
EINSTEIN, RENOIR, AND GREELEY: SOME THOUGHTS ABOUT EVIDENCE IN SOCIOLOGY*

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It is relatively easy to determine if evidence supports or does not support a given theory or conclusion, but a central problem in sociology is our inability to go beyond this and develop truly convincing evidence about a theory. Several changes in current practices are vital, including the cessation of nonproductive assaults between sociologists on the evidence used in the discipline. More fundamentally, theory should not be viewed as one pole of a continuum with research on the other end. A probabilistic view of theory is proposed such that a theory may be correct even if there is negative evidence. This leads to a different way of evaluating evidence, a different view of the role of examples, a distinction between explanations of particular events as opposed to evaluations of theories, skepticism about the possibility of developing theories that account for long complex chains of events, and a sharp differentiation between the evidence used in applied as opposed to basic research. A new sociological specialty should concern itself with epistemological issues that arise in determining the verifiable propositions that are the basic building blocks to sociological knowledge.

My thesis is straightforward. In sociology it is difficult to determine how *conclusively* the evidence supports a theory, let alone to choose between competing theories. This is very different from the relatively easy task of deciding if the evidence is *harmonious* with a theory. The problem is fundamental and pervasive — it existed when I was a graduate student in the 'fifties (and it was an old difficulty then), and it still exists. Progress in statistical analysis, access to new as well as improved data sets, developments in data-gathering methods like in-depth interviewing, participant-observation, and survey questionnaires, and the use of high-speed computers are all steps in the right direction. But evaluating the soundness of results is not equivalent to determining the relevance of these results for our confidence in a given theory. Ironically, we have well-developed rules for statistical analysis and well-developed understanding about research instruments. However, when it comes to deciding if a

given theory is strongly or weakly supported (or undermined) by the evidence, we operate without rules or even a clear comprehension of the issues involved. Usually we can decide if the data are consistent with a theory or not, but we are unable to determine how much the results strengthen or weaken our confidence in the theory. This is because a sound analysis of a well-designed and executed study is not the same as evaluating the relevance of the data set to a given theory.¹ In studying the causes of an ethnic separatist movement, for example, it is one matter to conclude that the data support a given theory; it is another matter to conclude that with these data we are confident enough about our explanation to rule out alternative interpretations. Note the wording "confident enough"; I do not worry about the fact that we can never be sure. Rather, the issue is how much confidence do we have?

One manifestation of this difficulty in evaluating evidence is the "double standard" that we all run into — evidence in support of an "undesired" conclusion is subjected to much tougher standards than evidence supporting other conclusions. This is disturbing because it means there

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¹ To be sure, when dealing with exceptionally narrow "theories" that are really restatements of the data, a sound analysis of the data is more or less the same as providing confidence in the theory. That is not the sort of question being addressed here.

is no consensus about the grounds for deciding whether evidence strongly supports or strongly undermines a theory, or whether considerable uncertainty remains. I have no doubt that the double standard is a problem in other disciplines, but it is probably an exceptionally severe problem for us because of several factors: We often deal with socially and politically sensitive topics that people have strong feelings and dispositions about; it is simply harder to develop strong evidence compared to other sciences; and we do not have even a rough set of guidelines for evaluating our evidence for a given problem.

As a consequence, we have severe difficulties both internally, amongst ourselves, and externally with the larger society. Regarding the latter, we are not taken as seriously as we would like.² We often provide strong conclusions based on evidence consistent with those conclusions, but for which the evidence is not sufficiently strong to rule out reasonable alternative causal interpretations. The larger society is therefore free to follow its biases. Internally, we engage in non-productive paradigm battles that reflect differences about whether evidence is to be taken seriously and, if so, about the types of evidence that are appropriate. In this paper, I comment on the nature of these difficulties and their consequences for our discipline. I also suggest some specific positive ways for dealing with them, such as disavowing some of the standard distinctions we use; re-examining the ways in which we link theory and research; introducing a radically different perspective on how theories should be stated and evaluated; and, following the logic of the above analysis, developing a neglected subfield in sociology.

I address this discussion of evidence to those with an interest in pursuing the truth, who wish to advance knowledge. I fear there are those for whom the social and political consequences of results determine whether they will accept them. Elsewhere, in two widely ignored papers, I have dealt with the political consequences of a given result. I recommend that you examine either of these papers (Lieberson 1988, 1989). Even in cases in which the evidence is not taken seriously, in my estimation the problem is a result of the difficulty of evaluating the strength of evidence. As a consequence, some people in our discipline

ignore empirical results, viewing them as nothing more than expressions of the researcher's dispositions.

CURRENT DISPUTES ABOUT EVIDENCE THAT ARE COUNTERPRODUCTIVE

Some of our current controversies about evidence are absurdly counterproductive. I do not want to dwell on them here, other than to point out that they are irrelevant to solving our problems. These controversies include assaults on "positivism"; an undue emphasis on the deductive process; the qualitative/quantitative distinction; and frictions between macro- and micro- approaches. These are essentially conflicts between interest groups, each pushing to advance its own distinctive concerns and seeking symbolic recognition. Essentially, these are Durkheimian conflicts in that they make a symbolic statement about the value of a type of work that, in turn, is a statement about ourselves and our own work. By some magical process, it usually turns out that what we do is better than what we do not do. This is understandable and inevitable and surely must occur in other fields as well. Alas, it does not help us solve the evidence problem, rather it confounds the issues.

One of the great contributions of sociology is its ability to bring information to bear on topics of interest and basic concern to society. Are the income gaps between blacks and whites changing over time? How common is rape? Do the official statistics underestimate the rates of rape? What percentage of children live in poverty? How many people are homeless? What are the stereotypes about different groups and have they changed in any particular direction? What is the rate of intergenerational occupational mobility and how is it affected by changes in the occupational structure? I select these questions because their answers provide useful information about society even if they sometimes are not couched in theoretical terms. Only a discipline trained to tie itself in knots can manage to find something wrong with such an activity and denigrate it — usually with the "positivist" label. (By the way, who said that knowing some facts is such a bad idea?) In fact, this information-gathering function is often a very sophisticated application of the existing theoretical and methodological tools of the discipline. These activities, however, are more an application of the discipline's theories than theory-testing — although the latter is not prohibited. For example, we know that various socioeconomic attributes affect life chances and

² I refer here not to those so strongly wedded to a position as to make all contrary research irrelevant, but rather to those members of society who are open to sociological results.

dispositions in a wide range of domains. As a consequence, researchers normally take such factors into account when dealing with many problems, e.g., the risk of automobile accident, the chances of unemployment, the likelihood of arrest, etc. In such circumstances, if a given socioeconomic factor has no influence on a given condition, it is unlikely that this will alter the initial perspective or disposition to consider such an attribute in other studies. The general goal in many such studies is to see to what extent existing ideas account for some observed pattern. This is neither theory-testing nor "mindless empiricism," rather it is the application of existing knowledge to a given problem.

A splendid example comes to mind of our evidence-gathering ability that should give us pride. Hauser (1987, forthcoming; Hauser and Anderson 1991) has been examining trends in the rates of college entry for black men and women, and in the past decade or so has found a decline in their rates of entry. Moreover, using careful statistical analyses, this research has evaluated the possible influence of factors like increasing high school graduation, family situation, income, academic achievement, interest in other post-high school educational institutions, the military, and financial aid. By my tastes, this is a substantial contribution to society and is of theoretical importance.

Often such studies stimulate theory as efforts are made to consider their surprising results. Cherlin (1991) suggested that the ongoing research debate about the marital chances of women who have never married by age 30 is not only unresolved, but raises issues about the role of women in contemporary society. One reason we have difficulty appreciating the role of such evidence is our inability to recognize that most knowledge moves ahead through an interaction between deductive and inductive thought processes. Research driven by a theoretical issue is fine, of course, but evidence gathered for different reasons is not necessarily atheoretical and, regardless, can be a valuable contribution. Certainly we would like to have work that does everything, but a slice of bread without caviar on top is still a slice of bread. I might add that basic facts are often the starting point for important intellectual developments. *The Truly Disadvantaged* (Wilson 1987), for example, tries to account for a paradoxical set of social facts, namely a deterioration in the situation of blacks when political and legislative developments would lead one to expect the opposite.

I also find it astounding that many of us make such a to-do about qualitative vs. quantitative

analysis. First, both are highly desirable routes to knowledge. There are some problems for which each is best suited. It is hard to imagine how Anderson (1990) could study the black underclass in any way other than by qualitative field work, or to think of a substitute for the in-depth interviews of women Marines and male nurses by Williams (1989), or how Waters (1990) might have ascertained what lay behind the ethnic responses in the 1980 census without intensively interviewing whites. On the other hand, there is no reason for survey researchers or other quantitative analysts to apologize. I would say, however, that these methods should not generate separate theories of a phenomenon and should not ignore other literature. All methods are in the pursuit of truth, and that goal is not achieved by ignoring contributions from certain methods.

The qualitative/quantitative distinction is itself somewhat arbitrary. Suppose I do 1,000 intensive interviews of a random sample of a population. Is this a qualitative study if I code and cross-tabulate the responses? Likewise, if I administer the General Social Survey questionnaire to 14 drug addicts, is this still a quantitative study? Consider our ideas about "generations," a deep and fruitful perspective on many problems. Should the quantitative scholar ignore the fundamental understanding obtained from Mannheim (1952)? Or is the qualitative person to ignore the important efforts to separate the generational effect from such intertwined characteristics as age and period (see, for example, Fienberg and Mason 1978)? And who can ignore its many exciting applications to various problems, witness the research by Schuman and Scott (1989)? What we really need is an effort to integrate these methods, to take advantage of both procedures and combine their outcomes. The proposal for triangulation is fine, but to my knowledge we still do not know how to deal with contradictory results obtained from different methods. Thinking this through would be far more useful than method bashing. If we are truth-seekers, then there should not be a qualitative truth and a quantitative truth.

In brief, if an astronomer discovers that the moon is made of yogurt, not cheese, then I really do not care whether it was a qualitative or quantitative analysis, whether a theory prompted the study, or whether the discovery was accidental. It is the end product that one appreciates. When concern with how the results are obtained overshadows the correctness of the end product, we have what I would call a "formalistic fallacy" in which form becomes the basis of evaluating the

validity and utility of the content. Certainly we should subject scholarly work to intense critical review, but it should be with *appropriate* concerns such as the logical reasoning of a study, alternative explanations, and the like. These are not to be confused with arguments over *taste*.

EVIDENCE AND THEORY

How we visualize theory and its linkages with evidence is a more fundamental problem that runs beyond the guild wars discussed above. Most of us are aware of the difficulties of linking theory and research, a result in no small way of the acute comments of a variety of scholars (e.g., Blalock 1979; Lenski 1988). Before considering radically different approaches, we should begin with some sense of what we mean by theory and review some of the ways evidence is currently employed.

Here is how two logicians (Woods and Walton 1982) define theory:

(1) A theory is a systematic and orderly organization of what is already known of a given subject-matter, whether it be physics, psychology, household economics or logic. What is already known may have been known only in a common-sense sort of way and without much recognition of underlying principles. A theory, however, not only preserves what is already known: it also articulates and exposes underlying principles.

(2) A theory also discovers new knowledge. In particular, by the orderly and systematic exposure of underlying principles, a theory discovers new implications of those principles and new applications of them, which may not have been apparent at the common-sense level.

(3) In order to achieve the necessary orderliness, a theory usually involves reference to somewhat abstract and idealized concepts. For example, ordinary theoretical physics talks about point masses and frictionless surfaces. A point mass is a quantity of matter just large enough to occupy a geometrical point, and a frictionless surface is one on which, except for the application of external forces, a body's velocity would never alter. Strictly speaking, there are no point masses and there are no frictionless surfaces. These are idealized abstractions. (p. 190)

This view of theory makes sense to me — theory involves generating principles that explain existing information; but it also goes beyond these observations to integrate and account for a variety of other phenomena in ways that would otherwise not be apparent. Our current practices often differ radically from this conception of theo-

ry. *Theory and research are not dichotomies or poles of a continuum; rather research is an essential part of theory since theory includes all known information about a given subject matter.* There are those in our discipline who have developed the warped view that theory is separate from research. In fact, by the definition above, a theory would “predict” all sorts of observations not yet made but it also would account for existing observations. Thus, although a theory attempts to explain events beyond those observed and uses language that is not purely empirical, it also deals with known empirical information about the subject it purports to cover.

This view is in keeping with a recent description of the experimental/theoretical interplay in physics:

A good experimentalist chooses experiments whose results will give better insight into the structure and dynamics of the object he or she is interested in.

The theoretical physicist, however, spends his or her time pondering the meaning of the results obtained by the experimenters. Most important, he or she must have a theory, an expectation of the results in a particular experiment, and must also be alert to what unexpected results would reveal about nature. (Weisskopf 1991, p. 175)

The key point is that theory includes research; they are not two poles on a continuum. Theory is the whole body of our knowledge. It can and should go *beyond* existing evidence, but it also must include existing information, if only to evaluate it. A theory that ignores existing evidence is an oxymoron. If we had the equivalent of “truth in advertising” legislation, such an oxymoron should not be called a theory. From this perspective, however, true theorists have every right to be arrogant. Talcott Parsons's (1953) advice to new sociologists may be self-serving, but it is correct:

Only a minority can or should be specialists in theory. These few however may play a vital role in the future of sociology. Creative theoretical thinking is perhaps the highest achievement in any field of science and certainly without it no amount of empirical industry or ingenuity will attain our goals. If you feel deeply interested and are sure of your talents don't let yourself be scared away from theory as a specialty. Sociology, particularly in America, is going to need your contribution. (p. 602)

However, keep in mind the shift this incorporates: One can be a theorist without doing research or gathering evidence, but one cannot be a theorist

(if you accept the definition of theory that I start with) and ignore existing evidence.³ The division of labor, if one exists, is not in the end product, but in the skills and disposition of the worker. In fairness, the theorist may ignore the evidence if the evidence is in error, but this is not a trivial decision. Consider, for example, how blithely Giddens (1981, pp. 9–11) ruled out consideration of American empirical research on stratification in his book on class, arguing that it failed to address his concerns.

CURRENT PRACTICES

At present, there are several different models or ideal types describing the nature of evidence in sociology. None, in my judgment, solves the problem of dealing with evidence. Let us start with our usual model, the physical sciences. A solar eclipse in 1919 provided a rare opportunity to determine if light rays are bent by the sun, as predicted by Einstein's general theory of relativity, in contrast with the straight line prediction derived from Newtonian physics (based on accounts in Clark 1971 and Hawking 1988). According to Einstein's theory,

Light should be bent by gravitational fields. For example, the theory predicts that the light cones of points near the sun would be slightly bent inward, on account of the mass of the sun. This means that light from a distant star that happened to pass near the sun would be deflected through a small angle, causing the star to appear in a different position to an observer on the earth. . . . It is normally very difficult to see this effect, because the light from the sun makes it impossible to observe stars that appear near to the sun in the sky. However, it is possible to do so during an eclipse of the sun, when the sun's light is blocked out by the moon. (Hawking 1988, pp. 31–32)

The solar situation in 1919 provided an excellent opportunity for Eddington and others to measure light from a group of exceptionally bright stars that were close to the sun (Clark 1971, p. 264). One of the photographic plates examined by Eddington supported Einstein. Eddington later viewed this as the greatest moment in his life (Clark 1971, pp. 285–86). The intellectual achievement is staggering when we think that Newton's principle, "the greatest of scientific generalizations was now, after more than two centuries, to receive its first modification."

(Whitehead, as quoted in Clark 1971, p. 290). No wonder that Einstein, upon receiving a telegram notifying him of the results, immediately wrote a card to his mother (Clark 1971, p. 287). All of this because Eddington's initial observations found the light from stars deflected at the edge of the sun at an estimated range between .87" and 1'.74". Ironically, later examination of these photographs showed errors "as great as the effect they were trying to measure" (Hawking 1988, p. 32). One more point before we ask what this means for us. Observations of a later eclipse in 1922 by a group headed by Campbell, using more advanced technology that eliminated a minor problem with earlier telescopes, were "as close as the most ardent proponent of the relativity theory could hope for" (Clark 1971, p. 371). When later asked what he expected to find from his eclipse photographs, Campbell replied, "I hoped it would not be true" (Clark 1971, p. 372).

What are the lessons of these events? Especially painful, at least for me, is that it represents a model we would *like* to attain. Who would say "no" if offered such precision in our theories and research? Measurement was so precise that an opponent of Einstein's theory accepted the theory after doing his own research. Moreover, just to rub it in, the research drew on observations rather than deriving from a true experiment — so we are talking about a type of research that is plausible for us to consider. To add insult to injury, the evaluation of the theory incorporated only a small number of observations. There are no questions here of whether there was a biased sample of eclipses, or tests of significance, and so forth. Yet, how appropriate a model is this for what we usually do? Observe that its success depends on extraordinary confidence in the measurements — we usually do not have such precision. In addition to data errors of a nontrivial sort, we have multiple causes that are not well-controlled or otherwise taken into account, and we have theories and data without tight links to one another. Could I do a study of 17 organizations in one middle-sized city and then conclude that Weber was wrong? Furthermore, our subjects tend to be heterogeneous. Physicists can be confident that the light waves observed from a nonrandom set of stars tell us something of a general nature, whereas our typical questions cannot be answered with much confidence by interviewing any 12 people. This critical test of Einstein's theory is a wonderful model or ideal that I believe represents the way many of us implicitly think about evidence. Although we certainly do

³ Whether Parson's work meets this test of theory is a separate question.

not want to rule out this model and we should encourage anyone who can develop such a tight test of conflicting theories, it is not a realistic image of how we can work with data.⁴ There are two additional questions to consider later. First, is any harm done by working with this model at present? In other words, is there anything counterproductive about this goal? Second, are we missing the real lesson of this model? For both questions, my answer will be "yes."

Consider the opposite solution to the problem of evidence: Since sociology is not the science some claim it to be — or that it would like to be — I can blithely ignore any questions of evidence and do pretty much whatever I want to do. This is "baby with the bath" kind of logic. In an appendix to *Habits of the Heart* (Bellah, Madsen, Sullivan, Swidler, and Tipton 1985), the authors pointed to the continuing disappointment among those expecting the social sciences to reach the same status as the natural sciences. They described Comte as "one of the most ardent disseminators of what we might call the myth of social science — the idea that social science is soon to become like natural science" (p. 297). To get at the really big questions, we are told we must return to the old ideal of "social science as public philosophy." Although every now and then something positive is said about what the authors think contemporary social science is, it is promptly followed by a new alternative. Reviewing *Habits of the Heart*, Greeley (1985) observed:

The thesis . . . is that civic commitment in the United States has declined in the last century and a half. The thesis is not proven but illustrated with a number of case studies. *The authors inform us that they have looked at survey results for several years and there is nothing in such results which would lead them to change their analysis. No attempt is made, however, to link the survey data with their case studies.* (p. 114, italics added)

Greeley then goes on to present empirical grounds for thinking that, if anything, civic commitment has increased rather than decreased in the United States during this period:

⁴Duncan (1984) observed that "I do not think the relationship between theory and measurement in the social sciences is much like what Kuhn describes for physics. . . . If Kuhn is right about the preconditions for such interaction in physics, and if physics is the model for sociology, then it will be a long time before measurement makes an important contribution to sociology as a basic science" (p. 169).

When I consult the empirical literature, most notably the civic and political participation of Sidney Verba and Norman Nie and their associates (who are cited in a footnote but apparently not taken too seriously), I find confirmation for my conviction. This literature shows no decline in participation in the times measured and suggests a powerful correlation with education. Since the level of educational attainment in American society has increased consistently in this century, it seems likely that civic participation has gone up, not down. (p. 114)

In short, Bellah et al. (1985) represent another solution to the problem of evidence: Since social science does not work as it was meant to work in the models emulating the natural sciences, let us forget such models and get on with our reasonable goal of telling the world what it should do. Note that I do not claim they are *wrong* to do what they do, but rather that it represents a different attitude toward evidence, an attitude that flows from a disbelief in the possibilities of a strongly developed social science.

Incidentally, there is a certain confusion in discussions of the humanities vs. sociology. Is the former, unhampered by the trappings of science, really doing more to help us understand society? The question misses the point. It is not an either/or proposition. Rather sociology exists because of its attitude toward evidence, to wit, a disciplined approach that attempts to use the standards of scientific inquiry in estimating truth. Sociology is not the only pathway to understanding society. A good novel or a good journalistic analysis can do much, but they rest on different standards. A work of fiction, for example, or even an essay, depends on the reader resonating to the writer's personal position. Proof lies in the agreement between reader and writer, and the latter is free to use whatever literary devices are available to create that feeling and understanding. Keep in mind that there is much to be said for our standards of evidence.

Finally, there is the situation in which a theory remains unmodified even though the data contradict the theory. I have in mind the analysis of class differences in the disposition toward the paintings of Renoir, as discussed by Bourdieu (1984). Throughout his discussion, Bourdieu indicated there are such class differences. He compared the "intellectual" or "left-bank" tastes with "right-bank" or bourgeois tastes in terms of preferences for the works of contemporary vs. older painters (p. 292, also pp. 267, 304, 341). For Bourdieu, dispositions toward Renoir as opposed to Kandinsky epitomized class differences in

tastes (p. 292). What is his evidence? Incomprehensible diagrams (Bourdieu 1984, Figures 11, 12, and 13 and pp. 262, 266) showed the preferences for Renoir, but there is no simple information reported in this dense book that directly cross-tabulates these tastes with the left- and right-bank populations. Bourdieu's Table A.2 of Appendix 3 showed such tastes, albeit not in the subclass detail that he was writing about. Keeping this limitation in mind, however, the available tabulations show that 49 percent of the working class selected Renoir as one of their three favorite painters compared to 51 percent of the middle class and 48 percent of the upper class. I will ignore questions about tests of significance, but a difference of one percentage point between the working class and upper class is hardly strong evidence in support of the theory. The other side of the equation is equally unconvincing. None of the working class selected Kandinsky, whereas 2 percent of the middle class and 4 percent of the upper class picked him. For every member of the French upper class who picked Kandinsky, there were 12 upper class members who picked Renoir. There is simply no evidence to indicate a substantial class gap in preferences for these two painters: Those in the professions were the most likely to select Kandinsky (10 percent), but they were also the most likely of any of the subclasses to pick Renoir (61 percent)!

In short, although the data and theory did not mesh, the data were ignored. On a superficial basis, the Renoir example provides grounds for chastising an influential sociologist for not paying closer attention to his data, and I am hardly reluctant to do so. However, as we shall see, the Renoir example also raises deeper and more far-reaching issues about evidence. In point of fact, Bourdieu's theory about the nature of class differences in tastes may be correct even though there were essentially no differences between the classes in their tastes for these two painters.

TOWARD A PROBABILISTIC SOCIAL EPISTEMOLOGY

These examples illustrate the heart of our problem with evidence. On the one hand, at present we assume that evidence that contradicts a theory shows that the theory is "wrong" or at least needs some modification. On the other hand, in the social sciences it is unrealistic to assume that all relevant data will be consistent with a theory even if the theory is correct. Yet evidence in support of a theory is rarely so strong as to eliminate

alternative interpretations. Thus, under current procedures, we are damned if we do and damned if we don't. If we are dealing with theories, then we are dealing with evidence. If we take the evidence too seriously, we may reject perfectly decent theories; if we ignore the evidence, we have no theory, merely speculation. How shall we resolve these problems?

The first step is to recognize that we are essentially dealing with a probabilistic world and that the deterministic perspective in which most sociological theories are couched and which underlies the notion of a critical test is more than unrealistic, it is inappropriate. If theories are posed in probabilistic terms, i.e., specifying that a given set of conditions will alter the likelihood of a given outcome, not only will the reality of social life be correctly described, but we will also be freed from assuming that negative evidence automatically means that a theory is wrong. (A deterministic theory posits that a given set of conditions will lead to a specified outcome, pure and simple.)⁵ Why is it reasonable to assume a probabilistic rather than a deterministic causal environment? I will ignore the massive, almost infinite array of data errors incurred when we measure social events that may prevent a given result from being observed even if it always occurs. Beyond that, in a complex multivariate world, it is unrealistic to act as if social life is driven by deterministic forces, even if we think it is. Since there is such a wide array of conditions affecting an outcome, it is naive to think that a correct theory will predict or even explain the outcome in any given circumstance. Only the most simplistic and mechanical conception would assume that a theory has to be the dominant influence in all historical settings and contexts, regardless of the heterogeneity of the units. Moreover, a theory that accounted for all events would border on being a history of the world.

You may wonder: Are there social forces so powerful and overwhelming that no other conditions can deter their influences? Two examples will hopefully illustrate the limitations of such a perspective. Thirty years ago, I theorized that the nature of the initial contact between ethnic and racial groups would help explain various racial and ethnic outcomes found in different societies (Lieberson 1961). In particular, indigenous subordinate groups that were conquered or otherwise

⁵ For a more detailed development of this distinction and its application to macrohistorical studies, see Lieberson (1991).

overrun would be far less likely to assimilate than would groups that migrated into a subordinate situation. The first type of subordinate group would also be more likely to resist the new nation-state and move toward separatism. The pattern appeared to hold in a wide variety of settings, and often even in the same setting, e.g., the United States and Canada, which have both migrant and indigenous subordinate groups.⁶ In the last few years, with the breakdown of controls in eastern Europe, numerous separatist movements emanating from indigenous groups have emerged. However, it would be foolish to assume that all indigenous subordinate groups will attempt to form separatist states and that no migrant subordinate group will make a similar effort. Moreover, the timing is not instantaneous — a few years ago, eastern Europe could have been cited as an example of how, from a deterministic perspective, the theory does not work; now it is an excellent example of an outcome that is harmonious with the theory. All we can say is that certain conditions, as specified in the theory, alter the *likelihood* of different outcomes.

The second example stems from a remarkable theory about the distribution of towns and cities in terms of their distance from places of comparable size (Lösch 1954). In one sense, you could say that Lösch's central place theory would be off by about 1,000 to 1,500 miles in accounting for the location of New York, the largest city in our nation. However, this is no problem for the theory because the distribution of activities in the United States, as well as historical antecedents, do not fit the model's assumptions. Where the assumed conditions of settlement and a homogeneous land use pattern exist, the theory provides remarkable approximations to the actual distribution, as in Iowa, for example (Table 34, p. 435). What saves central place theory is Lösch's ability and willingness to describe its assumptions and constraints and to explain why they are necessary. The point here is that a theory can be valid and yet one can find exceptions — sometimes glaring ones. Lösch's theory is correct under specified conditions.

One could argue that central place theory is incomplete and that a fuller theory would account for the urban distribution pattern regardless of conditions throughout the world, past and present. Likewise, one might argue that the soci-

etal theory of race relations is insufficient because there is no timing mechanism or added conditions indicating when separatism or cultural nationalism will occur. And who would object to such criticisms? However, it is realistic to assume that theories can be valid and yet not account for the incredible array of forces that may alter predicted outcomes. Indeed, it would be appropriate if theories tried to model the conditions or other factors that either cancel or modify a theory's expectations, as central place theory does.

From a probabilistic viewpoint, theories incorporating a complex chain of events are unattractive and empirical evidence is likely to be misleading. Elsewhere I have cited Popper's reasoning that led him to conclude "that practically *no sequence of, say, three or more causally connected concrete events proceeds according to any single law of nature*" (Lieberson 1985, pp. 9–10). A theory involving a set of sequences will be useful only if the probabilities within the sequence are all virtually 1.0 — and even then the probability value will decline rapidly with the number of sequential events. Suppose the probability of Y, given X, is .7; and the probability of Z, given Y, is .6. The probability of Z, given X, is $.7 \times .6 = .42$. Thus, both theories can be correct, but more often than not a theory that pools multiple steps will make weaker predictions than if each step were looked at as a separate theoretical issue. The errors are even nastier when a second step has a low probability of occurring. For example, to contract paresis, one must first have syphilis, which in turn must progress through several states without treatment. Even then, far less than half of those with untreated latent syphilis get paresis (Salmon 1984, p. 32). To be sure, for those with the initial condition (X or syphilis), the probability of the last outcome will be higher than for those not experiencing X or not having a syphilis infection. However, our analysis and understanding are much greater when we examine each part of the chain. Also, there may be parts of complex chains for which we have no theoretical understanding whatever. In the chain of events leading to World War I, what theory accounts for the assassination of Archduke Ferdinand in 1914 and what theory deals with the likelihood of war had there not been an assassination?⁷

Observe that this shift from deterministic to probabilistic theories makes sense in disciplines

⁶ Consider, for example, differences in the shifts to English described in Lieberson, Dalto, and Johnston 1975.

⁷ Presumably it would still have been very high, but we have no way of knowing what alteration occurs.

with far more developed accomplishments (Crano 1981, pp. 318–20; Lieberman 1985, pp. 223–27; Krüger, Daston, and Heidelberger 1987; Krüger, Gigerenzer, and Morgan 1987; Gould 1989). Whether or not one accepts accident or chance, for all purposes we must act as if they operate. Most important, however, is not what other disciplines do, but rather what this perspective does for sociology. A probabilistic perspective is both liberating and more demanding. It is liberating because deviations are not automatically grounds for rejecting a theory, e.g., Bourdieu's theory does not fall because of the data about Renoir. In this sense, it should make theorists more data friendly, which is a positive development. On the other hand, such a perspective is more demanding because tests per se are inconclusive for accepting or rejecting a theory. Theorists cannot alter existing theories by simply citing examples that existing theory fails to explain. Examples are acceptable but their critical function is reduced.

APPLICATIONS

Evaluating the Evidence

This probabilistic view leads to a radically different way of evaluating the evidence for or against a given theory. Evidence supports or does not support a theory if the frequency of expected outcomes is different from what occurs under other conditions. On the one hand, it means that a theory is invalid — even if the predicted outcome usually occurs — if the outcome is just as common as when the theoretical conditions do not operate (in other words, we look at the marginals). To use an example from Kyburg (cited in Salmon 1984, pp. 30–31), table salt dissolves in water every time someone casts a spell on it before immersing it in the water. Theorizing that it is due to the spell, we find that the theory predicts the outcome because the salt dissolves each time. Despite this frequency, however, the evidence fails to support the theory because salt is equally likely to dissolve in water when there is no spell. On the other hand, a theory can be valid if the predicted event is uncommon under its stated conditions, so long as the event is even rarer when the stated conditions do not occur, as in the case of paresis resulting from syphilis. More formally, if the probability of Y is very high when X_i is “positive” (I use dichotomies to simplify my point), but the probability of Y when X_i is “negative” is approximately the same, then X_i is not a

cause of Y. On the other hand, if the probability of Y when X_i is positive is modest but greater than the probability when X_i is negative, then that is a cause — even though the first probability is small. A theory may even consider the relative likelihoods of different conditions causing Y, but it would be a probabilistic statement rather than an explanation as we now tend to think of it.

At present, we say that a theory is wrong if an event runs counter to the theory. On the other hand, if new observations are consistent with a theory, our confidence in the theory tends to increase.⁸ From the perspective developed here, neither conclusion is appropriate without further information about the *frequency* of such outcomes under the conditions indicated in the theory. An observation consistent or inconsistent with a theory is incomplete information; what is needed is the frequency distribution for a set of observations.

Function of Examples

From this probabilistic perspective, a given observation running counter to a theory no longer undermines the theory. In that regard, the failure of Bourdieu's theory to account for tastes for Renoir is not critical. Because there are other tastes that correlate with social origin/class in ways that are consistent with the theory, a deviation per se is not damning.⁹ However, examples remain important in so far as they illustrate that a given phenomenon can or does occur. This is an important matter.

Black/white comparisons in intelligence have a long history in the United States. Key developments in this controversy are studies that simply provide eye-opening examples of the influence on test outcomes of previously ignored factors. Analysis of Army “intelligence test” scores for blacks and whites during World War I provided a major step in reconsidering widely held beliefs about the role of biological causes in black/white differences in intelligence scores. Not only did blacks from the North score substantially higher than those from the South (remember this was 75 years ago, when Southern segregated schools for blacks were unquestionably inferior to those attended by blacks in the North), but blacks from

⁸For a superb analysis of the contemporary research logic for evaluating theories, see Stinchcombe (1968, pp. 15–38).

⁹Other than for those who properly feel that greater care with data analysis is in order.

some Northern states had higher average scores than did whites from some Southern states (Rose and Rose 1948, pp. 267–68). Nearly 60 years ago, studies (Klineberg 1935) in New York City found that intelligence scores went up among black children who had migrated from the South and that the rise was a function of length of residence in New York. This was important on two counts: It was further evidence that opportunity could have an important influence on what had hitherto been taken as an innate ability, and it showed that the intelligence score differences among blacks were not solely a function of selective migration from South to North because these were changes *after* migration.

These were significant studies, but it would be a mistake to view their contributions as more than revealing an environmental influence not previously taken seriously. Because of such studies, the questions shift to the relative roles of environment and biology. These studies did not resolve the problem, but they were valuable in opening up what we now take for granted, namely the recognition that simple opportunity factors can generate large gaps.

The key feature of examples is that they do not help to resolve a theoretical problem (ignoring the Einstein-like situation). They show that something can exist — a particularly important step if not hitherto recognized. In a probabilistic perspective, examples are no more than that. An example is simply one number or case in the numerator of a fraction about whose denominator we know nothing and hence we know nothing about the example's real meaning. Finding that table salt dissolves in water when a spell is cast on it is an example that is harmonious with a theory, but it has meaning only if a spell alters the frequency of salt dissolving. You will find that this is radically different from the way examples are treated in most contemporary macrosocietal theories in which a given event is viewed as the product of deterministic forces and hence fully supporting or undermining a given theory.

Explanations

Explaining an event is very different from evaluating or testing a theory. An important function of our discipline is to help others understand social processes and structures, and this entails applying our knowledge to specific events. Such explanations are not intrinsically a test of a theory or inherently part of building a theory — rather

existing theories are used to account for specific events.¹⁰ It means determining the most likely processes that could have led to a given outcome. It recognizes that parts of the process are inexplicable. (It is disturbing to me that our discipline has such a low tolerance of incompletely explained theories and data analyses, acting as if a full explanation is the natural state of affairs.)

We know only the most likely causes of an outcome. This is different from looking at an outcome theoretically. A theory (as I have envisioned it) leads to assertions such as: If X_i is positive, then here is the probability of Y ; and if X_i is negative, then the probability of Y is different. An explanation of an event is radically different — it says here is Y , what could have caused it? If we have a single-cause theory, then we have an easy task because the presence of Y (the explicandum) is a result of the presence of, say, a given value of X_3 and, whatever the probability of that value generating Y , it is zero under any other circumstance. Hence, the cause of Y is X_3 because otherwise Y does not occur.¹¹ In the more likely case in which there are multiple causes of Y , each with different probabilities, then accounting for the specific event can be quite complicated and actually not always knowable. Suppose, for example, we have a theory such that the probability of Y is .2, .5, and .9 for positive values of X_1 , X_2 , X_3 , respectively (with a probability of 0 for negative values of these three variables). If only one of these variables is positive, then there is no difficulty — according to the theory, that is the cause of Y in the specific instance. If more than one factor is positive (and in this example a cause of Y), then it is possible to list the different causes that could have led to this outcome and to make a probability statement about the likelihood of each factor being the cause. For example, if X_2 and X_3 are both positive, then the odds are 9 to 5 that X_3 is the cause, but there is nothing more certain to say.

At first glance, this way of explaining a specific event appears to be closer to a statistical analysis than a “theoretical” analysis. In point of fact, it is the only possible form of explanation once we settle on a probabilistic type of theory construction. Incidentally, in a long series of events

¹⁰ To be sure, the failure of an existing theory to fit the events would raise new questions.

¹¹ If there is only one known variable that generates Y , say X_3 , and the values of Y are greater than zero for both positive and negative levels of X_3 , then this is grounds for suspecting that there is at least one unmeasured social characteristic affecting the outcome.

involving an array of individual steps for which we have a probabilistic theory, there are often many chains that could lead to the same outcome, each having a low probability of occurrence. Moreover, there are a variety of chains that would *not* lead to the observed outcome. Indeed, any given chain is an unlikely outcome. This does not justify highly deterministic theories of why some important event *had* to happen.

It is plausible, as I noted earlier, to view many physical and biological processes in probabilistic terms. In the case of evolution, this view is appealing and it ought to be appropriate for complicated social processes. (Remember it is plausible to view the genesis of humans as a chance event in evolution.) This means, of course, that it is essentially a futile and nonproductive undertaking to explain complex events with a single deterministic theory as if some outcome in a given setting *had* to occur. There are different parts to such a chain, and some elements are subject to theory while other elements are best viewed as accidental. For example, without accepting a “great man” theory of history, consider the possible impact on events if the assassination attempts on Franklin Roosevelt in 1933 or Adolph Hitler in 1944 had succeeded.

Looking at first names as another example, it is certainly possible to construct theories that would help us understand some of the mechanisms that led to the decline of Donald as a popular first name, or to the initial rise of Gary. But there are details that we cannot expect from any theory that would be part of a “full” explanation. For example, an agent changes the name of a young actor, giving him the name of the agent’s home town in Indiana (a town that had been given the surname of the chairman of the company that built the steel town early in this century). Thus, we go from Judge Gary to Gary, Indiana, and then to the actor Frank Cooper, who becomes Gary Cooper. This made-up name of Gary was given to 2.4 percent of boys in 1941. Likewise the decline of Donald was precipitated by the invention of a cartoon duck by Walt Disney in 1934.¹² We can analyze these names and link these events to important features in the creation and maintenance of tastes, but it would be a mistake to attempt a deterministic theory of why Gary *had* to become a popular name or *why* a duck was named Donald.

I purposely pick these examples from what some of you mistakenly think of as the almost

superficial features of popular culture because it is easier to see the unexplainable and “chance” factors affecting an outcome. In point of fact, what you see as deep and fundamental facets of a society may be, when interpreted in this grossly deterministic manner, the equivalent of Donald Duck.

Policy Oriented Research and Theory

Nonapplied or “academic” endeavors may sometimes seek to account for a given outcome, e.g., a theory of economic stratification, a theory of the nature of race relations or group cohesion, and so forth. Research oriented toward social policy likewise has this goal, but there is a critical difference between the two approaches in the required evidence. Unlike most social research, policy-oriented work must estimate not only the effects of a variety of social features on a given dependent variable (the “problem”), it must also consider the impact of these features on other dependent variables as well. This is the “side effect” problem. A strong case can be made for labeling work “unethical” when it does not consider these social side effects; this is no different than a drug company that ignores a high likelihood of cancer being caused by its prescription for headaches. Problem-oriented research should also be similarly confident in its evidence about the broader social consequences of a policy for other dependent variables. At the very least, it should state any unresolved risks. In a nutshell, policy-oriented research requires evidence about the diverse consequences of a given action, a requirement not normally faced in “academic” work.

Gathering Evidence

The evidence addressing a given theory is likely to be inconsistent, in some cases going in one direction and in other cases in the opposite direction, with a good dose of inconclusive evidence. This means that we have to interact with the data to resolve apparent paradoxes. Here meta-analysis may play an important role. In effect, meta-analysis statistically analyzes a variety of different results to infer underlying causal patterns. To my knowledge, it has been successful in medical research, which is generally similar to our situation in that many different studies on a given topic often do not give consistent results (see the review by Iyengar 1991). The point, of course, is that a given theory will not stand or fall because of a single study. (Another possibility, raised by

¹² Based on trend data for births in California.

several thoughtful readers, involves the use of Bayesian models in interacting between empirical evidence and theory. But this is a topic I cannot address in this paper.)

In addition, we need to use multiple data sets to take into account or "control" various factors rather than accomplishing this by statistically manipulating various characteristics in a single data set. A situational set of controls rather than statistical manipulation of a single data set may well be an appropriate solution to some of the difficulties of interpreting evidence, such as unmeasured selectivity and control problems. Freedman (1991, pp. 294–99) described how John Snow inferred the cause of cholera, which is an outstanding example of a causal inference based on situational comparisons rather than complex statistical controls. Duncan (1984, p. 99) referred to the situational evidence used by Chadwick to develop a method of sanitation that arguably saved more lives than any other measure up through World War II. At any rate, the probabilistic perspective on theory suggests that a single data set will not be strong enough to resolve the theoretical issues. This becomes critical as we recognize that finding results that are consistent with a given theory or interpretation is not the same as gaining sufficient confidence in the validity of the theory. Often, other perfectly plausible interpretations are also consistent with the data in any single study.

BUILDING BLOCKS: A BASIC NEED

As I have shown elsewhere (Lieberman 1985, chap. 3), empirical research — whether experimental, statistical, or qualitative — makes counterfactual conditional statements that, strictly speaking, cannot be determined to be either true or false. This is because conclusions are drawn about what would have happened had a given event not occurred. The advantage of true experiments with random assignment is our considerable confidence that the population subjected to a set of conditions would have behaved like the control population if it had not received the test condition. But this is still a counterfactual conditional conclusion because the test population did not experience the control group's situation. With the right procedures and statistical tests, we have a high degree of confidence in such conclusions even though we know they are not certain. When gathering evidence for most of our problems, however, we do not have randomly assigned experimental conditions; rather we have naturally

occurring nonexperimental or at best quasi-experimental conditions. Our confidence in the counterfactual statements we make is going to be weaker. This is a fact of life.

Under the circumstances, a central issue is to improve our building blocks of evidence. It seems to me that we might try to develop a set of counterfactual conditional results that are based not on trying to solve any given problem, but rather on estimates of what basic building blocks of knowledge are most important. In other words, what are the most fundamental sociological questions that require empirical study and determination?

Several illustrations should make this point clearer. The first builds on an issue I raised in *Making It Count*. In the important research by Coleman, Kelly, and Moore (1975) on the influence of school desegregation policies on white flight from the central cities of metropolitan areas, we would have stronger evidence if we were reasonably confident of our understanding of the forces influencing white exodus from central cities regardless of desegregation. This is because there had been net out-movements before the desegregation issue arose. If we had a strong understanding of this process, then the counterfactual conditional proposition about the influence of desegregation policies could be better evaluated. There would be greater confidence in our counterfactual conditional inference about desegregation if the study began with a more basic counterfactual inference about the general forces leading to white outmigration from the central cities.

Another example is stimulated by Wilson's (1987, pp. 84–89) fascinating results using changes through the years in the ratio of employed men per 100 women of the same age, separately for blacks and whites. This is evidence that black family deterioration increased with increases in male joblessness during the period studied. Again, if a strong and well-developed theory of marriage and illegitimacy could be applied to the problem, we would be in a better position to understand how much of the increase in black female-headed families is the result of the male eligibility problem rather than other long-term societal influences.

I select these two studies because they are probably familiar to many readers. But they illustrate a quandary. On the one hand, both studies dealt with problems of immediate and great significance to an understanding of race relations — they are, to put it mildly, big questions. Was school desegregation counterproductive in that it

actually increased black/white school segregation? What explains the deterioration of conventional black family structure? It would be disappointing to ignore such problems and it would be a “cop-out” to postpone such studies until the basic building blocks are known. On the other hand, both studies illustrate the need in our discipline for building blocks of knowledge. We can have maximum confidence in a counterfactual inference only when it is based on a high level of confidence that we know what would have otherwise occurred.

I suspect that the answer to this dilemma is not terribly difficult. Just as we have subareas and specialties devoted to different substantive areas in sociology, likewise we need to develop a specialty that attempts to understand our most fundamental counterfactual conditional statements, to develop strong ways of obtaining evidence about them, to evaluate the evidence, and when satisfied, go on to the next building block of knowledge. In other words, rather than only approach a problem from either the perspective of a theory about it or to try to answer a relatively specific empirical question, I am suggesting an additional approach. This would seek to determine what empirical questions are most fundamental for us to answer, and then seek to answer them with as much confidence as we can generate, given the limits of counterfactual evidence. We would build on this evidence to attain further blocks, ad nauseum. This would move us toward a set of building blocks of knowledge. One of the most important lessons of the Einstein example discussed earlier is relevant to this discussion. The test of Einstein’s theory required a considerable set of building blocks of knowledge, e.g., the knowledge of when the eclipse would occur, a deep understanding of light, and a grasp of what would occur if his theory was not correct. In that regard, knowledge builds on knowledge. It ought to be worth trying to determine what is the most fundamental knowledge needed to begin the building block process.

CONCLUSION

Sociology needs an appropriate epistemology; not one that blindly mimics a model of scientific practice that is not fully appropriate for our situation, but nevertheless one that works with evidence. This goal is hardly peripheral to our daily work; rather it is intertwined with the way our theories are formulated, the evidence we bring to bear on them, and how we interpret the evidence. It is

also central for training our students because, as far as I can tell, they are trained in statistics and methods of research like the construction of instruments, content analysis, and historical methods. However, to my knowledge, they rarely consider such epistemological questions as: How do we know what we know? When is our evidence reasonably conclusive? When is our evidence inconclusive?

In determining “conclusive” and “inconclusive,” can we do more than create a subjective sense of conviction? I would say that the answer has to be “yes” — there is simply no choice if we are to be a serious discipline. By conclusive and inconclusive, I do not mean statistical tests of significance applied to estimate a population universe. Instead, I have in mind taking the evidence, including statistical evidence, and determining the likelihood that it supports or does not support a given theory. *Statistical conclusions about a data set are different from conclusions about the importance of the data for evaluating a given theory.*

The key is the development in social science of the epistemic term “adequate evidence.” We need to develop criteria for adequate evidence and to recognize degrees of confidence in a theory (see Chisholm 1965, pp. 473–74). Chisholm (1977, p. 120) noted two very general questions: One dealt with the *extent* of our knowledge and the other dealt with the *criteria* of knowing. As he observed, the two are intimately linked such that advances on one question help us advance on the other question.

I have attempted to identify some of the steps that we need to take in this movement to develop a stronger basis for evaluating evidence. This means reconsidering our present tendency to operate with a simplified, mechanical view of a theory and its evidence, such that a theory must explain all relevant events if it is correct. A variety of forces operate in any given setting, some of which alone or in combination may overpower the impact of a given theory’s consequences. A more realistic view of our subject matter will result if we adopt a probabilistic view of both theory and evidence and if we use a probabilistic perspective to link them. This leads us to recognize that evidence can run counter to a given theory without meaning the theory is wrong. It also implies that evidence can be harmonious with a given theory without meaning the theory is right. In short, we need to think very differently about what is theory, what it should do, and what is appropriate evidence.

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