Annu. Rev. Sociol. 2002. 28:1–19
doi: 10.1146/annurev.soc.28.110601.141122

BARKING UP THE WRONG BRANCH: Scientific Alternatives to the Current Model of Sociological Science

Stanley Lieberson and Freda B. Lynn
Department of Sociology, Harvard University, Cambridge, Massachusetts 02138; e-mail: SL@wjh.harvard.edu

Key Words experimentation, evidence, evolution, Darwin, physics

> A fortiori *it is unlikely that a mere repetition of the tricks which served us so well in physics will do for the social phenomena too.*
> (*John von Neumann and Oskar Morgenstern 1944, p. 6*)

> *I do not think the relationship between theory and measurement in the social sciences is much like what Kuhn describes for physics. Talcott Parsons was right about the lack of interaction between the two in sociology. 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 . . . .*
> *But sociology is not like physics. Nothing but physics is like physics . . . .*
> (*Otis Dudley Duncan 1984, p. 169*)

■ **Abstract** The standard for what passes as scientific sociology is derived from classical physics, a model of natural science that is totally inappropriate for sociology. As a consequence, we pursue goals and use criteria for success that are harmful and counterproductive. Even those dismissing such efforts use the standards of physics as grounds for their objection. Although recognizing that no natural science can serve as an automatic template for our work, we suggest that Darwin's work on evolution provides a far more applicable model for linking theory and research since he dealt with obstacles far more similar to our own. This includes drawing rigorous conclusions based on observational data rather than true experiments; an ability to absorb enormous amounts of diverse data into a relatively simple system that did not include a large number of what we think of as independent variables; the absence of prediction as a standard for evaluating the adequacy of a theory; and the ability to use a theory that is incomplete in both the evidence that supports it and in its development. Other sciences are briefly cited as well, but the main emphasis is on the lessons that Darwin provides for social sciences such as sociology that obtain their evidence primarily from non-experimental sources.

INTRODUCTION

Ever since the establishment of sociology, various polemical and philosophical issues have surrounded the attempt to model a science of society after the hard sciences. Since the natural sciences differ in theoretical concerns, nature of evidence, and the special obstacles encountered in combining evidence with theory, these sciences also vary in their potential for generating ideas that could be useful for sociology. (Of course, the natural sciences at best are simply sources of ideas that might be helpful to us; under no circumstances are they templates that can be automatically adopted for usage in social science.) Our thesis is that deeply ingrained in sociology and other social sciences is a special model of natural science that is exceptionally inappropriate. It is derived from physics, particularly the classical physics that existed before the beginning of the twentieth century. Because of the impressive results obtained in physics and also its precision, it is understandable why this science would be favored as a model for the social sciences to follow. The use of physics as the ideal model for sociology is so embedded in our thinking that the influence and appropriateness of this particular model is rarely questioned. There are those, of course, who do criticize attempts to create a social *science* modeled after the natural sciences as pretentious, bound to fail, and intellectually sterile. Ironically, they too use physics as the model of hard science in order to demonstrate the error of such an attempt (see, for example, Flyvbjerg 2001). The appeal of classical physics is easy to understand; it offers very straightforward simple cause-effect connections that are very powerful and operate in an incredible array of situations. The actual path of a cannonball shot into the air, for example, is very close to the theoretical prediction that takes into account the angle of the cannon, the thrust of the explosion, the weight of the ball, and the altitude from which it is fired. For human societies, we have great difficulties making such simple cause-effect connections with comparable precision.

Our thesis is that other natural sciences actually offer epistemological and procedural models that are more relevant for the obstacles encountered in sociology and other social sciences: Their ways of interacting between data and theory are more suitable, and their notions about theory, causality, and explanation are also more pertinent for us. In particular, we focus on the early development of evolutionary theory. This highly successful natural science has much to teach us. In many dimensions it encounters obstacles similar to those in sociology, but evolution employs solutions very different from the ones we use. At the very least, we need to consider these approaches as an alternative to present-day practices. In doing so, we can actually revise our notions of the difficulties that occur when we pursue a very mechanical notion of science. Evolution and other natural sciences help us realize that what appears to be problematic and disappointing (when sociology is approached with physics as the model of natural science) is simply a consequence of utilizing an inappropriate model for studying social phenomena. That is, unnecessary difficulties or obstacles are created because an incorrect standard is in use. The absence of predictive power and high correlations, for example, are not a weakness of our theory (as Rein & Winship 1999 would have you believe) but should be seen as an inherent reflection of the way the social order operates and can best be understood.

Finally, the reader should note that we are not interested in applying biological models to social life (as in the notions of social evolution or social Darwinism) or promoting them. Nor are we concerned with biological questions about the hard-wiring of human behavior and the social order. Rather, our interest in evolution is strictly an epistemological one, looking at the very different ways theory and evidence are formulated and linked together in a natural science that confronts epistemological obstacles similar to those occurring in the social sciences.

DARWIN: A BRIEF REVIEW

The early developments of evolutionary theory provide an epistemological model of how a discipline, although facing many of the same obstacles commonly encountered in social science, is able successfully to combine sound evidence with a viable and useful theory. Arguably the early evolutionists encountered more obstacles similar to sociology than virtually any other hard science. The form of theorizing in evolution, the form of gathering evidence, and the form of interacting between theory and evidence have considerable relevance to issues encountered in sociology.[1]

Jones (2000, pp. xviii–xix) helps us understand the magnitude and importance of evolutionary theory: "Before Darwin, the great majority of naturalists believed that species were immutable productions, and had been separately created. Today, his theory that they undergo modification and are the descendants of pre-existing forms is accepted by everyone (or by everyone not determined to disbelieve it) . . . . The struggle for existence, the survival of the fittest and the origin of species are wisdom of the most conventional kind . . . . Although the notion is as simple as that of the solar system, Darwinism is not the obvious explanation of how the world works. Common sense tells us that life—like the Sun—revolves around ourselves. The idea has but one fault: it is wrong . . . . *The Origin of Species* is, without doubt, the book of the millennium."

The evolutionary model can be characterized as a two-stages process: the production of variation followed by the sorting of this variability by natural selection, the critical mechanism or motor in evolution. In more detail, the stepwise process is as follows: First, among individual organisms, there is variation, both natural (i.e., random) and domesticated, some of which is heritable. Second, organisms in nature generally produce offspring in excess of the number that can reach the reproductive age because of competition. Assuming a stable population, a "struggle for existence" ensues among the offspring for survival. Organisms with certain variations or characteristics are favored during this struggle. Those who are favored or naturally selected will thus have a significantly better opportunity to reproduce

[1]The substantive ideas also influenced social scientists such as Marx, Engels, Comte, Morgan, Spencer, and Weber (Antonio, 2000, p. 1782; Richter, 2000, p. 877; Cohen, 1994).

offspring and thus later descendants who possess some of their traits and variations. Over time, beneficial (i.e., well-adapted) characteristics will be accumulated in the population through the process of natural selection. Finally, over great lengths of time (i.e., geological time), new species may evolve in a manner congruent with this process of variation and natural selection (the discussion in this paragraph and the following one draws in part from: Jones 2000, Gould 1977, 1982, Bonner & May 1981, Mayr 1967, Simpson 1950, Lasker & Tyzzer 1982).

Evolution employs a set of powerful mechanisms to help one understand finer and finer parts of the story. In doing so, it helps the theory deal with the variety of specific types of causes that operate in various circumstances but are not always factors. Genetic drift or migration, for example, do not always occur, yet they are critical in some instances. These mechanisms serve to deal more successfully with a complex system in which there are often different crucial conditions present. In sociology, some of us try to control for all of these through statistical means; others try to treat each as an historical event that shares a common major condition. Here we have a solution that is far more promising. A variety of mechanisms may be called into play. They have something in common: They all fit into the overriding theory and support its goals; in this case they are part of the theory of evolution. One is not bothered if not all of the mechanisms are relevant to any specific case. However, the mechanisms are stated in generalized form as principles or theories. Evolution operates with neither unique historical events reflecting unique historical causes *nor* a crass mechanical approach that evaluates a mechanism by how often it operates or how much of the variance it accounts for. A mechanism is still pertinent regardless of how often it operates to generate changes in the survival of a species or a species' growth or decline in numbers. The mechanism is not evaluated in terms of the variance it explains. This bears greater consideration in our own enterprise (see Richter 2000, p. 876 for a distinction between individual history and the unique history of evolution). Sequential analysis is central to evolution. Namely a process of events is traced, as say leading from an environmental shift to a genetic shift as the living entity adapts to the new condition. A massive shift such that a new species appears would involve many steps along the way. Tracing these changes would provide evidence to support the claim.

LESSONS FOR US

Neither Right Nor Wrong

In reviewing developments from Darwin to more recent periods, we are struck by how the theory has a tolerance for problems and incompleteness that gives it a certain durability and that enables one to better cope with errors. The evolutionists do not confuse fatal errors, on the one hand, with problems stemming from incompleteness, information that is still insufficient or not yet determined, or even unresolved. The latter cases are worrisome and certainly not to be glossed over. Yet, it does not necessarily mean that the theory is to be abandoned or that Darwin was *wrong*. In evolution, *incomplete* is not the same as *erroneous*.

The excerpt below from Darwin's *On the Origin of the Species* is valuable to us because it is hard to imagine a sociological theorist being able to admit to such difficulties in a newly proposed theory. Read this not for the content of the theory or the missing information, but as an example of a way of dealing with evidence that rarely occurs in sociology:

> *There is another . . . difficulty, which is much more serious. I allude to the manner in which species belonging to several of the main divisions of the animal kingdom suddenly appear in the lowest known [Cambrian-age] fossiliferous rocks . . . . If the theory be true, it is indisputable that before the lowest Cambrian stratum was deposited, long periods elapsed . . . and that during these vast periods, the world swarmed with living creatures . . . . [But] to the question why we do not find rich fossiliferous deposits belonging to these assumed earliest periods before the Cambrian system, I can give no satisfactory answer. The case at present must remain inexplicable; and may be truly urged as a valid argument against the views here entertained.*
>
> (*Schopf 2000, p. 7, quoted from Darwin 1859, Ch. X*).

To be sure, through the century there were debates about Darwin's conjectures, but the theory survived uncertainty and incompleteness during that long span. It was only one hundred years later that fossils were uncovered that fully supported Darwin's conjectures (Schopf 2000, p. 18). One might argue that this is different from the incompleteness and uncertainty existing in sociology because in Darwin's case there was reason to hope that eventually data (i.e., fossils) would be uncovered to provide missing information about the past that could furnish information about the period in a definitive way. However, there is every reason to assume that additional information and understanding can be obtained about virtually any topic that sociology concerns itself with as well. It just takes work and patience.

Like sociology, evolution deals with complex evidence. Both are exposed to two enormous problems: (*a*) Given the lesser certainty that we have in working with the data that are available to us, we must use different standards from the conventional ones, and (*b*) in order to be sure that what we say is valid, we cannot rely simply on variance explained by models of evidence or act as if we are dealing with a sample from the universe. This is also a problem for qualitative small-N studies where a "full explanation" is the standard for knowing that the investigator is on track. On the other hand, if we are not using simple rules for accepting and rejecting, how do we avoid taking bad ideas and using them to expand onward and onward, further and further from the true path. In other words, how do you set standards that enable one to separate the wheat from the chaff? We have no immediate answer here, but a conviction that part will be intuitive, as it always is, and part will require us to develop clearer thinking about when the evidence truly is consistent with the theory, when it is irrefutably inconsistent, and when we have to say that "the evidence is not all in." We observed earlier how Darwin recognized huge holes in what he knew and what he could account for. How would we evaluate a theory with such gaps? Under present-day standards, how often could/would a theorist admit lack of full knowledge or understanding? How do we implement an epistemology that permits softer and yet harder standards?

A Successful Theory

Note also that Darwin's theory is a successful theory, but it is not a static theory. While there is every reason to read Darwin for historical reasons or to gain an admiration of his thinking qualities, his theory of evolution is not the same as the theory that is taught now. It is not necessary to read Darwin in order to work on evolution since the discipline has gone long past him. This is of interest because in social science we talk about theories being either right or wrong. It is not that simple, and moreover, it is a gross distortion of how a theory should change. Keep in mind the view of a theory as active process involving an interaction with known facts and then in turn providing generalizations that go beyond these known facts. So, as we know more through new observations and also as we reach new problems based on what we believe we already know, it is inevitable that the theory should change or at least be modified. It is a matter not of simply rejecting a theory, but rather of evaluating a theory, knowing at some point that it will have to be modified or even superseded. Put another way, if Darwin was *wrong*, then we should all be lucky enough to be as wrong as he was. Since Darwin was writing at a time when nothing was known about mutations and genetics, how could he possibly have a complete theory? In social science, we are more likely to want to destroy a theory or, at the other extreme, worship a theory and therefore resist its change or modification. This is very different from asking "What can I get from this idea?", "Where is this helpful in dealing with observed events?", "How can I modify the idea such that I keep the parts that are useful and yet expand its application?" Of course, one has to know when to give up and when something is genuinely useful and merits modification and further exploration.[2] In any case, as one goes on in the development of a theory of evolution, more and more detailed principles and mechanisms develop to explain finer and finer parts of the theory.

Way of Working

The formal way of writing up one's work is misleading because it is typically presented, particularly in articles, as an orderly product starting with a problem, usually derived from a theory and/or a social problem, and ending with conclusions based on the ensuing investigation. This is unfortunate. The formal way is indeed practiced in the sciences, but it is hardly the only pathway. Often scientific pursuits and sociological pursuits are much more intuitive, scattered, nonlinear, informal, and trial-and-error than the published literature suggests. Darwin is an interesting model on that score; his notebooks show that his process was for the most part nonlinear with unordered sequences of "theorizing, experimenting,

[2]Of course, always relevant is the internal logic of the theory, its application to specific empirical evidence, and matters of that nature.

casual observing, cagey questions, reading, etc [that] would never have passed muster in a methodological court of inquiry" (cited from Gruber in Bonner & May, 1981, p. xiii). Darwin himself characterizes the pursuit as the struggle toward "seeing the bearing of scattered facts." We fail to recognize a distinction between how many natural scientists and social scientists actually do operate as opposed to their formal style of presenting outcomes. We do ourselves a disservice when teaching methodology to our students as if the highly stylized form of presentation in journals and books is the same as the intellectual processes that lead to these conclusions. This actually encourages a more mechanical model of how knowledge progresses. This, along with the tendency to base our conclusion on only one data set, actually fits very well with a mechanical model of what our theories should be like. However, as was the case in evolution, it does not help us master the incredibly complex universe in which the social sciences operate.

JIGSAW PUZZLES, DETECTIVE STORIES, AND WHITE LAB COATS

Like sociology, evolution deals with a huge number of conditions that can influence the topic of interest. And, like sociology, much of the basic knowledge, at least certainly what Darwin had to work with, was drawn from observing naturally occurring events, that is, not from true experiments. So we have two issues here: first, how does the study of evolution manage to survive so well in dealing with the same type of complex and diverse sets of causal conditions that appear in a very large set of combinations and permutations, whereas sociology has trouble managing such data other than through very complex statistical models or through small studies that depend on deterministic and mechanical outcomes driven by a small number of conditions? Second, how can the students of evolution get along learning so much more from observational data than we do, even though sociology is saturated with procedures designed to estimate true experimental results from the nonrandom observational data that we usually have?

We have several speculations about this. One of the great accomplishments of Darwin was that he narrowed the problem down to a specific question. Certainly, he worked on a problem that was huge by any and all standards, the origin and changes in species, but it was still a restricted problem. It did not encompass all possible questions that you could ask about evolutionary biology. Indeed, one of Darwin's great leaps was that he never attempted to account for how variations arise, how variations are maintained, and how variations are inherited. His solution was simple: He took these as given and did not try explain these facts (it was through genetics that they were later resolved). Sociology does not seem to be as focused on a central big question in society. As a consequence, it is less likely to generate cumulative knowledge. But probably the biggest difficulty is that sociology attempts to simulate true experiments, hence the use of a variety of statistical techniques designed to accomplish this. Darwin's evolution worked to achieve similar understanding through situational observations that enabled him to coax out the answer. It is as if he were dealing with a 5000-piece jigsaw puzzle or were a detective called in to investigate a puzzling murder. Most sociologists do not deal with data in Darwin's way. Rather, they emulate the logic of the true experiment.[3]

Complexity and Experimentation

Evolution can also help us with one of the most difficult problems that sociology faces. There are an almost infinite number of conditions or influences on the dependent variable (to use the contemporary language of sociology). If a survey generates a complex analysis where, say, fifteen variables are taken into account, it is perfectly acceptable in contemporary analysis to propose that a sixteenth variable should also be considered. There is always the possibility that "controlling" for an additional attribute might completely alter the conclusions previously reached. Carried to the extreme, we are not in danger of approaching a slippery slope—we are on it. Of course, there never is full certainty. And that absence of full certainty will not be resolved if we double the number of variables.

What happens in evolution that is different? First of all, the underlying concern is defined in a much sharper way. It is not the scientific study of all living entities in all manifestations. It deals with a massive problem, to be sure, but it is structured in its own way with a modest number of *principles* rather than a virtually infinite number of *possible variables*. These principles are brought into play over and over again. Moreover, because we are implicitly following the logic of the true experiment in which there is random assignment and everything is therefore controlled, we are keen to have all of these variables considered. Our goal is to approximate the true experiment.[4] We can understand why the experimental simulation appears desirable, but this is not cost free. Clearly the experimental analogy makes one worry about too many things at the same time. The attempt to attain simplicity, in which the influence of one causal variable is examined net of other causes, or in interaction with a specific set of conditions, ironically requires the introduction of a very complex set of steps and controls. Darwin got around this by what we would now call quasi-experimentation in which he looked for naturally occurring conditions that enabled him to reason as if nature had performed the experiment for him. This is an overstatement on our part, but it is pretty much how it works out. Of course, experiments do occur in evolutionary studies, and there is greater movement toward experimentation (see Jones, 2000, p. 73), but many of the fundamental truths were obtained without them.

As noted earlier this is more a detective story than a mechanical statistical process. Quasi-experimental data are a terribly neglected way of thinking in

[3]This can be perfectly appropriate in small group experiments and an occasional social policy experiment in which there is a true random assignment.

[4]In point of fact, even if there were a true experiment with random assignment, a number of other considerations could limit our confidence in the apparent outcome. This topic, however, would carry us far afield from the issues of concern in this paper (see Lieberson, 1985, Ch. 3).

sociology and should be considered more often. With quasi-experimental data, we use situations that differ and where the source of that difference is unrelated to the dependent variable (in effect, there is no selection effect). This enables us to use different data sets to create, as it were, multiple studies based on the existence of different conditions. Suppose, for example, we are interested in the influence of natural disasters, such as tornadoes, on social life in towns. If we are confident that the nature of towns located in the tornado belt does not affect the likelihood of a town being hit by a tornado, then we can compare those hit with those escaping the disaster and be confident that the differences solely reflect the influence of tornadoes *and not other characteristics of the towns*. (Namely, for all purposes we have random assignment and hence a true experiment as if we had blindly assigned some towns to the test condition and others to be controls.) This is an attractive alternative to using one data set where an enormous number of controls are applied back and forth to get at the truth. Now this is an easy example and clearly involves the equivalent of random assignment. Although it is not clear how far we can go in obtaining data for many other social phenomena in this manner, there appear to be few efforts to use such quasi-experimental situations.

Another feature in evolution is that it operates with a set of mechanisms that are less complicated and more manageable than a set of measured variables. Darwin used theories and mechanisms that were not directly measured and correlated. Rather, he examined the expected empirical consequences of these ideas. This is different from when some consequence is measured and in turn correlated with a variable meant to represent the theory. In evolution, there is no restriction to using a theory or a mechanism even if it is not directly measured. If the outcome occurs according to the theory, then that is what counts. Ironically, in the effort to be scientific, sociology is sometimes too literal.

Finally, a theory that focuses on the nature of a population distribution, rather than each individual unit within the population, is a simplifying theory that enables the investigator to avoid what is best thought of as the *noise* or *static* in the observations. The terms noise and static do not necessarily mean that such effects are inexplicable, although they may be, but rather that they are best viewed as just random interference. To use the magnificent model from statistics (a model that qualitative social scientists can appreciate on esthetic grounds even if they are not inclined to use it), there is no concern with why one coin turns up five heads when flipped five times, whereas another series yields four heads, and another yields three heads, and so forth. The concern is with the distribution of outcomes from a series of five tosses with each coin. Ironically, sociologists may be inclined to say this is fine as far it goes, but a full explanation *ought* to take into account each coin as well. In point of fact, as we dip into such detail, we make it progressively harder to develop principles and propositions that will be useful in accounting for the question at hand, the distribution of a population characteristic. If a species of birds living on side of an island begin to differ on the aggregate (say in the darkness of their feathers) from those living at the other side, and we find that environmental conditions are changing on one side in a way that is harmonious with a given mechanism, then that is understandable. It is not necessary to account for why some birds under the changing conditions still have lighter-colored feathers.

PREDICTION

There are two features of evolution that, at first glance, appear to be distressingly different from the common standards in social research. On the one hand, the social sciences are concerned with prediction; a good theory is one that enables us to predict. This often takes the form of accounting for already-observed changes, or sometimes of evaluating conflicting theories when there are contradictory consequences that would be deducted from each theory. Which one correctly accounts for the specific outcome? This focus leads to concern about the predictive power of a theory. Evolution does not evaluate its accomplishments in terms of this goal (see Mayr 1961, Scriven 1959). Evolutionary theory is unable to say what a particular species of birds or ants or ferns will be like one hundred years from now. There are too many conditions in the system that are hard to predict so far in the future and that can easily have a bearing on the outcome (extinction being one such possibility, by the way).

So, we have a problem here. Evolution is a far more developed field than is sociology or, indeed, just about all of the other social sciences. Yet, evolution is generally satisfied with explaining past events and not attempting for the most part to make theoretically driven predictions into the distant future. Why is this the case? In our estimation, it is the fact that sociology's notion of prediction and deduction from theory is based on the classical physics model in which determinism seems to work pretty well, but that most natural sciences have a much more modest goal. As a consequence, we are again trying to do something that we have no business expecting to be able to do, at least in a world of complex influences that are not restrained as they would be in a true experiment. This again leads to unrealistic goals and forced efforts to have tests of predictability that are not appropriate, as well as criticisms from those dubious about the possibility of sociology being a science because they implicitly use a standard from classical physics rather than a more realistic standard of how it works in the wider set of sciences. Ironically, in the case of prediction, this is difficult in almost all of the natural sciences, given that they work with probabilistic situations. Indeed, there are parts of physics that have the same problem. Just think about this: here is a standard used without challenge in sociology even though more advanced fields have a difficult time with this. Before we try to address this issue, we should consider whether the goal of prediction is attained very often in sociology, particularly if we are doing something more than applying a short-term projection of a long-term trend that we observe. That, by the way, is not a prediction based on theory, it is a projection based on a graph. Likewise, applying a retrospective prediction is not quite the same, especially when we toss in a number of independent variables to account for the outcome variable and then convert this correlation to a theory based on the independent variables that seemed to work. This is not exactly the same as making predictions for events twenty years from now by deducting what the theory would predict, locking these predictions in a box with guards preventing access, and then opening the box twenty years later to see how it all worked out.

Rather than worry about all of science, sociology should consider several distinctive features of evolution that lead evolution to avoid predictions. First of all, we noticed that evolution has a specific question and a specific overriding mechanism (survival of the species and its successful reproduction in succeeding generations). To accomplish this, adaptation to a constantly changing environment is necessary. In adapting to such an environment, the species constantly changes. The mechanism involves no intent of any sort; it is an inherent outcome: Changes occur at all times thanks to mutations, but the issue is whether any of these changes will permit the species to adjust to the new survival conditions. If so, then the species will survive; if not, there will be extinction or a decline in numbers or in spatial range reflecting these new conditions. In order to make predictions under such conditions, one would have to predict the entire universe of events that would have a bearing on adaptation and survival. Moreover, since mutations can be viewed as chance events, evolution would have to predict a set of chance events not only for the species under consideration but for all other species that are relevant as predators or prey. The specifics are different for the social order, but chance is not (see Lieberson, 1997a,b).

As the subheading of Scriven's 1959 paper puts it, "Satisfactory explanation of the past is possible even when prediction of the future is impossible" (p. 477). This is something to keep in mind when both the proponents and critics of social science want to use prediction as a standard for estimating the success of sociology and other social sciences.

THERE IS MORE THAN ONE KIND OF CAUSE

There is a distinction between types of causes that is typically ignored in sociology, even though it is a central one. Essentially it is the distinction between deeper underlying causes that lead an event to occur even in the absence of the immediate cause. For example, suppose whites seek to advance their interests. However, if a law is passed that eliminates one source of their advantage over blacks, would that gap now disappear? Perhaps sometimes, but more likely the gap would not decline anywhere as much as the statistical analysis prior to the law would suggest. This is because the dominant group would seek other channels in order to continue to enjoy its dominance. This distinction runs under several different names: in logic, it is between *remote* and *proximate* causes; in sociology there are *precipitating* and *underlying* conditions (Lieberson & Silverman, 1965) or *basic* and *nonbasic* causes (Lieberson, 1985); or, in epidemiology, *fundamental* and *intervening mechanisms* (Link & Phelan, 1995). In biology, it is the distinction between *proximate* and *ultimate* causes (Mayr, 1961, pp. 1502–3). Mayr provides a fine example in the question of what causes a bird's migration. Specifically, why did the warbler at Mayr's summer place start his migration on the 25th of August? He has four specific causes that appear equally legitimate. Now, this seems rather straightforward to us in conventional sociological thinking. The difficulty in this conventional mechanical response is that some causes are proximate, in this case conditions that set off the bird's migration at the specific time observed; and two are ultimate causes such as would be understood through the mechanisms of natural selection that developed through thousands of generations. It is a mistake to confuse them or to merge them with one data set. And this is not simply a problem because our N is not big enough. Both are reasonable questions, and "the functionalist biologist would be concerned with the analysis of the proximate causes, while the evolutionary biologist would be concerned with analysis of ultimate causes."

This distinction is important for us to apply more regularly because, as Mayr (1961, p. 1503) observes for his own discipline, many heated debates about the cause of a phenomenon "could have been avoided if the two opponents had realized that one of them was concerned with proximate and the other with ultimate causes." Evolution does not view all causes as potentially equal, but rather operates with a causal hierarchy so that broader conditions are necessary for narrower conditions to operate. As a consequence it is important to distinguish tightly between basic and nonbasic causes. It would be terribly helpful for us too.

HISTORY

Evolution provides a perspective on history that is unlike anything we typically think of as history in the social sciences. First of all, evolution in one sense is a history, since it attempts to account for the survival, disappearance, and changes of species under a constantly changing set of conditions. This is quite different from standard history, however, because the interpretations involve the application of the same set of evolutionary principles and mechanisms under all times and places. Purely ad hoc or improvised explanations would not be acceptable, unless of course they are newly suggested mechanisms with application to more than the very narrow development that is currently observed. This emphasis on mechanisms merits consideration for those sociologists engaged in historical analysis when they have no concern for generality—note we say "generality" not "universalness." There are important probabilistic elements in this history. It is by no means assumed that what exists now would always develop if we rolled history back to some earlier point and let it progress. Because mutations are largely chance events and, in turn, play an enormous role in the changes within a species as well as the development of new ones, there is no reason to think that the appearance of homo sapiens or any other species was inevitable given an earlier starting point with exactly the same conditions as operated previously. Actually, the role of mutations is such that the plant and animal environment for a given species could easily be radically different from what had been.

There is a dimension to evolution's perspective on past and future events that is highly relevant to much of what currently passes as theory testing, or the evaluation of the validity of an explanation offered to account for an earlier event. If the conditions responsible for a given species were to turn back to their earlier state in every respect, evolutionary theory would not expect the existing species to regress back to its predecessor. That is, if the conditions responsible for the evolution from ancestor to giraffe, for example, were in all ways to reappear, we would not expect the giraffe to evolve into what its predecessor was like. This is because evolution is an asymmetric causal process and would therefore be similar to the principles of asymmetrical causality described for social phenomena (Lieberson, 1985, Ch. 4). This is referred to as irreversibility in evolution (Simpson, 1950, pp. 266–67).

There are several reasons why this would be the case: Since the earlier period when the pre-giraffe mammal evolved into the giraffe, a wide variety of other conditions would have changed as well. Hence even if the specific conditions that led to the giraffe returned to their earlier state, there would be other conditions, not relevant then but relevant now, that would prevent such an event from occurring. When one also includes the chance factors linked to mutation, then it is even more improbable that the ancestor would reappear. Moreover, if we ask the question of whether the giraffe would return even if *all* conditions of its development were restored regardless of whether each was deemed a cause for the evolution of the giraffe, then we are dealing with a logically contradictory statement. If all conditions were as they once were, then we would be assuming that the giraffe does not presently exist because its existence is a condition that didn't exist prior to its existence. So we are asking if the giraffe, which did not play a role in the conditions leading to its existence, would be irrelevant now to the conditions leading to its change. This is not just a philosophical argument, it is relevant for us as well.

Returning to the issue at hand, this asymmetrical perspective provides a strikingly unexpected consequence for social analysis that is normally not viewed as particularly historical. Typically, we ask if an increase in the value of X seemed to lead to a decline in Y, shouldn't Y reverse when X declines (we could change these statements to refer to presence vs. absence of conditions, to show it is also applicable to nonquantitative studies as well). However, this is actually an example covered under the model of asymmetrical causality. Even if the presence of X was the cause of a decline in Y, then a reversal in X need not lead to a reappearance of Y. As a consequence, this particular way of analyzing a causal conclusion only works to the degree that the causal linkage is reversible. Moreover, it provides us with a different way of understanding the way social processes operate. (For a less biological and more sociological angle, see Lieberson, 1985, Ch. 4).

TIME

At two different points earlier in the paper we referred to the geological scale of time under which evolution operates. Changes leading to new species can be very gradual, insofar as broad environmental conditions change slowly—radically slowly, from the perspective of the life span of plant and animal life as we know it. There are also theories about relatively rapid changes involving sudden jolts, but that should not keep us from recognizing a serious void in sociology and other social sciences. Namely, we have no sense of time as a dimension to any causal relationship. There are certainly situations where a change in one condition leads to a virtual instant change in another condition (consider the many innate and conditioned reflexes occurring among humans and other animals). And certainly, this does occur in nonphysiological instances. However, there is no reason to expect the general situation in the social order to yield instant responses to a causal condition. Since we usually have no strong temporal theory, we have no ability to deal with situations in which an outcome does not rapidly occur when the causal condition posited by a theory or mechanism would lead us to expect a change. There is no problem if the change is relatively immediate, but there is a problem if the change takes, say, ten years or twenty years (or even longer) to come into play. Evolutionary theory provides us with an example of how vital it is to have some understanding of the time lags that undoubtedly operate in the social order as well. It need not be in the magnitude of geological time, but a theory or mechanism is hardly wrong if the change is not instantaneous. We wish we could be more helpful in suggesting ways of dealing with this issue in sociology. At this point, all we can do is call to the reader's attention the fact that, although we know lags exist, we have no idea about their timescale. Likewise, it is important for sociologists and others to recognize that there is nothing in sociological theory that suggests that it is inappropriate for sociology to consider relationships other than those involving very short delays. Of course, the problem is that infinite delays or unspecified delays can keep us waiting forever on tenterhooks for something that may never come. An added possibility is that at some later date newer conditions may short-circuit the earlier process. That is, it will work its way through only in the unlikely case that other relevant conditions do not change in the interim. In any case, the point is clear: Sociology has not worked out temporal lags as a feature in many mechanisms and social theories.

CLASSIFICATION AND TERMINOLOGY

Classification schemes and the development of appropriate terms are extremely important in evolution as well as other natural sciences. There is no shortage of terminology or classification schemes in sociology either. How useful are they?

Evolution provides us with two important lessons. We have to think more closely about superficial as opposed to deeper classifications. Consider three kinds of bears: black bears, koala bears, and panda bears. The same word "bear" is used for all three animals and, although we can readily tell them apart, there is enough similarity to justify our calling all of them bears. This is popular usage, but the three are profoundly different when considered structurally. Koalas are marsupials (bearing their offspring in a pouch). Pandas, although they also suggest bears, are related to and resemble raccoons, and black bears are entirely separate from either of these animals in terms of their place in the system of mammals. Likewise, bananas are an herb, what we call a banana tree is not at all a tree, and strawberries are totally unrelated to other berries.

We have a special problem in sociology because our disposition to use new words to describe phenomena that are presumably suitably described by obvious and commonplace terms is notorious—and in many cases, the criticism of jargon is justified. On the other hand, there are often important reasons for not allowing popular terminology to confuse our scientific efforts. We should guard against adopting popular classification schemes originating in popular discourse. In the same way that natural scientists discern that whales who swim in the sea are not fish but are mammals that have evolved to meet the demands of their habitat, we too need to make distinctions that go beyond superficial observations.

Are students of race and ethnic relations putting too much into *race* or *ethnicity*? If you start with all the groups that are now or have been popularly labeled as a race or call themselves a race, is this the equivalent of pooling koalas and brown bears and pandas together and then trying to figure out the nature of bears (or racial groups or ethnic groups, and so forth) with an answer that is only acceptable or convincing if it can incorporate the fact that various populations are in this category? We do have some efforts in this direction, but hardly anything approaching the great classification system found in the taxonomy proposed by Linnaeus (for a discussion of the widespread deprecation of Linnaeus's work when it first appeared, see Gould 2000). And, by the way, the periodic table in chemistry is another example of an extraordinary ordering, on the atomic level.

A model and a caution are both called for here. The model is that organizing phenomena can be an exceptionally valuable contribution to knowledge, or it may be of little value. The key (assuming the schema holds up logically and empirically) is the *use* that can be made of the schema. Does it simplify and connect observations in a way that helps us move toward understanding or explanation for the problem at hand? Or does it just sit there, of no obvious value at this point in time? In any case the schema has to be more than a set of labels without order or structure. This is a difficult task, but the potential intellectual payoff is enormous. In any case, sociology and any other discipline that deals with phenomena that are part of popular language have to take great care in deciding whether to keep these terms for scholarly work or to substitute for them terms that avoid existing usage because the terms cause a problem by incorporating phenomena that appear to be similar in form but have different fundamental structures (homomorphy). Or, terminology may cause a problem in the opposite direction, by applying different words to phenomena that are basically the same. All of this involves an ability to distinguish between pointless and crucial terminology.

NORMATIVE ISSUES

Both presently and in the past, evolutionary theory and sociology have had difficulties when they clash with the norms held by a substantial part of the population. Certainly the theological resistance to evolution was massive at the outset (helped along by distortions of what evolutionary theory said). In the United States there is still a nontrivial resistance to evolution in several forms. To name a few: "it is just a theory"; "who actually witnessed these changes?"; "the bible says . . ."; "I can't believe that we descend from monkeys." Moreover, a competitive theory, creationism, claims to provide scientific evidence in contradiction to the evolutionists. Sociology has a problem as well: Many ideas are resisted by forces within society. At the annual meetings of the American Sociological Association in 1964, the senior author presented an empirical paper on the immediate precipitants and underlying conditions responsible for race riots. The study included evidence to suggest that race riots were more likely to occur in cities where there were a relatively small number of blacks on the police force (Lieberson & Silverman, 1965). Although this is hardly a shocking result by contemporary standards, J. Edgar Hoover, reading the FBI summary report on this talk, dismissed as "typical sociology" a statement that would barely raise an eyebrow at present (Keen, 1999).

There are two lessons to be drawn from evolution. First, a new idea that runs counter to existing ideas that are deeply felt, an idea that is hardly conclusive with respect to its evidence, is likely to generate enormous resistance. Indeed, if the idea runs counter to the norms, there can be great resistance even if the evidence is strong, and this resistance can continue on, hopefully in diminishing degrees, for generations. Its acceptance and diffusion, however, can be helped by those who present and explain the ideas to the general public. Sociologists who denigrate or look down on efforts to present controversial ideas to the public are badly mistaken. (It is another matter when popular work seriously distorts our results or makes claims that are invalid.) On this score, introductory undergraduate courses should not be relegated automatically to the teachers lowest on the totem pole, particularly when they are graduate students. Such courses are an extremely important facet of the presentation of sociology to the larger society. After all, crucial to the standing of evolution is that it is widely supported by the highly educated population and that in turn serves to help protect this powerful intellectual contribution from attack. (The very best and most effective teachers in each sociology department should be encouraged to give our large introductory undergraduate sociology courses. There is no better reason for reducing other assignments and other forms of teaching to encourage those with such skills.) Also, there is not sufficient appreciation in sociology that weakly documented and weakly supported results are not the same as sound results when addressing public issues. We should appreciate and encourage those who present results to the larger public in as rigorous a way as possible. (An excellent and most promising development in this respect is the American Sociological Association's new journal, *Contexts*, dedicated to a wider audience.)

THE ROLE OF OTHER DISCIPLINES

Darwin's work was greatly dependent on the work of geologists, particularly the account of the inorganic world of land, sea, and climate changes (Hodge, 1987, p. 239). Likewise, discoveries in genetics played a crucial factor in building evolutionary theory past the limits of Darwin's knowledge. (Knowledge of mutations was a central step forward in understanding the mechanisms of evolution.) The relevance of this for sociology may not be immediately obvious. However, it is very important insofar as there is an amazingly widespread disposition to rule out the use of other disciplines. Economists, we notice, are often cited by sociologists but it is usually only to serve as a foil—as an example of a field that is missing the mark and the ideas of which are wrong once a sociologist turns to the real issues, and so forth. There are exceptions, to be sure, such as rational choice theory and the use of econometric models. Yet this viewpoint is strange and painfully out of date. There is a constant crossing of boundaries in the natural sciences as they take advantage of closer linkages in topics, concerns, and overlapping contributions to a common enterprise. This is not something that can be preached or legislated, but it is a bias that merits our attention and necessitates an avoidance of a reflex-like rejection of work that makes *too* much use of other disciplines. We should be aware that we are in search of knowledge and therefore should be happy to use whatever will help us. Indeed, even casual observation will show that many disciplines are happy to borrow ideas first developed in sociology. It is not a sin for them, and it should not be grounds for objection within our own discipline for doing the same from other fields. If it is relevant and helpful, why must such knowledge be ignored or rediscovered by a sociologist?

OTHER SCIENCES

Evolution is not the only discipline that can provide alternative scientific ideas that could be useful and help us break away from an inappropriate model. Two illustrations are in order.

Chemistry reveals a much broader realm of scientific explanation and causality than we typically pursue. The chemist's investigation of material structure and material transformation stands in contrast to the sociologist's investigation of change in the social world. While sociology remains remarkably centered on the question of whether changes in Cause X increase or decrease Outcome Y, the practice of chemistry revolves around the idea of X and Y reacting with each other, in which case both X and Y undergo transformation (i.e., a chemical change versus a physical or superficial change.) Moreover, while social scientists ascertain the relative strength/magnitude of X's impact on Y as a means of developing causal arguments, chemists seek sequential explanations detailing the process of transformation. Social policy comes to mind as an immediate beneficiary of this approach. Rather than simply asking what is the effect of a change in X on a given condition of social concern, it would mean asking about the entire set of reactions to such an event. Hence, there would be direct consideration of the impact on all facets of society including the consequence for X itself, as well as what could be considered negative side effects.

Efforts in meteorology to understand the weather, let alone forecast it, are another example that involves special problems faced in sociology. From an epistemological standpoint, social science and parts of meteorology have similar problems as they endeavor to make sense of a remarkably dynamic and interactive system. In both cases, there are less-than-perfect observations coupled with heavy reliance on observational data from the past and present. Moreover, the atmosphere is an inherently dynamic system governed by the continual interaction of its elements. Over time, the "winds change the winds" so to speak; the motions of the atmosphere give rise to the motions that follow, and small disturbances often grow into massive disturbances. This is hardly different from what confronts the sociologist studying the dynamic and constantly changing social order. The impressive developments in meteorology in recent years stem from an expansion of both the quality and extensiveness of data on the weather, due to satellites and other technical achievements in space. This bears closer inspection for its relevance to sociology.

CONCLUSION

We hope this paper will lead others to question some of the standard practices in sociology that are drawn, implicitly, from the model classical physics provides of science. In our estimation, pursuit of these models generated from our image of classical physics is in many cases damaging to the discipline. At present, there is a profound neglect of natural sciences that entail very different notions of evidence and that are likely to provide far more valuable models for thinking about social processes. These approaches are not only fruitful elsewhere, but also have features that are closer to our own obstacles and therefore have the potential of being productive for us as well. One caution is in order: View them as *suggestive* for thinking about ways of handling our special concerns and obstacles in social science. Evolution and some of the other natural sciences are *closer* to our own, but they are hardly identical.

ACKNOWLEDGMENTS

We gratefully acknowledge helpful suggestions from S.M. Miller, Peter V. Marsden, Joseph Boskin, and David Frank. We also thank Andrew Doolittle for his help.

The *Annual Review of Sociology* is online at http://soc.annualreviews.org

LITERATURE CITED

Antonio RJ. 2000. Materialism. In *Encyclopedia of Sociology*, ed. E. Borgatta, R. Montgomery, 3:1780–84. New York: Macmillan Ref. USA

Bonner JT, May RM. 1981. Introduction. *The Descent of Man, and Selection in Relation to Sex*, by Charles Darwin, pp. vii–xli. Princeton, NJ: Princeton Univ. Press

Cohen IB. 1994. *Interactions: Some Contacts Between the Natural Sciences and the Social Sciences*. Cambridge, MA: MIT Press

Duncan OD. 1984. *Notes on Social Measurement: Historical and Critical*. New York: Russell Sage Found.

Flyvbjerg B. 2001. *Making Social Science Matter: Why Social Inquiry Fails and How It Can*

Succeed Again. Cambridge, UK: Cambridge Univ. Press
Gould SJ. 1977. *Ever Since Darwin: Reflections in Natural History*. New York: Norton
Gould SJ. 1982. In praise of Charles Darwin. In *Darwin's Legacy*, ed. CL Hamrum, pp.1–10. San Francisco: Harper & Row
Gould SJ. 2000. Linnaeus's luck? *Nat. Hist.* 109:18–25, 66–76
Gruber HE. 1981. *Darwin on Man: A Psychological Study of Scientific Creativity*. Chicago: Univ. Chicago Press.
Hodge MJS. 1987. Natural selection as a causal, empirical, and probabilistic theory. In *The Probabilistic Revolution*, Vol. 2: *Ideas in the Sciences*, ed. L Kruger, G Gigerenzer, MS Morgan, pp. 233–70. Cambridge, MA: MIT Press
Jones S. 2000. *Darwin's Ghost: The Origin of Species*. New York: Random House. Rev. ed.
Keen MF. 1999. *Stalking the Sociological Imagination: J. Edgar Hoover's FBI Surveillance of American Sociology*. Westport, CT: Greenwood
Lasker GW, Tyzzer RN. 1982. *Physical Anthropology*. New York: Holt, Rinehart & Winston
Lieberson S. 1985. *Making it Count: The Improvement of Social Research and Theory*. Berkeley/Los Angeles: Univ. Calif. Press
Lieberson S. 1997a. Modeling social processes: some lessons from sports. *Sociol. For.* 12:11–35
Lieberson S. 1997b. The big issues in society and social history: a probabilistic perspective. In *Causality in Crisis? Statistical Methods and the Search for Causal Knowledge in the Social Sciences*, ed. VR McKim, SP Turner, pp. 359–85. Notre Dame, IN: Univ. Notre Dame Press
Lieberson S, Silverman A. 1965. The precipitants and underlying conditions of race riots. *Am. Sociol. Rev.* 30:887–98
Link BG, Phelan J. 1995. Social conditions as fundamental causes of disease. *J. Health Soc. Behav.* (Suppl.):80–94
Mayr E. 1961. Cause and effect in biology. *Science* 134:1501–6
Mayr E. 1967. Introduction. *On the Origin of Species.* By C. Darwin, pp. vii–xxvii. New York: Antheneum
Rain M, Winship C. 1999. The dangers of "strong" causal reasoning in social policy. *Society* 36(5):38–46
Richter MN. 2000. Evolution: biological, social cultural. In *Encyclopedia of Sociology*, ed. E. Borgatta, R. Montgomery, 2:875–80. New York: Macmillan Ref. USA
Scriven M. 1959. Explanation and prediction in evolutionary theory. *Science* 130:477–82
Schopf JW. 2000. Solution to Darwin's dilemma: discovery of the missing Precambrian record of life. In *Variation and Evolution in Plants and Microorganisms: Toward a New Synthesis 50 Years After Stebbins*, ed. FJ Ayala, WM Finch, MT Clegg, pp. 6–20. Washington, DC: Natl. Acad. Press
Simpson GG. 1950. Evolutionary determinism and the fossil record. *Sci. Monthly* October 1950: 262–67
von Neumann J, Morgenstern O. 1944. *Theory of Games and Economic Behavior*. Princeton, NJ: Princeton Univ. Press

Annual Review of Sociology
Volume 28, 2002

CONTENTS

Errata

An online log of corrections to *Annual Review of Sociology* chapters (if any, 1997 to the present) may be found at http://soc.annualreviews.org/