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Advancing Social Research: An Essay Based on Leiberson's Making It Count

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by

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We review Stanley Lieberson's book, Making It Count: The Improvement of Social Research and Theory (University of California Press, 1985). This is an important, stimulating, and provocative book that should be required reading in sociological methods and theory courses. It deserves attention because it is a reasoned critique of the logic underlying contemporary social research by a sociologist who has been engaged in research for many years. Lieberson's arguments cut to the heart of what sociology is and should be, calling for a different approach to the study of social phenomena.

The book deals with the objectives of social research and focuses attention on the current disjuncture between research and theory. In essence, Lieberson argues that social research should more often be designed for the purpose of providing evidence relevant to theory and that moving in this direction will require changes in research practices. In this essay we review and elaborate on Lieberson's major points.

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1. Introduction

Stanley Lieberson's book, Making It Count: The Improvement of Social Research and Theory (University of California Press, 1985), is an important, stimulating, and provocative book that should be required reading in sociological methods and theory courses. It deserves attention because it is a reasoned critique of the logic underlying contemporary social research by a sociologist who has been engaged in research for many years. Lieberson's arguments cut to the heart of what sociology is and should be, calling for a different approach to the study of social phenomena. It is a mistake to view this book as a narrow critique of the application of quantitative methods (Costner, 1986; Berk, 1986). It deals with the objectives of social research and focuses attention on the current disjuncture between research and theory. In essence, Lieberson argues that social research should more often be designed for the purpose of providing evidence relevant to theory and that moving in this direction will require changes in research practices. In this essay we review and elaborate on Lieberson's major points.

The book under review is divided into two parts: a discussion of current problems in the development of sociology--roughly three-fourths of the book--followed by an outline of directions for future research. In Section 2 we review and comment on Lieberson's principal criticisms of contemporary social research. Section 3 contains a review of the research agenda proposed in the book; however, we also elaborate on and extend Lieberson's discussion beyond the general points which he raises. On the basis of the reviews published to date (Costner, 1986; Berk, 1986), it seems clear that the research hints provided by Lieberson were not sufficiently detailed to provide clear directions for persons inclined to pursue the various paths outlined in the book.

Thus we take some liberties and present some explicit examples of necessary next steps. We believe that our proposals are very much in the spirit of what Lieberson intends, but only he can judge the fidelity of our assessment of his intentions. The main point is that we hope to clarify where new research might begin immediately and thus start the process of responding in depth to Lieberson's call for reform.

2. Current Problems

Lieberson isolates six problem areas which are in serious need of thorough reconsideration. These are:

- (i) the controlled experimentation paradigm;
- (ii) the role of control variables;
- (iii) the contamination problem;
- (iv) conceptualizations of causality;
- (v) the pervasive goal of "explaining variance";
- (vi) inadequate attention to and clarity about levels of analysis.

Subsections 2.1-2.6 treat these topics in turn.

2.1 The Controlled Experimentation Paradigm

The methodology of social research is based on a paradigm of controlled experimentation as developed and practiced in many physical and biological science specialties.[1] Controlled randomized experimentation--an essentially 20th century development utilized in only limited precincts of physical and biological science--is viewed as an ideal method of inquiry. After a searching and thoughtful evaluation of contemporary sociological research, Lieberson concludes that very little progress of a fundamental nature is being made within this paradigm and that the strategies leading to the development of a defensible knowledge base in sociology need to be set upon a new foundation. In particular, a foundation is needed that is essentially tuned to the phenomena under investigation.

Controlled experimentation, in which the investigator sets the conditions and perturbs the system in order to measure a response, is rarely possible in sociology and most other social science disciplines.[2] Most of the fundamental determining influences with which any "science of society" must be concerned are not subject to manipulation. Even when experimentation is possible, it is often socially unacceptable for ethical reasons or, if acceptable, cannot duplicate adequately the events that would occur in a "natural" setting. Lieberson does not rule out the possibility that experimentation may be acceptable and desirable in some situations, but he argues that it can be used only rarely to address social science questions.

Because most social research is nonexperimental, social scientists have attempted to simulate controlled randomized experimentation through a quasi-experimental approach. In nonexperimental research, the investigator must deal with the operation of selective processes that influence outcomes observed in the conditions under study. "If there is any feature of social life about which a high degree of confidence exists, it would be this simple principle: social processes are selective processes" (Lieberson, p. 19). With an investigative paradigm based on controlled experimentation, a natural analysis strategy for dealing with selectivity in the observational studies in sociology is to introduce a wide range of "control variables" as, in effect, surrogates for the actual controlling that occurs in the domain of experimentation. As Lieberson points out, it requires rather yeoman assumptions and/or unusual phenomena to justify a comparative analysis of an observational study as though it represented conclusions (inferences) from an experiment. The main point, however, is that the attempt to imitate the experimentation paradigm is where the trouble lies. The goal of approximating, in one sense or another, the natural science experiment should be given up--except for some exceptional circumstances--and replaced by strategies which deal with selective processes directly.

2.2 The Role of Control Variables

One reason the quasi-experimental approach breaks down is that a typical research problem involves many complex layers of selectivity, only some of which may be known and fewer of which may be controlled. It therefore becomes practically impossible to address the problem of selectivity with full and appropriate controls. Even the control variables that are used may not operate effectively as controls if they take on different meanings in different situations. This may occur when changes in social context cause a variable with the same formal properties to vary in its consequences. Another problem with the use of control variables is that conclusions rest on the assumption that relevant variables are free to vary with one another and that every possible combination of the values of the variables can appear. In many circumstances a control variable is inextricably tied to levels of the independent and dependent variables so that a conclusion about the relationship between the independent and dependent variables net of the control variables is based on assumptions about combinations of variables that in reality cannot exist. Holding control variables constant under these circumstances does not provide an approximation to what is really going on but leads to "conclusions about the consequences of shifts in different variables that either will not occur or, if they do occur, will represent such fundamental changes in the society as to make it certain that the observed linkages will not remain unchanged with these new distributions of the variables" (Lieberson, p. 212).

2.3 The Contamination Problem

Counterfactual conditional statements, in which conclusions are predicated on events that did not happen, are intrinsic to virtually any comparative analysis, and the potential that they will lead to unjustified conclusions is present in the physical as well as the social sciences. Moreover, such statements are not avoided by getting away from the controlled experimentation paradigm.

One factor affecting the confidence with which counterfactual conditional statements can be made in the social sciences is "contamination." If a variable acts in a given setting and generates a response there, it is often the case that the same or a similar response will be generated in other settings where the variable in question has never acted. This "contamination problem" is the result of diffusion of information across settings and human reactions to it. Ignoring this process can lead to major errors of inference about the consequences of interventions or simply gross errors in projections.

It has been suggested that one way to avoid the contamination problem is to take a "trajectory approach," in which past events in a given setting rather than events in other settings are used to determine what "would have been." The problem with this approach is that it is usually impossible to establish what would have been with any confidence on the basis of the trajectory of changes in the dependent variable prior to the change of interest in the independent variable. One reason for this is that trend data and basic knowledge of social processes are sparse. Future projections based on past events have tended to be quite inaccurate. There is also the problem that, just as humans in one setting may anticipate and respond to events in other settings, humans may anticipate and respond to forthcoming events before they occur.

2.4 Conceptualizations of Causality

Asymmetric causal forces are a common feature of social phenomena. However, in social research and theory most causal forces are implicitly assumed to be symmetric, or bidirectional. Policy research (recommendations) is often misguided as a result of ignoring this aspect of a phenomenon and proceeding as though a given causal linkage was reversible. Lieberson identifies three different types of irreversible causes in social phenomena: (1) constant or invariant social forces; (2) a causal sequence that leads to a fundamental change in the dependent variable so that it will not respond in the same way to a reversal of the independent variable; and (3) a causal sequence that leads to changes in other conditions that preclude the dependent variable from responding to a reversal of the independent variable. A substantial research agenda, only partially stated by Lieberson, must be addressed if asymmetric causality is to be adequately dealt with in social research. In particular, there are intrinsic limits to defensible knowledge that can be acquired on asymmetric causal processes where replication of such a process is not to be found naturally and where experimentation can basically not be carried out.

2.5 The Pervasive Goal of "Explaining Variance"

There is an enormous theory gap which leads much empirical social research to focus on "variance explained" about a dependent variable(s) in terms of crudely plausible independent variables as a primary target in analyses. Searching for variation _before_ delineating candidate theories linked to specific research goals essentially forces the investigator away from fundamental questions about social processes. The substantive importance of variables is judged in terms of their contribution to explained variance, and variables that do not contribute to the explained variance are often discarded. Conclusions derived from this approach tend to be _ad hoc_, since the distributions of the independent and dependent variables vary across settings and data sets. Thus, accounting for variation within restricted classes of models is an "end-game" in the research process. It is not germane to getting at deep understanding of basic social processes.

One problem with the most popular models in use is that they attach importance to systematic variance but not to random variance. If a process is stochastic, both deterministic and chance factors may operate so that knowledge of deterministic factors would not be expected to account for all of the observed variation, even under ideal research conditions. As Lieberson notes, this point was made by Spilerman (1971) in an analysis of intercity variation in race riots during a period of the 1960s, but it has had little impact on subsequent work in sociology. In order to determine how much variance should be explained by measured variables, it is necessary to have a theory so that the effects of variables can be analyzed within the framework of an appropriate model. For a superb series of examples illustrating this point in the context of historical demography, the reader should consult Wachter (1978).

Another problem with the focus on explained variance is that this goal has had a major effect on the choice of problems for study within sociology. In order to explain the variance in a dependent variable, there must be variance in both the independent and dependent variables examined. This need for variance in variables under study has led researchers to focus on problems addressable with data sets that contain variation on the variables of interest. In most instances this variation has been at the individual level, and the subject of analysis has been interindividual differences in a dependent variable. Many, if not most, fundamental sociological questions, however, cannot be addressed through this type of analysis because the questions focus on macro-level, structural forces in which there is little or no variation.

Because the analysis of interindividual differences or variation is not well-suited to address many questions of importance in sociology, it is of interest to consider why this approach has become so predominant in social research. The answer is that a transfer of methodological knowledge occurred from genetics, where statistical methods for analyzing variance were originally developed (Fisher, 1918; Wright, 1921). However, in genetics the analysis of variation was motivated by the major substantive concerns of the field:

> "The genetics of a metric character centres round the study of its variation, for it is _in terms of variation that the primary genetic questions are formulated_ [emphasis added]. The basic idea in the study of variation is its partitioning into components attributable to different causes. The relative magnitude of these components determines the genetic properties of the population, in particular the degree of resemblance among relatives." (Falconer, 1961:129)

Lieberson rightly claims that the questions in much social research are _not_ driving the analysis; instead, the reverse is frequently true. Thus an often useful tool now diverts attention away from the more fundamental substantive issues, many of which are not naturally formulated in terms of variation.

2.6 Inadequate Attention to and Clarity about Levels of Analysis

Since preoccupation with explaining variance has led researchers to focus largely on accounting for interindividual differences, and many of the questions of central importance in sociology concern the functioning of larger collectivities, there has been a tendency to confuse levels of analysis and either assume or argue that the analysis of associations at the individual, or micro, level can inform us about processes operating at a higher level. This is not the case since an "association observed at one level need not hold at another level" (Lieberson, p. 108). Observations of associations at the micro level cannot be used to infer that similar associations exist at a macro level, just as observations of associations at a macro level do not indicate that similar associations will be found at the micro level.

The importance of distinguishing between processes operating at the micro and macro levels and of understanding the dynamic interconnections between these levels has been an issue raised in two recent articles on the current state of sociology (Coleman, 1986; Collins, 1986). Lieberson argues for greater attention to macro-level processes in social research, cautioning that the associations among variables at the micro level which now receive considerable attention usually do not provide information about "basic causes."

3. New Approaches and a Research Agenda

With Lieberson's criticism at hand, we discuss some necessary responses. Many of the specific suggestions in subsections 3.1-3.5 could be readily implemented and would, in our opinion, shift much social research in a more meaningful direction.

3.1 Research Goals, Doable Problems, and Uncertainty Principles

Lieberson laments the fact that much social research is carried out without clearly delineating the goals of the investigations. In particular, descriptive studies, which in our view comprise most sociological research, are not sharply distinguished from theory testing, and these two types of studies in turn are often not treated as distinct from evaluations of policy interventions. The goals of description, theory testing, and policy-targeted program evaluations are associated with quite different working environments, and a failure to take account of the environmental constraints can lead to unnecessary attempts at what we would regard as intrinsically undoable problems.

Most descriptive analysis and theory testing operates within the competitive environment of the academic social science community, and the pace of advance of knowledge is largely set by the activity level of research workers. Policy analysis, if it is to be useful, must also be timely, and the nature of the information and the time scale for its delivery is largely set--in the United States--by the pace of the legislative process and the information requirements of political actors (Coleman, 1978; Cordray, 1986). Fundamental theoretical developments and long-term empirical studies cannot ordinarily be carried out if information delivery is to be synchronous with the needs of the Congress in developing new legislation--e.g. bills on welfare reform or housing subsidies--or, for that matter, judges in need of refined evidence for particular litigation.

One among many examples of this situation were the income maintenance experiments authorized by congress to guide welfare legislation and carried out in the late 1960's and 1970's (Ferber and Hirsch, 1982). The experiments were of long duration and had major components in descriptive analysis, theory testing, and even theory development. The time required for these activities virtually guaranteed that the results of these experiments would be produced quite out of synchrony with the legislative process for welfare reform. For more details on this point as it relates to income maintenance experiments, housing allowance experiments, and time-of-use electricity pricing experiments, see Fienberg, Singer, and Tanur (1985). See also Cordray (1986) and references therein for further elaboration of the timetable and structural features of policy research which is responsive to congressional information requirements.

Even in traditional descriptive analyses, one may encounter undoable problems. For reasons not entirely clear to us, there seems to be an implicit view in much of social science that any question which might be asked about a society is at least answerable in principle. The uncertainty principles which intrinsically restrict the class of answerable questions in physical science settings somehow do not seem to have clearly enunciated analogues in social science. We regard this as a major knowledge gap and a research item--i.e., the delineation of broad classes of uncertainty principles--of major importance. There is already a nice start in this direction (Cohen, 1986), and we illustrate the issues with a simple example.

Suppose you are interested in comparing an individual's survival chances in two different countries. An obvious strategy is to seek a single pair of comparison groups that share as many as possible of that individual's characteristics. The goal of "as many as possible" runs into an uncertainty principle that limits the evidence that can be mounted to address the question of individual survival chances. In particular, the maximum number C of characteristics that can be used to specify two comparison groups and the minimum difference d in probabilities of death that can be detected in given populations are constrained by inequalities--i.e., an uncertainty principle--of the form

$$N(\rho_1)^C \geq \rho_2/d^2$$

where N is the population mean of two countries of comparable size, and ρ_1 and ρ_2 are constants where $0<\rho_1<1$ and $\rho_2>0$. (See Cohen, 1986:35 for more details.) The important feature of this inequality is that as the number of characteristics C increases so must the minimum detectable difference d, thereby demonstrating a tradeoff between refinement of population definition and precision of measurement. Matching on "as many as possible" of the characteristics, even if they exist in a data set at hand, can lead to the impossibility of detecting a difference at the level of precision attainable. Limiting the number of characteristics on which comparison groups are matched to account for the resolution of current measuring techniques is what is needed.

3.2 Theory Development and Testing

Lieberson argues that more social research should be directed to the goal of developing social theory. A primary requirement for this development to occur is the construction of formalized theories. Richer theories leading to the use of fewer control variables and setting up apriori relationships among variables would go a long way toward eliminating excessively complex use of control variables and thus freeing much social research from the controlled experimentation paradigm. Where appropriate, such theories should specify the influence of both deterministic and chance elements, indicating how exact a theory is expected to be in explaining a given outcome. The conditions under which the theory pertains should also be precisely specified and increasingly refined through a process of elaboration and testing. Carefully formulated theories are important because they provide the basis for development of a cumulative body of knowledge. Since there is now relatively little formalized theory in sociology, going beyond description is a major innovative exercise. Thus, although a wide response to Lieberson's directive concerning the development of theory is of central importance to the advancement of sociology, it will not be easy to bring about.

"Theories and/or data that create strong counterfactuals or disprove counterfactuals that we think are strong are major advances to knowledge. An important function of theory is to suggest, or actually provide, new counterfactuals or to disprove old counterfactuals" (Lieberson, p. 229). It is important in a wide variety of specific problems (research domains) to develop a body of maximally plausible counterfactuals upon which one can build and expand into other counterfactuals.

With this plea for major theoretical developments, we looked for a social science example that would suggest meaningful starting strategies in what is surely a monumental task. In our opinion a nice point of departure, well worth careful examination, is the development and empirical evaluation of the Heckscher-Olin-Vanik (HOV) model of international trade by Leamer (1984). This study begins with a new development of the HOV model as a theory of international comparative advantage and provides a rigorous derivation--from the theory--of a set of linear estimating equations. Leamer then develops a carefully articulated level of aggregation of traded goods across industries and presents data from 58 countries on 10 commodity aggregates and 11 productive factors in the years 1958 and 1975. He establishes relationships between resource endowments and trade in what is, in our opinion, an exquisite collision between an economic theory and empirical data analysis. In addition to providing a role model for theory development and testing, Leamer even concludes the book with a chapter on counterfactual experiments that will surely repay careful study. Again, this international trade investigation is only a beginning, but one which may stimulate theory development of the type Lieberson is recommending.

3.3 Clarifying What is Meant by Causality

The word "causal" has come to be used loosely in sociology and therefore has no precise meaning as a scientific term. Rather than continue to use this term as if it has meaning, we need to rethink what we mean by causality--both conceptually and operationally--and to clarify the circumstances under which we will agree that X "caused" Y. As a concept, causation is only one of several forms of determination. Other forms of determination include quantitative self-determination, interaction, mechanical determination, statistical determination, structural determination, teleological determination, and dialectical determination (Bunge, 1979:17-19). All forms of determination involve "constant and unique connection" among objects or events, but causation has the additional property of "productivity"--i.e., the production of change by external factors. Therefore, many relations in sociology which are now termed "causal" are not causal but involve other forms of determination. It should be recognized both that causality is not the only form of determination and that other forms of determination play an important role in scientific laws.

Lieberson is never precise about what he would regard as adequate evidence to say that X "causes" Y. A coarse but useful start at a formulation is presented by Mosteller and Tukey (1977:260-61):

> "Two or three sorts of ideas usually are required to support the notion of 'cause':
>
> 1. Consistency.
>
>   ▷that when other things are equal in the population we examine, the relation between x and y is consistent across populations in direction--perhaps even in amount.
>
> 2. Responsiveness.
>
>   ▷that if we can intervene and change x for some individuals, their y's will respond accordingly.
>
> 3. A mechanism.
>
>   ▷that there is a mechanism which someone might sometime understand, through which the 'cause' is related, often step by step, with the 'effect'--the sort of mechanism where, at each step, it would be natural to say 'this causes that'."

Only Point 1 can be confirmed by observation. Point 2 requires experiments including "natural experiments," and these tend to be rare in sociology. Point 3 awaits both the theory development and empirical testing discussed in the previous subsection.

For most social research these standards are currently--and are likely to remain for the foreseeable future--quite out of reach. Thus one is led to either dropping the terminology of "causal relation" or considering weaker, but nevertheless very useful, notions of it. In this regard the evolving notions of causality in epidemiology, starting with the Henle-Koch postulates --see the superb review articles by Evans (1976, 1978)--and proceeding to present day criteria involving immunological and molecular genetic evidence, are very instructive. Indeed the evolving--with technological advance and field discovery--criteria for causality where a given organism(s) is said to give rise to a given disease (or chain of diseases) in the epidemiological literature would be a useful starting point for thinking precisely about what one should mean by "causality" for social processes. In this connection Evans (1978:255) has already set forth a 1:1 correspondence between rules of evidence in criminal law and contemporary operational criteria for establishing causality in epidemiology.

Since virtually all statements about causality are assertions about the behavior of a social process over time, it is imperative that both theoretical formulations and empirical analyses focus on dynamical processes. From the perspective of data collection, this means that longitudinal data should be the standard form of empirical evidence utilized for studying social processes. The yeoman assumptions--almost always untested and untestable--which accompany much of contemporary cross-sectional research should be disregarded, and a new longitudinal research agenda substituted in its place. Of particular importance is the fact that any hope of distinguishing between asymmetric and symmetric causality lies in the direct study of social processes evolving over time. This, in turn, puts longitudinal data on center stage. However, it should be noted that longitudinal data in themselves are not sufficient to establish a causal relation. As noted by Bunge (1979:39), "the cause is existentially prior to the effect--but need not precede it in time." Even longitudinal data must be used with care when making inferences about causality.

As a final point it should be emphasized that the rethinking of causality being advocated here focuses on the phenomena under investigation and their measurement. Thus it stands in contrast to model-based notions of causality--

sometimes referred to as "causal modelling"--which dominate the contemporary social science literature. Ideally one would like to link these approaches; however, such a discussion would also require, in our opinion, incorporation of the various concepts of degrees of determination articulated by Bunge (1979). For an excellent review of model-based notions of causality, as contrasted with Bunge's discussion, the reader should consult Geweke (1986). Also, an instructive, earlier article is Hurwicz (1962).

3.4 Selectivity

Most contemporary social research that takes account of selection processes refers to this phenomenon with the negative connotations, "selection problem" or "the problem of selection bias." This vocabulary is a consequence of the fact that:

(i) Most program evaluation is carried out in a setting where a primary goal is extrapolation of conclusions about the effectiveness of an intervention to a target population that includes people who never self-select to be program participants (Heckman and Robb, 1986). In this situation strategies for bias adjustment become of paramount importance;

(ii) There is a pervasive emphasis on national surveys or, more generally, large heterogeneous population studies in which a major aim is generalizability of behavioral comparisons made on subpopulations whose existence is partially or entirely the result of a selection process to broader target populations. The aim of generalizability automatically forces consideration of a selection "problem."

The technology of selection bias adjustment is still in a relatively formative stage (Wainer, 1986), and this fact suggests that intensive development of the statistical methodology of such adjustments should dominate the research agenda on selectivity in the immediate future. In our opinion, a more productive activity would first involve a shift of philosophy in which selection processes as such are viewed as primary foci of investigations. Developing dynamical models of voluntary participation in manpower training programs, family planning programs, migrant farming, health insurance plans, and a diverse array of other activities would stimulate the production of refined theories of selection as a social phenomenon. For a nice example of this kind of investigation in the context of both legal and illegal immigration from Mexico to the United States, the reader should consult Massey (1985). This study deserves careful scrutiny for its sensitive mix of anthropological field methods with conventional survey techniques to get at a deeper understanding of voluntary participation in illegal Mexican-U.S. border crossing with an eye toward improved economic position for Mexican families. In our opinion, Massey's Mexican study is a good role model for a response to Lieberson's plea for closer examination of selection processes.

3.5 Knowledge Synthesis

The vast proliferation of studies of educational interventions, the impact of school desegregation on performance of blacks on achievement tests, the influence of various health insurance plans on health care utilization in elderly populations--to name only a few topics--has created a need for rational strategies for combining evidence from multiple sources to summarize current states of knowledge. This topic has a long history in the physical sciences--see, e.g., Cohen, Crowe, and Dumond (1957)--and has rather recently made a somewhat controversial appearance in the social sciences under the guise of meta analysis (Hedges and Olkin, 1986). In our opinion, this topic will be of increasing importance both for the development of new sociological theories and, most especially, in the policy arena where the production of timely knowledge syntheses is of critical importance. The U.S. General Accounting Office has, from its inception, been a knowledge synthesizing organization for the U.S. Congress. Here, the ever increasing demand for rapid and defensible syntheses of evidence on such diverse topics as housing subsidies and their impact on low income populations, quality control and its impact on the cost of military aircraft, and the economics of alternative energy resources has led to an important and innovative development of meta-analytic strategies (Cordray, 1986). In our opinion, further efforts in this direction should be of high priority because of the need for knowledge synthesis in theory development and policy planning.

4. Conclusions

Making it Count is an important and provocative book whose contents deserve to be discussed and debated on a wide scale. The message is that, without a thorough redirection of research efforts, little progress will be made on fundamental conceptual and methodological issues in sociology. The challenge is great, but so are the potential rewards. It is our hope that Lieberson's critique of current practices will be given a serious and thoughtful hearing and that some of the research topics that we have spelled out in the previous section will be taken up as a much needed response to Making it Count.

FOOTNOTES

1. It is a somewhat subtle matter to be precise about what one means by the word "experiment." See, for example, Campbell (1957), Bunge (1979), Feinstein (1985).

2. Although Lieberson speaks in terms of the social sciences generally, his focus is essentially on sociology (including social demography). Our review therefore focuses on this discipline, although many of our comments pertain to economics, social psychology, and epidemiology.

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