research situation for fieldwork and nonreactive research or more programmed constraints for surveys and experiments. The multimethod strategy provides the opportunity to overcome these methodological constraints upon problem formulation and thereby gain the advantages of each approach while compensating for its disadvantages.

Sieber (1973), for example, notes Stinchcombe's (1964) reliance upon about six months of fieldwork among the teachers and administrators in a high school to formulate the hypotheses that guided Stinchcombe's analysis of survey data from the same school. Sieber (1973) concludes that "an optimal schedule for theoretical survey research would include a lengthy period of fieldwork prior to the survey" (p. 1346). He further observes that although he could find in the literature few other examples of this practice of deriving a survey's guiding theory from fieldwork, it may be quite common, since "Often, only passing acknowledgment is made of prior personal familiarity with the situation, a familiarity that has produced rather definite ideas for research (p. 1345). Sieber (1973) cites, for instance, Lipset's (1964) autobiographical account of how the childhood experience of his father's membership in the International Typographical Union, along with the classic works of Robert Michels and Alexis de Tocqueville, influenced the research problem that Lipset and his colleagues formulated and tested in the classic survey study, *Union Democracy* (1956). If, as Dewey suggested, the correct formulation of research problems is crucial to their solution, then it is critical that no source of potentially valid information—no matter how "unscientific" it may seem—be ignored.

Furthermore, Sieber (1973) demonstrates how despite "an historical antagonism between proponents of qualitative fieldwork and survey research," integration between these two research styles has been achieved in numerous studies (p. 1335). He shows how fieldwork has been employed to define the theoretical structure of problems later studied in surveys, to define and gain greater knowledge of the problem-relevant populations for surveys, and to reformulate problems by aiding in the interpretation of surprising survey findings and statistical relationships between variables. He likewise shows how surveys have been used to define and pinpoint relevant cases for fieldwork, to verify and establish the generality of field observations, and to cast new light on "hitherto inexplicable or misinterpreted" observations.

Generating Versus Verifying Theories

The issue of when and how to formulate research problems is closely related to another issue: the relative importance of generating new theories versus the verification of existing theories. Both building and testing theories empirically, as Chapter 2 explained, are important research activities, but they serve very different functions in scientific inquiry. Since at least the 1960s, the appropriate balance between these two aspects of research has provoked considerable controversy in the social sciences.

For example, Glaser and Strauss, writing about sociology in 1967, observe: "Verification is the keynote of current sociology. Some three decades ago, it was felt that we had plenty of theories but few confirmations of them—a position made very feasible by the greatly increased sophistication of quantitative methods. As this shift in emphasis took hold, the discovery of new theories became slighted and, at some universities, virtually neglected" (p. 10). Glaser and Strauss (1967) argue that the emphasis on verification of existing theories kept researchers from investigating new problem areas; prevented them from acknowledging the necessarily exploratory nature of much of their work, encouraged instead the inappropriate use of verificational logic and rhetoric; and discouraged the development and use of systematic empirical procedures for generating as well as testing theories. To compensate for the overemphasis upon verification, Glaser and Strauss urged that research designed to build empirically "grounded" theories must be recognized as a legitimate social scientific pursuit independent of verification. They saw no necessary logical conflict between empirically building and testing theories. But they felt that the social and the psychological conflicts "reflecting the opposition between a desire to *generate* theory and a trained need to *verify* it" (p. 2) were so strong that clear designation of theory building as a proper research goal was essential: "when generating [theory] is not clearly recognized as the main goal of a given research, it can be quickly killed by the twin critiques of accurate evidence and verified hypotheses" (p. 28).

If we accept that generating theories empirically is not a substitute for empirical verification, then building theories without immediate regard for testing poses no special logical problems. However, it may complicate matters methodologically. One serious complication is that theories are often built empirically using research methods that are different from the methods required to verify them.

Each style of social research can be employed either to generate or to verify theories. But in fact, purely generational studies tend to rely more upon fieldwork or nonreactive data sources than upon experiments or surveys, and often more upon qualitative than upon quantitative observation and analysis. The transition from generational to verificational research may therefore involve a methodological shift as well as a change in the focus of problem formulation. As Chapter 2 suggested, studying a theory with different research methods provides an opportunity for fuller examination of that theory. However, employing a new or different method also creates difficulties. It may be far from obvious how, for instance, concepts and propositions developed through qualitative field studies may be measured and operationalized in terms suitable for quantitative surveys or experiments—or vice versa, how to design a field study to test a theory deriving from surveys or experiments. There may also be questions about the appropriateness of the new method to the theory's content, or about whether or not operational hypotheses that can be tested with that method do in fact adequately represent the theory and so provide a fair and full test.

Bernstein, Kelly, and Doyle (1977) encountered these kinds of difficulties in formulating and testing hypotheses derived from symbolic interactionist theories of deviance. These were theories that had been generated largely in qualitative field studies. Bernstein et al.'s strategy was to combine qualitative field observation with quantitative analysis of interviews and court records collected for a larger sample of criminal defenders. This multimethod approach, which is an example of the transition study described in Chapter 2, allowed them to use the fieldwork data to aid in both the design and the interpretation of the survey and archival segment of their study. The approach also permitted them to be open and sensitive to the kinds of firsthand field observations that had prompted the initial theories. They thereby retained descriptive realism without sacrificing either the quantitative precision required for verification or the generalizability provided by their larger sample.

The Empirical Unfolding of Research Problems

Once a study is published, it is in many ways irrelevant whether the research problem prompted the study or instead emerged from it. With publication, the study's problem enters the public domain and becomes

the responsibility not only of the study's author but of all who are professionally interested in that research area. At that point, the key issue is what to do with the problem next. Research into a problem does not end with a single study. Nor is there truly a final formulation of a problem any more than there is a final solution. All research, as Chapter 2 suggested, involves some simplification of the problem being investigated. This is unavoidable given the limitations on our resources, theories, and methods. However, each of a discipline's separate new studies, or each phase of study in an individual's research program, reveals new aspects of the problem by addressing issues (such as those raised by the "skeptic's questions" in Chapter 2) that earlier research could not address.

The two modes of formulating research problems that we have just discussed differ in that one looks to past studies, while the other looks to ongoing work. But the two are similar in that both rely upon empirical inquiry rather than upon nonempirical procedures, such as speculation or the purely logical analysis of ideas. This means that whether research problems emerge from current research or instead derive from earlier work, research methods are directly implicated in the process. Every empirically based research problem has a methodological as well as a substantive component, and this methodological component may equally influence our perceptions as to which particular phenomena and theories are problematic. One of the central questions to be posed, therefore, is how do the methods employed in research directly affect the formulation of research problems?

The Substantive Importance of Methodology

Deutscher (1966), for example, posed this question of methodological influence by revealing one of the major simplifications of social policy research conducted through the early 1960s. He noted the very heavy reliance upon survey research at that time, and suggested that this reliance upon surveys led social scientists to oversimplify research problems by assuming that verbal responses reflect behavioral tendencies. Deutscher observed that only by making this assumption were researchers, who were studying issues such as racial and ethnic discrimination, able to make causal inferences about behavior solely on the basis of questionnaire and interview data. However, he stressed that this assumption neglected a central problem that had begun to emerge from exploratory field studies as early as the 1930s: People's words and deeds frequently do not agree. To correct this oversimplification, Deutscher urged both that

this neglected problem of "attitude versus action" must be formulated more systematically and that a new research technology, a multimethod approach, must be developed to capture both attitudinal and behavioral aspects of policy problems.

The problem of attitude versus action is now a major topic of multimethod research. But when Deutscher addressed this problem in 1966, the topic was relatively unexplored. New areas of inquiry, where little is presumably yet known, promise productive research problems. However, the actual formulation of the problems may be more difficult than in more developed areas in which consistent bodies of empirical generalizations and theories have already been established. This became evident when Deutscher (1966) set about formulating the problem of attitude versus action:

> We still do not know much about the relationship between what people say and what they do—attitudes and behavior, sentiments and acts, verbalizations and interactions, words and deeds. *We know so little that I can't even find an adequate vocabulary to make the distinction!* Under what conditions do they say one thing and behave exactly the opposite? In spite of the fact that all of these combinations have been observed and reported few efforts have been made to order these observations. (p. 242)

As research into a problem proceeds with researchers posing it in different ways, the problem ideally (as Dewey implied) unfolds to reveal new dimensions that facilitate the problem's solution. The variety of available research methods is a key element in this process in that it provides researchers with a multifaceted empirical view of the phenomena and of the theories in question. This enables researchers to formulate problems in a manner that does greater justice both to the complexity of social phenomena and to the complex implications of our theories. For example, Chapter 1 demonstrated how the variety of methods now employed to measure crime led to a more discriminating conceptualization of the phenomenon of criminal deviance. And Chapter 2 illustrated how the employment of multiple methods allows researches to consider more fully a theory's empirical implications.

However, employing a variety of methods also complicates the process of problem formulation because different types of research methods very often provide conflicting answers to the same research questions. For example, Deutscher (1966) found the problem of attitude versus action to be complicated by the fact that experimental studies generally reported

greater consistency between subjects' words and deeds than did observational field studies. When such methodologically linked contradictions appear in the course of a problem's development, the suspicion is that they may derive from theoretically irrelevant characteristics of the different methods employed rather than from the substantive complexity of the problem. Inconsistent findings require reformulations of research problems. When these inconsistencies reflect unanticipated substantive complexity, then concepts and propositions must be recast to take account of that complexity. But although more complicated theories are sometimes necessary to achieve theoretical realism, simplicity is preferable. And if, in fact, contradictory research findings are attributable to methodological influences and can be shown to be consistent with existing theories, once those influences have been taken into account, so much the better.

The substance of social life is certainly diverse enough to generate inconsistent findings, but the methods of social research are also diverse. Only by analyzing the methods employed to obtain research findings can it be determined which source of inconsistency any given set of findings reflects. For example, Hovland (1959) observed that textbooks summarizing the effects of communication on opinion-change in the 1950s often reported substantive contradictions in research findings without regard to differences in methodology, despite the fact that stronger effects were generally found in experiments than in surveys. However, Hovland found that upon closer inspection these apparent contradictions might be explained in terms of the idiosyncrasies of these two different types of methods and might not require new theoretical explanations. In sum, although the exclusive use of a single type of research method can oversimplify research problems, the use of different types of research methods, without systematic comparisons of their results and an understanding of possible methodological influences, can make problems appear to be more complex—or complex in different ways—than they really are.

Research Questions Stemming from Multimethod Research

Multimethod research can help in sorting out substantive and methodological issues. But not even this approach can provide totally method-free results. No research style can do that; *what* we know is always shaped in part by *how* we came to know it. Multimethod studies may be expected, therefore, to spawn as well as to aid in answering research questions. A major problem is how to proceed with inquiry once it has been discovered

that two or more methods' findings diverge. As we said in Chapter 1, convergent results from different methods increase confidence in each method, but contradictory results call for reanalysis of the methods, both in relation to one another and in relation to the original research problem. (We shall see in Chapters 6 and 7 that under some circumstances convergent findings also raise questions, but it is best to examine one source of problems at a time.)

When contrasting different methods' results, there are two general classes of potential research questions that emerge in particular. The first is whether quite different styles of research really study the same phenomenon in anything but name. The second is whether different variants of the same research style will yield the same results. Let us consider the types of potential research problems that emerge from multimethod research in connection with these two issues.

In Chapter 1, we saw that crime data from official statistics and from criminal victimization surveys might measure quite different aspects of criminal deviance. Hindelang, Hirschi, and Weis (1979) have considered still another survey method of measuring crime (the self-report method), which has also often been found to give results different from those obtained with official statistics. They find that many of the apparent discrepancies may stem from a failure to recognize that these two methods may tap quite different domains of crime (trivial versus major crimes), which may have quite different social correlates.

The self-report method of measuring crime, most commonly used to measure juvenile delinquency, calls upon respondents to report their own offenses rather than offenses committed against them, as in criminal victimization surveys. The method is essentially a technique for estimating delinquency in nondelinquent populations; that is, among juveniles not officially labeled as deviant by arrest or conviction. It provides data to study possible correlates of self-reported delinquent behavior, irrespective of whether or not that behavior was previously identified in official data sources, which are often suspected of measuring official action more nearly than deviance. With self-report data it is possible to estimate how (if at all) social factors such as gender, race, or social class are related to delinquent behavior, when the possibly contaminating influences of official detection and recording practices are eliminated.

Self-report studies, like criminal victimization surveys, were intended to resolve the crime-measurement problem. But the result was quite different. Instead of confirming the findings of earlier methods, the self-report

studies often produced divergent findings. For example, Tittle, Villemez, and Smith (1978) report that in 35 studies of the relationship between social class and crime, those studies conducted before 1964 (using self-report data) consistently show no relationship, while those using official data show a negative relationship; and that in studies done after 1964 no relationship between class and crime was found in either type of study. These findings led Tittle et al. (1978) to conclude that the often assumed relationship between social class and crime was a myth, probably based upon the tendency for police data to overreport the crimes of lower class offenders.

However, Hindelang et al. (1979) argue that the discrepancies between the two types of studies may themselves be an illusion, largely reflecting the tendency for self-report measures to include many more minor offenses than do measures of delinquency based upon official statistics—trivial offenses that may in principle be chargeable but in fact are "almost by definition outside the domain of behavior that elicits official attention" (p. 996). Their argument underscores the point, made earlier with respect to uncoordinated single-method studies, that methodological diversity may create an impression of contradiction and inconsistency where none exists. But their argument suggests a second equally important point: Special care must also be taken in designing multimethod investigations to ensure that convergence between different methods' findings will be evident if it is present, not masked by incomparable data.

To bolster confidence that convergent findings are not the result of the methods' shared biases, the multimethod strategy calls for methods whose weaknesses differ. But to make convergence as evident and as likely as possible, the multimethod strategy also calls for methods whose strengths coincide in relation to the research problem. If methods fail to complement one another, then divergent findings have problematic significance, as Hindelang et al. (1979) suggest in the case of self-report studies. "Regardless of how often it is said that self-reports measure primarily trivial offenses . . . it is easy to forget that they do. Self-report offenses are routinely treated as equivalent to official offenses in comparing correlates of delinquency. . . . When the results using the two criteria are inconsistent, it seems to follow that one or both measurement procedures is faulty. An alternative interpretation remains: it may be simply inappropriate to compare the correlates of trivial and serious offenses" (p. 997). They conclude that "explicit attention to seriousness and content issues across methods must precede comparisons of their results" (p. 1010).

Dunaway, Cullen, Burton, and Evans (2000) have done more research specifically designed to help clarify these issues. They conducted a self-report mail survey of the general population aged 18 and over living in an urban area in the Midwest, inquiring about serious crimes (including violence) as well as lesser offenses. Their study thus focused on adults rather than juveniles and a fuller range of offenses than covered in earlier studies of juvenile delinquency. They describe their effort as follows: "The goal of the current study has been to add an additional piece to solving the class-crime puzzle. In particular, we offer the conclusions that among the general adult population social class appears to be weakly related to involvement in self-reported criminality, but that evidence exists to suggest that that this relationship is specified by race and type of crime (violence)" (p. 611). However, they recognize that no single study such as theirs will resolve the empirical issues. And they urge that in "the under researched area of adult crime, we may need carefully designed, sustained research on class that uses multiple methods across diverse samples" (p. 611). But they suggest also that the current state of the research, unresolved as many issues may be, presents a theoretical problem that needs to be addressed: "those researchers wishing to construct class-based theories of crime must confront why class position, even if related to serious crime, is only modestly implicated in the causation of less serious offenses" (p. 620).

Brannon, Cyphers, Hesse, Hesselbart, Keane, Schuman, Viccaro, and Wright (1973) pick up the problem of "attitude versus action" as it stood in the early 1970s. They note that most studies at that time had reported either negative or mixed relationships between what people say and do. But they also note that these studies had not "concentrated on the validity of typical survey questions in general populations." They carefully observe that although this failure does not invalidate the evidence from the earlier studies, it does leave us "uncertain of their implications for the validity of standard cross-section attitude surveys" (p. 625). Their remedy was to pose their questions about substance and method jointly and to design a multimethod study in which a typical attitude question on the important social policy issue of "open housing" was embedded in a larger survey. The survey was then followed three months later by a field experiment designed to test respondents' willingness to act in a manner consistent with their earlier expressed attitudes. Brannon et al. (1973) report an overall high level of consistency between the survey responses and the later experimental findings. However, they conclude not simply that

"attitudes and actions are consistent" but rather they use their findings as the basis for stating three hypotheses to explain why *in this instance* attitudes and actions were found to coincide.

The study by Brannon et al. (1973), like Hindelang et al. (1979), in addition to illustrating how to develop specific multimethod research questions, illustrates an important general point about multimethod research. Generic labels for research methods, such as those that we introduced in Chapter 2, conceal a great deal of species variation among the actual research techniques that compose the style designated by each label (fieldwork, survey research, etc.). For example, a *survey* may refer either to a questionnaire study of a convenient sample of college students conducted by a faculty member in a college classroom or to an interview study conducted by a team of paid interviewers in the households of a cross-sectional sample of a city's population. (Similarly, Campbell and Stanley [1963] have demonstrated the variety of "experiments.") Broad classifications of methods are useful for purposes of general discussion, and they are important to the analysis of research when they designate groups of techniques that are thought to have common strengths and weaknesses. But if the labels are used without regard for the underlying variations in techniques, they can easily lead to mistaken inferences. In all cases, it is an empirical question whether the findings from a given form of a method correlate well or poorly with a given form of another method.

The Role of Theory in Problem Formulation

Theory plays a dual role in research. On the one hand, new theories solve research problems by accounting for unexplained phenomena and by superseding questionable older theories. On the other hand, existing theory guides researchers in formulating research problems. In determining whether and in what respects a phenomenon or a theory is problematic, researchers consider the context of accumulated theoretical as well as empirical knowledge. And only those phenomena and theories that appear to be problematic when viewed in that context are then studied. Ideally, at least, formulating problems in this manner ensures the orderly advance of knowledge because new research is aimed at solving problems left unsolved in past work rather than being aimed at either totally new or theoretically irrelevant problems.

The guiding role of theory in problem formulation is obvious in verificational studies. But while less obvious, it is equally important in exploratory

research. To a large degree, preexisting theories define both the territory to be explored in the search for problems, and the nature of the new facts one hopes to discover. Of course, opinions differ about how explicit the theoretical background of exploratory research should be. Some recommend spelling it out in nearly as much detail as in verificational research, stating exactly what existing theory leads you to expect and why. Others object to granting existing theory such a directive role and prefer instead to work with general theoretical orientations that sensitize the investigator to important but less precise categories of data. Closure in either the definition of concepts or the statement of hypotheses is avoided in favor of more open "sensitizing concepts" and the "suspension of expectations." In the first view, new problems and hypotheses emerge from the confrontation between old theories and new data, much as in verification. In the second view, new problems and hypotheses emerge from the confrontation between the data and a theoretically oriented and sensitized investigator.

Decentralized Theorizing and Cumulative Knowledge

In contrast with an earlier time, when Glaser and Strauss (1967) criticized the overemphasis on verification in social research, many social scientists now do their own theorizing in the course of their research, rather than testing others' theories. As Freese (1972) has suggested with respect to sociology, there is now widespread acceptance of two premises "(1) sociological investigations should consist of constructing and testing theories; and (2) theory construction is not the exclusive province of an intellectual elite, but is the proper responsibility of each sociologist when he defines some social phenomenon as problematic" (p. 473).

If individual researchers are to be their own theoreticians, however, then each must also accept some responsibility for synthesis; otherwise, we risk inundation by idiosyncratic theories that may be firmly grounded in their authors' research but that are of problematic significance in the larger scheme of things. Verificational research, which by its very nature draws upon and feeds back into a larger body of knowledge, is the conventional way in which researchers in the past assumed this responsibility. However, today we need models of synthetic problem formulation for researchers who want instead to generate theories. We, therefore, conclude this chapter with two such models, *paradigmatic pragmatism* and *mixing metaphors*, which take into account both the role of theory and the role of method in defining research problems.

Paradigmatic Pragmatism

It has become increasingly clear that research methods cannot be assumed a priori to be neutral or atheoretical tools. For example, Walton (1966) demonstrated that the different theories of community power held by political scientists and sociologists might well be a consequence of the fact that researchers in these two disciplines have characteristically studied community power with different types of methods. And Perrucci and Pilisuk (1970) have further shown that the method employed to study community power may not only determine which theory one accepts but may also determine which theories one can formulate and test.

More generally, Ritzer (1980) has posited systematic links between theoretical styles (or paradigms) and research styles (or methods):

> Those who accept the social facts paradigm tend to use questionnaires and/or interviews when they do empirical research. . . . Those who accept the social definition paradigm tend to use the observation method in their research. . . . The choice of methods is, of course, made necessary by the nature of the social definition paradigm. . . . All of the methods discussed in this book *could* be used by the social behaviorist . . . [but] . . . the behaviorist almost invariably uses the experimental method. (pp. 67, 125, 177–178)

However, Platt (1996) in her historical study of sociological methods has cautioned against assuming tight connections between general theoretical orientations and particular types of method. She concludes that "the link between theory and method has been a loose one. It does not follow that it is insignificant, or equally loose in all cases, but it is clear that we need to look elsewhere to explain both the origins of particular methods and the choice of method made in particular projects" (p. 123).

To the extent that theoretical styles and research styles are systematically linked, it may be expected that researchers will pose problems that are compatible with their own particular theoretical orientations and with the methods linked to those orientations, and will ignore problems that are either theoretically or methodologically incompatible. Specific theories and methods may be associated for at least two reasons. First, certain methods may be better suited for gathering data on specific types or classes of variables, and these variables may in turn suggest certain types or classes of theoretical concepts and propositions more readily than others. Second, certain theories may contain concepts and propositions that imply types or classes of variables that in turn recommend

certain methods as being more appropriate than others. Both of these reasons imply that the form in which research problems are initially posed may be shaped by methodological or theoretical commitments, but neither implies any necessary linkage between or limitation by singular methods and singular theories.

And it is also to be expected that researchers working in different theoretical and methodological styles will frequently disagree about the relative importance of particular research problems and even as to whether particular phenomena or theories are problematic. For example, survey researchers who assume a "common understanding" about questionnaire items are often criticized by phenomenologists (e.g., Cicourel, 1962) who regard such understandings as highly problematic. And human ecologists and demographers who study the relationships between resources and population characteristics are criticized by conflict theorists for ignoring concepts and variables pertaining to political power and governmental structures.

Such debates will and should continue until the issues are resolved. However, the pragmatism of employing multiple research methods to study the same general problem by posing different specific questions has some pragmatic implications for social theory. Rather than being wedded to a particular theoretical style, its pet problems and questions, and its most compatible method, one might instead combine methods that would encourage or even require the integration of different theoretical perspectives to interpret the data. If hypotheses and variables that have been previously isolated each within their own theoretical systems are instead empirically interrelated in the same study, then conceptual linkages between different theoretical systems are more likely to follow.

Sandole's (1999) multimethod and multiparadigm study using simulation-generated, archival, and survey data to investigate violent ethnic conflicts in the post cold war era provides a good example of research that tries "to achieve not only the 'normal science' type . . . of *additive cumulation* , whereby one refines and expands what one already knows, but the potentially 'extraordinary science ' type of *additive cumulation* . . . whereby one goes beyond what one already knows, perhaps shifting or combining paradigms in the process"(p. 192).

Mixing Metaphors to Generate Research Problems

"A stitch in time gathers no moss" may make little sense as a homily, but if one struggled to make sense of it, then a new meaning or insight

might be generated from two common sayings. Mixed metaphors, crossovers of theories, or applications of a theory developed in one subfield to another may provoke new questions, provide useful insights, and suggest new ways of looking at phenomena. Becker's (1963) work on deviance, for example, can be seen as an application of Mead's (1934) concepts and propositions of identity formation in social psychological development to the area of deviance, as can his application of Hughes' (1960) research on work and occupations to the development of concepts and propositions on "deviant careers." "Night as Frontier" (Melbin, 1978), "Neighborhoods as Fashion," and "Cities as Organisms" are but a few obvious examples of analyses based on the conscious use of metaphorical thinking. Theories of stratification also contain metaphorical concepts related to the physics and chemistry of geology (such as strata, crystallization, and permeable and semipermeable boundaries). And the economist Lester Thurow (1996) has borrowed the geological concept of plate tectonics and combined it with the evolutionary biology concept of punctuated equilibrium to explore the future of capitalism: "Today the world is in a state of punctuated equilibrium—which is being caused by the simultaneous movement of five economic plates. In the end a new game with new rules requiring new strategies will emerge. Some of today's players will adapt and learn how to win in the new game. They will be those who understand the movement of the economic tectonic plates. They will become the top-of-the-food-chain, 'fittest' individuals, business firms, or nations. Historically, they will come to be seen as the economic equivalent of mammals" (p. 8). Early social scientists, of course, also drew heavily on biological or anthropomorphic metaphors—for example, the work of Spencer (1898) and Durkheim (1897/1951). The early Chicago School of Sociology likewise drew heavily upon biological metaphors, especially in its human ecological theories of cities.

The systematic use of metaphorical thinking is closely related to argument by analogy. Analogous thinking requires seeing similarities among disparate entities and asking whether what is known to be true about the one may be generalized to the other. Posing metaphorical questions is not simply a word game, but enters centrally into the social scientist's paradigmatic view of the world, of what is problematic about that world, and of how to conduct empirical research to understand those problems. According to Kuhn (1970), "puzzle solving" is a characteristic of normal science. Rather than employing explicit rules that define problems and

their solutions, scientists work by example, analogy, or metaphor, applying exemplars from one situation to another:

> The resultant ability to see a variety of situations as like each other . . . is, I think, the main thing a student acquires by doing exemplary problems . . . After he has completed a certain number, which may vary from one individual to the next, he views the situations that confront him as a scientist in the same gestalt as other members of his specialists' group. (p. 189)

The legitimacy of the use of exemplars or analogies is ultimately based upon the community of practicing scientists accepting such models. For example, biological analogies have been widely accepted within sociology, whereas models from physics have not. *Social physics* was offered by St. Simon as an alternative label to sociology as a name for the discipline, but only a few analogies from physics are to be found in research. One such is Samuel Stouffer's (1940) *gravity model* (inverse square law) that relates the amount of geographical mobility between cities to the distance between them.

New metaphors and new concepts suggest new variables and new methods—new questions and new data to answer them. As in the parable of the blind men and the elephant, the metaphor used to describe a phenomenon (it is *like a snake* said the man holding the trunk, *like a tree* said the one holding a leg, etc.) depends partially upon the aspect of reality one happens to get hold of. But metaphorical depiction of reality is also determined by the method of observation one uses. Hearing an elephant, one might liken it to a trumpeter swan; or tasting a juicy, rare elephant steak, one might liken it to a cow. In short, metaphors are often (some say always) used to define reality, and metaphors are in part measurement and method specific. Mixing metaphors can suggest new questions requiring new methods, and mixing methods can generate new questions leading to new metaphors.