

PART ONE

Current Practices

Introduction

There were many failures before humans successfully learned to fly. After watching birds flap their wings, bold and adventurous individuals built huge winglike structures, leaped off cliffs, flapped their wings vigorously, and broke their necks. There are principles of flight to be learned from watching the birds all right, but the wrong analogy had been drawn. In similar fashion, our empirical approach to social behavior is based on an analogy. The natural sciences are incredibly appealing, the physical and biological sciences have been generating counterintuitive and esthetically elegant ideas for centuries. No wonder scholars thought: "Why not a science of society, too? Just find out what the natural sciences do and then apply the same procedures to social events. That is what has to be done." We have attempted, I believe, to apply science to society in as crude and inappropriate a way as when mankind modeled human flight after the behavior of birds. The issue is not how to get as close as possible to the natural sciences in terms of quantification, verification, formalization, and the like—not that any of these is undesirable. The problem is more fundamental, namely the goal itself is based on an ill-conceived analogy. This argument, which will be developed in this volume, does not lead to the conclusion that we should forget the whole business of pursuing a scientific approach to society. Rather, it will lead to a different way of

thinking about the rigorous study of society implied by the phrase "science of society."

Most sociological research is nonexperimental. This is an understandable situation, given the inevitable and proper limits imposed by society. We are not about to assign identical twins randomly at birth to different family units; nor can we suddenly double the income of some third world countries and halve the income of others. Moreover, it is by no means certain in many cases that actual experiments could duplicate adequately events that would change, by their very nature, if strict experimental procedures were used. This is not a debate that we need enter here, however. For good or bad, most empirical social research is based on nonexperimental situations, even though one recognizes the desirability of experiments whenever possible.

Surprisingly, although most sociological research is based on nonexperimental data, the experimental approach does not occupy a secondary position in the development of research procedures. To the contrary, nonexperimental data—the normal source of empirical information—are treated as far as possible *as if they were truly experimental data*. The data are sliced, chopped, beaten, molded, baked, and finally artificially colored until the researcher is able to serve us proudly with a plateful of mock experiment. A great deal of attention is paid, of course, to approximating the experimental model, but little to the distinctive features of nonexperimental social research. Texts in sociological methodology—at least the ones I have looked at—give far more attention to the pure experimental model than to the nonexperimental simulation so frequently employed in sociological research (see, e. g., Galtung 1967; Kaplan 1964; Lazarsfeld, Pasanella, and Rosenberg 1972; Nowak 1977; Sellitz, Wrightsman, and Cook 1976; Smith 1981). Discussing the influence of John Stuart Mill on contemporary sociology, Costner (1971: ix-x) observes that his "basic ideas . . . still dominate methodological thinking, even though these basic ideas have been elaborated in ways that Mill did not foresee." In effect, we are still totally oriented to the experimental model.

What is this experimental model that sociologists have in mind? How appropriate is it for societal data generally and nonexperi-

mental results in particular? Note that there is no point in asking about what it is that natural scientists really do. First, natural scientists do lots of different things; astronomy, nuclear physics, and plant ecology are not the same. But, more to the point, it does not matter for the purposes at hand what these scientists do—what does matter is the notion of science driving the actual work being done in social research. And it is the simple—deceptively simple—experiment that was and is the model used.

There is at least one important difference between the laboratory of the physical scientist and that of the social scientist. In chemistry, physics, and even biology the subjects of study can be brought into the laboratory and studied under controlled conditions. This as yet, except on a small scale as with institutes of child research, is not feasible in the social sciences. The objects of social science research, as persons, groups, and institutions, must be studied if at all in the laboratory of community life.

Yet it is quite as necessary in the social as in the physical sciences to make observations and comparisons of behavior under controlled conditions. One method of obtaining control in the social science laboratory is, first, to determine the significant factors, or variables, which influence behavior, and then to find out for each its quantitative value in extent or degree. In this way, where it is possible, the social sciences obtain what is an approximation of the controlled experiment in the method of the physical sciences. (Burgess 1929: 47)

Boyle's law epitomizes the model implicit in much of the sociological research, both in terms of the kind of data analysis that would be ideal and the linkage between empirical research and theory. In a nutshell, the law posits a certain constant relationship between the pressure (P) and volume (V) of a gas when held at a given temperature, such that $PV = K$. Thus, as pressure goes up or down, the volume of gas declines or expands accordingly. Empirically, one can visualize a leakproof container with a pistonlike top holding a certain quantity of gas, in which the pressure on the top is increased and then decreased over and over again with the same consequences for the volume of gas in each case. The pressure is doubled and the volume of gas goes down by half; the pressure is then halved and the volume of gas doubles; *ad nauseum*.¹

One can visualize this experiment being performed in a rather different manner. Instead of shifting the pressure on one piston to determine changes in gas volume, we could use a set of containers that are identical except for the weight (pressure) on their piston

tops. If an equivalent weight of the same gas is introduced through a valve at the bottom of each container, one should find that the volume varies between containers in accordance with the pressures of their tops in the same way as occurred earlier in the single piston. The difference here is that the conclusion is now based on a *cross-sectional* analysis, whereas in the earlier experiment the conclusions were drawn from *longitudinal* approach in which the pressure in one container was manipulated in various directions to determine the consequences this had for the volume of gas. In the case of Boyle's law, this difference seems to be much ado about nothing. However, since much of the data in sociological research are of necessity cross-sectional rather than longitudinal, there will be much ado about this later.

This simple experiment, performed in either of two ways, represents the model of natural science research that is implicit in most sociological research based on nonexperimental data. Researchers, automatically and without thought, attempt to manipulate their analysis of nonexperimental data so as to approximate, as closely as possible, the kind of experiment that would be used to examine Boyle's law. To do so, it will be shown, they are obliged to make impossible assumptions—assumptions that appear to be matters of convenience but in reality generate analyses that are completely off the mark. What are these analogies? How and why does the application to nonexperimental social research take us so far afield? If these criticisms are correct, what alternative exists that would not abandon efforts at rigorous empirical social research?

THE UNDOABLE

Before addressing these questions, first consider the possibility of removing a horrible constraint that burdens the thinking of most sociologists. Namely, some of the tasks that social researchers undertake are simply undoable in the way that they are presently conceived. Natural scientists, to my knowledge, intentionally avoid certain issues in both their theories and their empirical research. Divine entities or other supernatural powers do not usually

occupy a place as either a causal force or as a topic whose existence they can test with their normal methods (Grüner 1977: 150–151; Barbour 1968: 5–6). The science of society likewise excludes deities as either plausible causal forces or as a potentially verifiable empirical truth. Otherwise, if the event in question is observable or if the theory under consideration has observable consequences, then the empirically oriented social scientist reasons that this is a matter for social research. There is no notion of the *undoable* if observation and measurement are possible. To be sure, the problem may not be solvable in practice because of practical considerations, such as: the number of cases is too small; the probable contribution of the variable is so slight that measurement errors will swamp the hypothesized influence; sampling problems; enumeration errors; absence of suitable indicators or quantitative measures; sticky statistical problems that are difficult to manage, and the like.

The early giants in the natural sciences, in order to construct simple mathematical theories, intentionally ignored a large number of features and factors that could be observed in their data (see the distinction between primary and secondary qualities appearing throughout Burt 1954). To be sure, we are not bound by what they did—after all, thoughtless modeling will get us as far as our ancestors did by flapping mechanical wings. But it is of interest to compare that situation with contemporary social research. At present, we do not think of questions that are purely and simply unknowable, regardless of the quality of the data, or if potentially knowable, that are unanswerable at present because basic underlying knowledge would be required before the questions could be approached. Because research reports so often conclude with one or another variation of the famous theme, “Further Research Is Called For,” we have to ask ourselves if there is a special reason for this. Is it simply the quality of the data, or the need for more data, or the need to repeat research over and over again before the conclusion can be nailed down? The typical project involves working with data sets that are far from ideal. Because of this, perhaps we are apt to avoid the issue of whether *some* questions are simply not answerable with the tools of empirical social science even if the available data are of exceptionally high quality. If current research does not appear to

answer the question it addresses, one must also consider whether it is really a researchable problem. (This ignores the issue of whether the theory driving the research is adequate, since that is what the research results will presumably help the investigator decide.)

This, then, is one of the fundamental issues to be addressed in this volume: Are there questions currently studied that are basically unanswerable even if the investigator had ideal nonexperimental data? If so, what are the alternative questions that can be dealt with successfully by empirical social research, and how should they be approached? In the chapters ahead, it will be important to keep in mind this doctrine of the undoable. Of course, one cannot simply mutter "undoable" when a difficult obstacle is encountered, turn off the computer, and look in the want ads for a new job—or at least a new task. Instead, it means considering if there is some inherent logical reason or sociological force that makes certain empirical questions unanswerable. There are four types of undoable questions to consider: those that are *inherently* impossible; those that are *premature*; those that are *overly complicated*; and those that empirical and theoretical knowledge have *nullified*.

The first undoable refers to issues that are inherently no more answerable with the techniques of social science than are questions about the existence of a deity resolvable by the astronomer. In addition to supernatural forces and issues, one can include here many of the questions addressed by poets, novelists, humanists, and others. There should be no conflict between the different practitioners since each field has its own tasks and problems occur only when they are confused. But here I would also include as undoable those questions that simply cannot be addressed through the usual nonexperimental social research. After all, experiments seem to imply *control*: the researcher sets conditions that allow certain hypothesized consequences to follow. Or the investigator simply wants to see what will occur under some specific combination of circumstances. Naturally occurring events are therefore ersatz data for an experimental model; under *some* conditions the results may represent inherently undoable research. It is vital that one determine this more precisely since the experimental analogy appears to be either applied without reservation or rejected flatly and totally by

its critics. This reexamination of the experimental analogy is a task that will commence in the next chapter.

Another type of undoable is the premature question—that is, a question that cannot be approached until a more fundamental base of knowledge exists. It would be as if one tried to build a locomotive before the wheel was discovered, or tried to change a base metal into gold with the knowledge available to alchemists. This does not mean that a premature problem will take a book with ten or even twenty-five chapters to answer fully, rather than a journal article or two. Rather, it means that basic unanswered questions require considerable work before the query can be approached. It is valuable simply to recognize this possibility, because when one does see an issue raised in the sociological literature as empirically undoable, it is usually with respect to the quality of the data or the statistical issues, and so on. Rarely is an empirical question viewed as premature simply because underlying knowledge is not yet available before the specific problem can be approached. It would be nice, of course, to have a way of determining at least some of the time when a problem is premature without having to wait for the benefit of hindsight.

There are empirical research questions that are undoable simply because they are too complicated. Visualize a lengthy chain of events to be analyzed. At some or many of the junctures, there are a variety of forces influencing the outcome and some of these are not totally understood. Under such circumstances, even though considerable knowledge is available about the outcome at each juncture, the chain is so long and complicated that a model is helpless to deal with the final outcome. This would be no different than asking economists to generate a theory of interest rates that would enable them to predict what the rates will be a year from now, or asking meteorologists to determine the weather in a specific place one year from now. In each instance, there are so many forces operating, and they are so complicated, that the outcome is in doubt even though knowledge is very good for each point in the chain.

Karl Popper, in his battle with historicism, puts it well:

The crucial point is this: although we may assume that any actual succession of phenomena proceeds according to the laws of nature, it is important to realize

that practically *no sequence of, say, three or more causally connected concrete events proceeds according to any single law of nature*. If the wind shakes a tree and Newton's apple falls to the ground, nobody will deny that these events can be described in terms of causal laws. But there is no single law, such as that of gravity, nor even a single definite set of laws, to describe the actual or concrete succession of causally connected events; apart from gravity, we should have to consider the laws explaining wind pressure; the jerking movements of the branch; the tension in the apple's stalk; the bruise suffered by the apple on impact; all of which is succeeded by chemical processes resulting from the bruise, etc. The idea that any concrete sequence or succession of events (apart from such examples as the movement of a pendulum or a solar system) can be described or explained by any one law, or by any one definite set of laws, is simply mistaken. There are neither laws of succession, nor laws of evolution. (1964: 115)

Finally, there are questions that at first glance appear to be reasonable and answerable but are undoable simply because available empirical and theoretical knowledge has nullified them. When John F. Kennedy was assassinated in Dallas, among the questions a shocked nation asked was, "Why Dallas?" On the surface, this would appear to be a reasonable question; after all, the president was assassinated and it was in Dallas. Reflection on the matter will show that it is a nullified question, voided because existing empirical and theoretical knowledge shows that it is without meaning. Let us assume that there are people throughout the nation with a propensity or disposition to assassinate the president. Let us assume that this disposition fluctuates with the mood of the time, the mood in their local place of residence, and the characteristics of the president. If there are such people everywhere (albeit their relative frequency may vary from place to place), and if there is a small probability that on any given day an assassination attempt will be made, and if there is in turn a small probability that such an attempt will prove successful, then its occurrence at any given place is indeterminate. To be sure, its probability will be raised if the president spends much time in the city; hence, in that respect, the probability is higher in Washington, D.C., than in Dallas. However, the exposure to risk will obviously be greater in public settings than in controlled and restricted situations. In that regard, an open vehicle in Dallas is of greater risk than a reception in the White House. If there are proportionately more people in Dallas than in the rest of the nation with such impulses toward Kennedy—and

that is an open empirical question—then a day’s exposure in Dallas has a greater risk than a day’s exposure elsewhere (assuming people never leave their city of residence in order to attempt the assassination). But it is without meaning to ask why it happened in Dallas if, in that sense, is meant an event that could have happened only in Dallas. For to ask such a question is to ask a question that has been nullified by the available empirical evidence coupled with a certain theoretical approach to the event. All one can ask is whether the chances for such an event occurring in Dallas were greater than for another place, but it is without meaning to ask why it occurred in Dallas.²

Observe that the question is nullified, and hence undoable, because of the joint influence of theory and empirical data. If the disposition to assassinate occurred only in Dallas, or if the social conditions leading people with such dispositions to act out their impulses were only found in Dallas, then it would be a doable question. The intriguing feature here is that the question on the surface appears so reasonable and yet it is not—given certain knowledge. The nondeterministic statistical explanation need not involve a high probability of an event’s occurring and can be applied to events that are “intrinsically improbable, even though they sometimes occur,” and would otherwise, “defy all explanation” (Salmon 1971: 9). Observe a simple fact that will be worth returning to later: a complete explanation, in the sense of accounting for all of the variance, under specified conditions can be a nullified task. Evaluating research in terms of variance explained may be as invalid as demanding social research to determine whether or not there is a deity or to determine accurately the world’s population a hundred years from now. This argument will be developed in chapter 5.

This is not an effort to generate an exhaustive list of all possible undoables. But the four just discussed—the inherently undoable, those that are premature, the overly complicated, and the nullified question—are sufficient to help the reader see that undoables exist. Moreover, this raises very different issues about empirical research from the ones generated by those who complain that empirical social research attempts to do too little. Rather, the existence of undoables means that empirical social research can get itself into a hole

because, in some respects, it is too ambitious and seeks to tackle questions that cannot be answered. In critically examining the methods underlying social research, we are therefore free to recognize that there may be some goals and procedures that are totally inappropriate, that involve efforts to do the undoable.

FREEDOM AND CHALLENGE

Some of the most bothersome features of social research need not be problems if it turns out that our initial concerns were generated by a simplistic and inappropriate effort to apply the experimental science model. If this model is not an appropriate one for the subject matter of society, then we are free to shed these concerns and go on to empirical problems and theoretical issues that are appropriate to a science of society. For example, in worrying about sampling problems over time and space, we often compare with envy the situation faced by the natural scientist who takes some oxygen from one place and time with no concern about this matter. Indeed, we should envy the natural scientist if we are trying to work with the same methods as the natural scientist. But if we carry the time and space sample problem to its ultimate end, then we are defeated anyway. There are samples that cannot be taken no matter what the researcher does—to wit, samples of the future are unavailable. For all purposes, there are samples of the past that are unavailable, at least with the data that one wants. Indeed, for most problems, there are samples of the present that are not available either.

What does one do? There are really only a few outcomes thus far. One of increasing popularity is to abandon any serious and rigorous empirical effort altogether. This, to my way of thinking, means throwing the baby out with the bathwater. For empirical research and observation is an absolute must—I say this not to persuade the unpersuadable, but rather to leave no doubt what the starting point is. As Kaplan observes:

If science is to tell us anything about the world, if it is to be of any use in our dealings with the world, it must somewhere contain empirical elements (or, like mathematics, be used in conjunction with such elements). For it is by experience alone that information about the world is received. . . .

It is in the empirical component that science is differentiated from fantasy. An inner coherence, even strict self-consistency, may mark a delusional system as well as a scientific one. Paraphrasing Archimedes we may each of us declare, "Give me a premise to stand on and I will deduce a world!" But it will be a fantasy world except in so far as the premise gives it a measure of reality. And it is experience alone that gives us realistic premises. (1964: 34–35)

The mystique and prestige associated with science are not important. What does seem eminently reasonable is the notion of using evidence to provide feedback on the theories and propositions developed about society. Hence, a conclusion to throw it all out appears unreasonable.

However, it also seems unreasonable to ignore the possibility that the simulated experimental model followed by most empirical social researchers incorporates indefensible and illogical procedures accompanied by a certain scientific ritualism. Again, we can turn to Kaplan:

There are behavioral scientists who, in their desperate search for scientific status, give the impression that they don't much care what they do if only they do it right: substance gives way to form. And here a vicious circle is engendered; when the outcome is seen to be empty, this is taken as pointing all the more to the need for a better methodology. The work of the behavioral scientist might well become methodologically sounder if only he did not try so hard to be so scientific! (1964: 406)

It is therefore a plague on both houses: those who would give up on any effort at rigorous empirical social research and those who contend that the imitation of hard sciences is the answer. Given this reasoning, it seems natural to reexamine and reconsider the model of empirical research underlying most empirical work. The goal is neither a whitewash nor capital punishment, but rather it is to reform and mold empirical research into an activity that contributes as much as possible to a rigorous understanding of society. Without worrying about defining science or even determining the essence of the scientific enterprise, the goal is one that pools together logical thinking and empirically determined information.