1981

Social Science Hubris--A Review of Lindblom and Cohen's Usable Knowledge Review Article

Hans Zeisel

Review Article

Social Science Hubris? A Review of Lindblom and Cohen's *Usable Knowledge*

Hans Zeisel

After decades of optimism and confidence within the social sciences, one now hears with increasing frequency voices of self-doubt and critical self-examination. In *Usable Knowledge*[1] Charles E. Lindblom and David K. Cohen join this chorus. At the outset they state the problem they propose to deal with:

> The stimulus that gives rise to this book is dissatisfaction with social science and social research as instruments of social problem solving.
>
> . . . .
>
> In public policy making, many suppliers and users of social research are dissatisfied, the former because they are not listened to, the latter because they do not hear much they want to listen to. (Pp. vii, 1.)

The specific focus of the inquiry is the social science research used, or aimed at use, by government, business, or anyone who tries to ameliorate or solve a "social problem." The term covers anything from a minor business decision to the broad problems of society; the research put to such use may range from the thoughts of "seminal minds" (Adam Smith, Karl Marx) to custom-designed studies for special uses in the marketplace or the political sphere.

This is an interesting, tersely written book, full of stimulating insights about the many imperfections of the body of social science research, some of them avoidable, some not. With some regret, therefore, I find that the authors' main thesis about what stands in the way of more usable social science research does not come to grips with the problem they want to cure, and I am not even sure that the problem they claim to see is real.

The authors raise three main criticisms. Social scientists, they say, too often

Hans Zeisel is Professor Emeritus of Law and Sociology and Research Associate, Center for Studies in Criminal Justice, at University of Chicago; and Consultant, American Bar Foundation.

1. Charles E. Lindblom & David K. Cohen, Usable Knowledge: Social Science and Social Problem Solving (New Haven, Conn.: Yale University Press, 1979).

1. disregard the wealth of ordinary knowledge that often precedes and successfully competes with scientific knowledge,
2. fail to see that some social science problems are better solved through the interaction of social groups than through scientific inquiry, and
3. claim an authoritativeness for their studies which they do not have.

The book, accordingly, is an effort in what one might call metasocial science. The cure, the authors suggest, will come from a systematic examination of these problem areas. But they fail to show, either through examples or otherwise, how such an examination should lead to more useful research, except perhaps by a more circumspect selection of the research projects that should or should not be undertaken.

To avoid from the outset any misunderstanding: I too believe that the quality of much social science research, both unpublished and published, leaves much to be desired. What I am not sure about is that the cures the authors propose will make it better.

Ordinary Versus Scientific Knowledge

"Ordinary" knowledge, as defined by the authors, owes its origin to "common sense, casual empiricism, or thoughtful speculation and analysis" (p. 12). They mention journalists, businessmen, and public opinion leaders as possessors of such knowledge. They might have added mothers, teachers, and every one of us at one time or another. Living requires constant social problem solving; growing up is learning how to do it well. It is unreasonable to expect social science to play a preeminent role in this ocean of moment-to-moment decision making.

Under these circumstances, the authors' statement that "for social problem solving . . . people will always depend heavily on ordinary knowledge" (p. 12) seems a superfluous admonition.

The authors concede that there are many interfaces between ordinary and scientific knowledge. But they fail to stress that normally when it comes to using social research, even the moderately expert user will effectively discount its weaknesses. More important, the authors fail to emphasize that one of the more interesting functions of social research has been to correct ordinary knowledge when it is wrong.

The way econometric models are being used provides a good example of how different kinds of knowledge inform the decision process. Most major economic institutions—the United States Treasury, the Federal Reserve Bank, major private banks—are using a variety of models to help them forecast the future of the economy and the consequences of planned interventions. The policy makers and their experts carefully look at these models, but their final decision is shaped by the proper mixture of what the model teaches them and their own expert intuition.[2]

As to head-on collisions between ordinary and scientific knowledge, Paul Lazarsfeld's famous review of Samuel Stouffer's *The American Soldier* comes to mind, in which he had some fun with the knowledge gained from "thoughtful speculation." At the outset he notes that often the argument is advanced that surveys only put into complicated form observations that are already obvious to everyone. He then lists half a dozen convincing examples from *The American Soldier* and after a thoughtful discussion, asks, "Would it not be wiser to take [these insights] for granted and proceed directly to a more sophisticated type of analysis?" "This might be so," he appears to agree, and then adds, "except for one interesting point about the list. Every one of these statements is the direct opposite of what actually was found."[3]

Another instructive collision between ordinary and scientific knowledge occurred more recently, in the course of a large-scale social experiment. Through "casual empiricism and thoughtful speculation" many professional criminologists came to be convinced that one of the major causes of speedy recidivism of ex-convicts is their inability, upon release from prison, to secure a normal living for themselves. When I once asked the warden of one of our larger prisons for his main recommendation to reduce recidivism, he answered: "Give the convicts, when they leave prison, an amount of money that will allow them to stay away for a while from their old haunts." Recently the states of Texas and Georgia joined with the U.S. Department of Labor in a major research effort designed to test the truth of that proposition. It was the best large-scale, scientifically controlled experiment ever conducted in a natural setting: randomly selected ex-convicts were given unemployment insurance payments in various forms for six months, and their recidivism rates were compared with those of a control group of convicts who did not receive such payments. The result was shattering: in none of the experimental groups did the

2. The addition of the expert intuition does not always improve the decision. I may be forgiven for relating a personal experience: After the end of World War II, I sat on some dais next to L. D., an old friend who was the advertising account executive for one of our large oil companies. For many years I had been research director of that agency and so I asked, "How is the . . . company doing?" He answered, "All right; but they would have done better had they followed your advice." When I shook my head in amused puzzlement, he added, "Don't you remember when during the war you made some estimate of the size of gasoline consumption after the war?" I then remembered, and my friend added, "Your estimate happened to be on the nose, but . . . did not believe it; they remembered the depression after World War I, and failed to expand their refinery capacity."

3. Paul F. Lazarsfeld, The American Soldier—An Expository Review, 13 Pub. Opinion Q. 377, 378 (1949).

recidivism rate differ from that of the control group. Again, the "ordinary knowledge" that seemed so plausible turned out to be wrong.[4]

Interaction Versus Analysis

The authors argue that in many instances it makes little sense to try to solve a problem through difficult analysis if it can be solved through "interaction," a term that covers anything from coin tossing to the complex operations of the election process or the marketplace. The authors acknowledge that the two modes of "solving" often overlap and complement one another, yet they present analysis and interaction essentially as alternatives. The loose language here hides an important difference. Interaction may "solve" a problem but only research can tell us how good the solution is and how much it costs. Legislative interaction may decree minimum wages, but only research will show that such action increases youth unemployment.

False Authoritativeness

Policy makers, the authors suggest, are often misled by unjustified claims of scientific conclusiveness made on behalf of particular social science findings. As far as I can tell (the authors' exposition here is more discursive than systematic), the indictment centers on three offenses:

1. claiming a finding to be correct when the foundation from which it was derived is imperfect,
2. claiming for a finding a degree of generality that transcends the foundation from which it was derived, and
3. claiming authority for social science conclusions that are based in part on value positions that have not been made explicit.

As to point 1, social science findings are hardly ever derived from perfect foundations. Even controlled experiments are usually flawed. Belief in the correctness of such findings should depend on the seriousness of these flaws, on the degree to which the finding agrees with other, independently derived data on this issue, and finally, of course, on how well it fits into the theoretical context, if one exists.

Thus, claims of having found a true result are bound to be made with varying degrees of assertiveness. Enthusiastic researchers may err on one side, skeptical reviewers on the other. Whether a policy maker will act on that finding will depend in part on his estimate of the likelihood that it is true, and in part on his perception of the relative risks of acting on that

4. In fairness to the authors I must report that the above evaluation is mine. It is in conflict with the view of the investigators: Richard A. Berk, Kenneth J. Lenihan, & Peter H. Rossi, Crime and Poverty: Some Experimental Evidence from Ex-Offenders, 45 Am. Soc. Rev. 766 (1980). My dissent will be published as "Disagreement over the Evaluation of a Controlled Experiment."

finding, or of not acting on it. It is difficult to see why the researcher's exaggerated claim to authoritativeness should be a serious source of error in this complex, many-faceted decision process of the policy makers, which as a rule involves more than one person.[5]

There is one situation where the policy maker typically will not play the role of the skeptic, namely when the "use" he intends to make of the research is to buttress a decision he had reached independently of that research. Many social science footnotes in decisions of the appellate courts are of that sort. In such situations, it is the "user" who puts a premium on authoritativeness, and some producing social scientists happily cooperate, losing thereby the benefit of criticism which a skeptical user would supply.

Problem 2 concerns the extent to which a social science finding, obtained at one place, at one time, can be assumed to have more general validity. The correct judgment will again depend on how that research finding fits in with other research and with its general theoretical context.

This problem, like so many others the authors raise, is not peculiar to the social sciences, although it is more serious there than in the natural sciences, where the extended network of general laws facilitates the decision as to how far one may generalize. In any event, the problem involved in generalization is well known, and it must be solved individually for each finding. The general warning not to err on the optimistic side does not strike me as particularly helpful.

The same is true of problem 3. I doubt that implied value judgments are often so deeply hidden behind social science conclusions that they form a serious impediment to the proper use of that research.

Selection of Topics

The authors have many worthwhile things to say about the type of social science research likely to prove useful to decision makers. They recommend, for instance, that social science research "should be more discriminatingly focused . . . should develop . . . critical or pivotal interventions in social problem solving rather than the comprehensive studies often advocated for policy analysis" (p. 18). This is true when the field has been reasonably well explored. It is less true when a first study of the field is contemplated.[6]

5. Irving L. Janis & Leon Mann, Decision Making: A Psychological Analysis of Conflict, Choice, and Commitment (New York: Free Press, 1977).

6. I have had the good fortune twice to participate in such comprehensive first efforts: in 1932, in the first study of the psychological effects of unemployment (Marie Jahoda, Paul F. Lazarsfeld, & Hans Zeisel, Marienthal: The Sociography of an Unemployed Community, trans. John Reginall & Thomas Elsaesser with the authors (Chicago: Aldine-Atherton, Inc., 1971) (first published Leipzig: S. Hierzel, 1932)); and more recently, 1964, in the first comprehensive study of the jury system (Harry Kalven, Jr., & Hans Zeisel, The American Jury (Boston: Little, Brown & Co., 1966; Chicago: University of Chicago Press, Phoenix Books, 1971)).

The authors also argue that much research suffers from predictable obsolescence and hence should not have been undertaken. This is a strangely myopic point. Consider one of their examples—studies of voting behavior.

It is true that the political structure of the American electorate has greatly changed since the publication of the first of these studies, *The People's Choice,* in which Paul Lazarsfeld and his associates analyzed the Roosevelt-Willkie election.[7] Few of the generalizations observed in that 1940 election still hold, and in that sense the research is obsolete, as are most studies of social events long past. But is not the study of social changes over time itself an important part of the social sciences? Lazarsfeld himself suggested that one task of the social scientist is to do studies for the future historian.[8] On that view, *The People's Choice* and the many later studies of the American electorate, most of them by now "obsolete," are not wasted. In any event, the general warning will do little to prevent misinvestments; only the more careful scrutiny of individual research proposals will.

Misdirected Social Research

I agree with the authors that there is much poor and much misdirected social research. Curiously, however, the policy makers share with the researchers the responsibility for much of this low quality. At least two typical situations are apt to produce poor research.

There are, first, the many large-scale surveys that are sponsored and conducted for no better reason than that the field they survey is temporarily in the public eye, with little regard for the yield they promise.

Second, it has become fashionable in recent years whenever there is a problem to be solved, to first order some research, particularly when the desire to solve it is not great. Ordering research has often become the substitute for the older prescription—to appoint a committee. The hoped-for function of such research is to delay action, and that (and often not much more) is what the decision makers get.

Finally, in much of social research the sponsors, through fault of their own, do not get their money's worth. Typically, federal law requires independent research evaluation whenever federal funds are given for a specific purpose. The quality of such obligatory research is at times questionable. But as Donald Campbell recently suggested, these obligatory

7. Paul F. Lazarsfeld, Bernard Berelson, & Hazel Gaudet, The People's Choice: How the Voter Makes Up His Mind in a Presidential Campaign (3d ed. New York: Columbia University Press, 1968).

8. Paul F. Lazarsfeld, The Obligations of the 1950 Pollster to the 1984 Historian, *in* Paul F. Lazarsfeld, Qualitative Analysis: Historical and Critical Essays 278–99 (Boston: Allyn & Bacon, Inc., 1972).

evaluations, under time pressure from legislators who are in a hurry, begin and end too soon, and are therefore unsatisfactory.

A Closer Look

Whenever the authors try to prove the relevance of their criticism they refer as a rule to out-of-context statements by more (and sometimes less) distinguished social scientists. If they cite research examples at all, they do it only in passing. One cannot help wondering how their criticism would fare if instead it were confronted with some actual studies. So let us take a closer look at two examples, *The American Jury* and a compendium that goes under the name of baseball statistics.

I have selected *The American Jury* for this purpose because it is a fairly typical effort to shed first light on an important social institution. For the present discussion, it also has the advantage of being in many ways flawed, albeit for reasons beyond the authors' control. And I have selected baseball statistics because this very different sort of effort constitutes a precise, well-defined body of social research, compiled not merely for the connoisseur's delight but also to aid the manager in his play-by-play decisions.

I have also a personal reason for selecting these two pieces of research. I know them well, having collaborated on the jury study and having written one of the earlier analyses of baseball statistics.[9] In later years my baseball knowledge matured in the conversations with Harry Kalven who had the capacity to transpose bleacher talk into high humanistic adventure.

The American Jury

Our research aim was to find out what difference it made, if any, to have a case tried before a jury instead of by a judge. Instead of the ideal but impossible controlled experiment, we had to be satisfied with collecting some three thousand jury verdicts and comparing them with the verdict the presiding judges would have rendered, which they communicated to us in confidence. To learn what caused differences in verdicts, instead of interviewing the jurors, we had to rely on the judge's view. Even the sample design was not ideal, since judges are not accustomed to following professors' orders. Nevertheless, with proper allowance for all these imperfections, the study provided important insights. We learned for the first time approximately how often having a jury makes a difference, in what direction the difference goes, and the reasons why a difference arises in some cases and not in others. Within their limitations, we

9. See Kalven & Zeisel, *supra* note 5 for the jury study. The passage on baseball indices is in Hans Zeisel, Say It with Figures 84–86 (5th ed. New York: Harper & Row, 1968) (first published in 1947).

reported our findings with "authoritativeness," in the sense that we believed them to be tentatively true. The findings competed with the "ordinary knowledge" of the many practitioners of the jury trial, and the action of a jury is perhaps a prototype of what the authors mean by resolving a problem through interaction.

In what sense then can one say that *The American Jury* was useful to policy makers, courts, legislators, and practicing lawyers? For policy makers, to the extent they have accepted our findings, the book has narrowed the debate over the merits of the jury. Many factual issues were resolved and thereby removed from the debate, allowing discussions of values to focus more sharply on what the jury in fact does. Lawyers have found the study helpful in anticipating jury reactions, and the courts cite our findings, mostly in footnotes but occasionally also in the body of the opinion. The book, moreover, has stimulated an entire generation of jury research, especially by small-group psychologists who, tired of playing laboratory games with undergraduates, have seized on the jury as an important small group to experiment with.

Thus, in spite of its methodological shortcomings, *The American Jury* has proved useful to decision makers. And although they have occasionally emphasized the book's freely stated and discussed flaws, critics have rarely disputed its many findings.

Baseball Research

Baseball research, in contrast, is less complex and more precise. The collection of available statistics is becoming ever more specific. Batting averages, for example, are refined not only with respect to different pitchers, but also with respect to different ball parks, and different batting situations (e.g., with men on base and with "nobody on"). As the data become more differentiated, they require less generalization and provide better guidance for specific decisions.

The practitioners, nevertheless, have no illusions about the limited "authoritativeness" of those statistics and about the need for combining such scientific knowledge with the ordinary or not so ordinary insights of the manager. No matter how refined the statistics, the decisions he is confronted with will always be more specific than the statistics provide for, if for no other reason than that the manager must predict, must infer from the past to the future at hand.

The absolute irrelevance to baseball research of the other two of the authors' criticisms—the hidden values, and the solution through interaction—should be noted.

Invisible Research and Great Expectations

I suspect that the authors' vision of the discrepancy between the supply of and demand for usable research is distorted by two errors. First, they probably underestimate the amount of research that is actually used, and second, they probably take unjustified expectations to be the expression of real demand.

At no point have the authors tried to estimate the amount of social research that is being used year in and year out and paid for by those who use it. That research ranges from collecting information to feed into very minor decisions of individual marketers, of politicians running for office, or of police departments assigning their manpower, to such comprehensive model building as the dynamic simulation of the economy for which its originator recently received the Nobel Prize in Economic Science.[10]

Perhaps one reason why the authors underestimate the volume of used research is that much of it remains invisible, since it results from private contracts and remains in the private domain. On the other hand, they have perhaps accepted too uncritically the laments of the decision makers that they expected more help from social research.

There is a widely held sentiment about social science research that its primary function is to solve social problems. Youngsters about to enter the study of the social sciences will often say that they are motivated by the desire to help solve some of our social problems. That was my motive too when I began. Now, I know better and tell these youngsters that if changing the world is their primary motive they had better become lawyers or politicians and engage in "interaction" rather than in research.

The power of the social sciences remains small. And the greater the problem, the smaller that power is. Social science, for instance, has not yet found a cure for either war or crime. Even for the resolution of more limited problems, social science is often unsuited because it is as yet unable to cope with great complexities. And even when there is a reasonable expectation that social science research could improve the decision process, the road is often closed either because the research enterprise could not be finished in time, or because it would cost too much compared to its potential usefulness.

Thus, by underestimating the amount of usable research and taking too seriously complaints of the users, it is just possible that the problem is not as pressing as the authors think. Still, I share the basic sentiment from which the dissatisfaction stems: desiring to see more social policy decisions informed by social research. Such progress, however, will come about not so much through the critical self-examination of basic positions as through the painfully slow process of advancing the quality of social research. To evoke the memory of Paul Lazarsfeld once more: his unrealized dream of institutions concentrating on the teaching of applied social research might still be the most important forward step.

10. Lawrence R. Klein in 1980.