



THE POVERTY OF DEDUCTIVISM: A CONSTRUCTIVE REALIST MODEL OF SOCIOLOGICAL EXPLANATION

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*Despite the lip service which many sociologists pay to Popper's hypothetico-deductive model (HDM) of theory testing, few if any major social theories have been definitively falsified. The reason is that sociological explanations do not fit the deductivist model of explanation: They do not contain universal or statistical "covering laws" from which falsifiable hypotheses could be deduced. Sociological explanations are better conceived in realist terms as causal models of the social processes that produce certain outcomes. While few models are completely false, some are nonetheless more empirically adequate than others. This essay argues that (1) the CLM is inadequate to sociology and that (2) attempts to reformulate the HDM are therefore destined to fail. It then outlines (3) a constructive realist model (CRM) of sociological explanation and uses it to develop (4) explanatory realist (ER) criteria for evaluating explanations. Deductivist and realist approaches to methodology are then compared through an examination of Skocpol's *States and Social Revolutions*.*

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Social science is nothing but history: this is the thesis.
K. Popper, *The Poverty of Historicism*

At least since Comte, sociologists have sought after a method, a set of rules and procedures that could vouchsafe the scientific character of their enterprise. In postWar American sociology, the most influential interpretation of scientific method has undoubtedly been the hypothetico-deductive model (HDM), based loosely on the philosophy of Karl Popper (1963, 1965).¹ In this essay, I argue that the HDM rests on an inadequate model of social-scientific explanation and develop an alternative model, which I call *constructive realism*.

The attraction of the HDM for social scientists is obvious: It provides a simple, unambiguous procedure for evaluating theories—“falsificationism.” But while sociologists pay a great deal of lip service to falsificationism, they would be hard put to identify a single, major theory that has been definitively falsified. Marx, Weber, and Durkheim are alive and well and by all indications remain as influential as ever. Rumors of Parsons’ death are also premature; functionalism has simply gone into European exile, where it lives under a new identity: “systems theory.”

The problem with the HDM is the covering-law model (CLM) of explanation that underlies it. According to the CLM, which is most clearly developed in the work of Carl Hempel, any valid scientific explanation can be stated as a deductive argument in which a universal or statistical law forms the major premise. This model is inadequate for one simple reason: sociology has not yet generated any laws, at least not universal or statistical laws that could serve as premises in deductive arguments.²

¹Virtually every major current within contemporary sociology was influenced by deductivism—from structural-functionalism, with its emphasis on “general laws,” to multivariate, statistical analysis, with its insistence on “falsifiability.” The only real dissenters were those who, following Dilthey, disputed the very possibility of a social *science* and insisted on the primacy of interpretation or *Verstehen* (Dilthey 1974; Weber 1985; Winch 1953; Habermas 1973, 1981, 1985).

²This is not to deny that sociology has produced many broad, empirical generalizations. But generalizations are not the same as laws. Advocates of rational choice theory sometimes claim to be in possession of laws. I consider this claim below.

Recognizing this problem, several social scientists have advanced reformulated versions of the HDM. Following Imre Lakatos (1970, 1978), Michael Burawoy (1990) argues that science progresses not through the falsification of theories but the falsification of falsifications of theories. Stanley Lieberman (1985), by contrast, argues that social theories can specify only the probability of certain outcomes. Hence, they can be falsified only probabilistically, by distributions of outcomes. The problem with these proposals is that they require some rather strong—and I believe unsustainable—assumptions about sociological theory and social causality.

I therefore advance a constructive realist model (CRM) of explanation. The CRM construes explanations as linguistic representations of causal processes—i.e., as *causal models* constructed on the basis of certain theoretical assumptions about the structure of the social world. Any explanatory model, moreover, makes empirical claims—evidentiary, causal, and ontological—which can be evaluated, and even falsified, relative to the evidence and in comparison with other explanations. Generally speaking, constructive realism implies that we should prefer those explanations that are most adequate empirically and most strongly confirmed.

The remainder of this paper is organized into five sections. Section 1 reviews the standard, deductivist model of science. Section 2 examines Burawoy's and Lieberman's efforts to reformulate deductivism in "holistic" and "probabilistic" terms. Section 3 outlines the CRM of explanation. Section 4 uses the CRM to develop explanatory realist criteria of evaluation. Section 5 compares deductivism and realism through an analysis of Skocpol (1979) and the "methodological critiques" of Burawoy (1989b) and Lieberman (1992). In the conclusion, I consider the use of deductivism as a heuristic and rhetorical device.

1. DEDUCTIVISM: THE STANDARD MODEL

In his *Essay Concerning Human Understanding* ([1748] 1988), Hume posed a simple question that has kept philosophers busy for well over two centuries: How can we have knowledge of the future based on our experiences of the past? *Logically* speaking, he argued, we cannot. Simply because the sun has risen every day up until now does not *entail*

that it will do so tomorrow. Hume's question has come to be known within philosophy as the "problem of induction" (Russell [1912] 1959).

In his *Logic of Scientific Discovery*, Karl Popper ([1934] 1965) proposed an equally simple solution to this problem. Although we cannot know with certainty whether some statement about the world is true, we can determine if it is *false* by logically deriving its empirical consequences and subjecting them to experimental test. If some theory "H" logically entails some empirical consequence "O," and it is reliably demonstrated that O is false, then we can infer by the principle of *modus tollens* that H is also false. If O is true, then this provides corroborating evidence (though not proof) that H is true.

The clearest and perhaps most influential attempt to adapt Popperian falsificationism to social science is to be found in Arthur Stinchcombe's (1968, chap. 2) *Constructing Social Theories*. Social scientific "inference," argues Stinchcombe, proceeds in the following way. We begin with one or more "theoretical statements" about how one "class of phenomena" is connected to another, and derive, "by logical deduction and by operational definitions of the concepts, an empirical statement" (1968, pp. 15–16). Then, we design an appropriate test and, based on the results, reject or retain the theory.

This same principle can be used to "adjudicate" between "alternate theories." To each theory H, there corresponds some unique set of empirical statements O₁, O₂, O₃ . . . etc. For any two nonidentical theories, H₁ and H₂, there will necessarily be some O entailed by H₁ but not H₂. Having identified one such "prediction," we can construct a "crucial experiment" that will allow us to infer that (at least) one of the theories is false (Stinchcombe 1968, pp. 20–22).

The standard version of the HDM set forth by Stinchcombe implicitly assumes a deductive or "covering law" model of explanation.³

³The covering-law model of explanation, like the hypothetico-deductive model of theory testing, is rooted in a scepticist epistemology, and, more particularly, in a Humean analysis of causality. Suppositions about causality, argued Hume, derive from the observation of empirical regularities. We infer causality whenever we observe that one event is related to another by "priority" in time, "contiguity" in space and, most importantly, "constant conjunction" in appearance. There is no necessary connection between cause and effect, concludes Hume: "necessity is something, that exists in the mind, not in objects" (Hume [1740] 1988, sec. 7). Following Hume, the logical empiricists argued that scientific explanations involve claims about *logical* necessity, not physical necessity. Explanations, in their views, are deductive arguments covered by universal laws.

The clearest and by far the most influential formulation of the CLM is to be found in Carl Hempel's renowned essay, "Aspects of Scientific Explanation" (Hempel 1965, chap. 12). In Hempel's version of the CLM, explanations consist of an "explanans statement" containing one or more "general laws" (major premise) and a statement of "initial conditions" (minor premise), which together logically entail the "explanandum sentence" (conclusion) (p. 336).⁴ For Hempel, then, explaining something means showing that the outcome was "to be expected under the circumstances," by subsuming it beneath one or several laws. Testable predictions can be derived simply by changing the statement of initial conditions.⁵

Few scientific explanations are actually stated in anything like this form, as Hempel is well aware (p. 412). There are, in fact, a great many scientific explanations that contain no reference to laws whatsoever.⁶ But Hempel does not argue that all scientific explanations are presented

⁴Hempel distinguishes two different types of scientific explanation. In "deductive-nomological" (D-N) explanations, the covering laws are "universal" in form (e.g., Newton's laws of motion, Boyle's gas laws) and entail singular, "deterministic" outcomes (pp. 336–47). "Deductive-statistical" (D-S) explanations contain statistical laws and specify "probabilistic" *distributions* of outcomes (such as result in games of chance) (pp. 380–81).

⁵D-N explanations, it should be emphasized, are *not* causal explanations, though they *may* be restated in causal terms. But, says Hempel, since

a statement of individual causation leaves the relevant antecedent conditions, and thus also the requisite explanatory laws, indefinite, it is like a note saying that there is a treasure hidden somewhere. Its significance and utility will increase as the location of the treasure is more narrowly circumscribed, as the relevant conditions and the corresponding covering laws are made increasingly explicit. (P. 349)

Causal explanations, in other words, are just signposts along the road toward general laws.

⁶Some are "elliptical": they simply leave certain relevant laws or facts unstated. Others are "partial": they do not fully account for the explanandum (p. 415). Both elliptical and partial explanations may often be regarded as "explanation sketches,"

presenting the general outlines of what might well be developed, by gradual elaboration and supplementation, into a more closely reasoned explanatory argument, based on hypotheses which are stated more fully and which permit of a critical appraisal by reference to empirical evidence. (P. 424)

Historical sciences, such as geology, evolutionary biology, and sociology, provide numerous examples of explanations that probably cannot be fleshed out into deductive explanations in this way. For a detailed discussion of this problem, see Miller 1987:21–24.

as deductive arguments, merely that most good explanations *could* potentially be stated in this form.

Within “hard” sciences, such as physics, many explanations undoubtedly *can* be stated in deductive form. But Hempel believes that the CLM can also be fruitfully applied to “soft” sciences, such as history and sociology. Explanations in these fields, he argues, frequently appeal to “general laws” of the following form:

In every case where an event of a specified kind C occurs at a certain place and time, an event of a specified kind E will occur at a place and time which is related in a specified manner to the place and time of the occurrence of the first event (p. 232; chaps. 9, 11).

Indeed, Hempel suggests that good explanations in the social and historical sciences always refer to general laws, at least tacitly. This in fact is what distinguishes them from “pseudo-explanations.” Obviously, this definition of explanation is quite restrictive. To begin with, it excludes narrative histories that do not generalize beyond a single case. By the same token, it eliminates “multivariate” explanations in the social sciences that do not specify deterministic outcomes. In effect, Hempel’s requirement excludes many explanations that historians and social scientists would recognize as good explanations.

Hempel did not claim that an explanation must be deductive to be good, however. He also identified an “inductive-statistical” form of explanation, in which an observed statistical association between two events takes the place of a universal law. This allows us to infer (but not deduce) that the first event, when it occurs, is almost certain to be followed by the second. The relationship between the explanans and the explanandum is one of “inductive support,” not deductive entailment. So long as the level of association is very high (Hempel insists it be close to 1), however, such explanations will be readily falsifiable. Upon closer examination, this definition of explanation is almost as restrictive as the previous one.⁷

⁷Consider this example from epidemiology. Paresis is a nervous condition that occurs in cases of untreated syphilis at a rate of around 30 percent. It is not known to be associated with any other risk factors. Syphilis, in short, is a necessary but not sufficient cause of paresis. But by Hempel’s standards, this explanation is not a good one.

A level of association at or near the “1” level, needless to say, is a requirement that few explanations in social science (except the most patently spurious ones), are likely to meet. Clearly, Hempel’s dicta are inappropriate to the complex causal relations that prevail in the social world.⁸

Contemporary defenders of the standard model have dispensed with laws altogether, by conceptualizing causality in “probabilistic” (rather than deterministic or modal) terms. For them,

one event is the cause of another if the appearance of the first event is followed with a high probability by the appearance of the second, and there is no third event which we can use to factor out the probability relationship between the first and second events (Suppes 1970: 10; cf. also Good 1961–1962; Salmon 1971; Suppes 1984).

Causes, in other words, raise the probabilities of their effects. A cause is “spurious” if “an earlier event may be found which accounts for the conditional probability of the effect just as well” (Suppes 1970: 21). Where more than one such prior event is identified, the one closer in time to the explanandum event is to be preferred (p. 28).⁹

The probabilistic model of explanation certainly fits actual practices of explanation in social science much better than do Hempel’s D-N and I-S models. But it undermines the logical basis for falsificationism. Probabilism, in essence, replaces falsification procedures with statistical inference rules. But as any social scientist well knows, these rules, if applied mechanically, often lead to erroneous conclusions, for they provide no criteria for distinguishing correlation

⁸In essence, an I-S explanation must be complete: It must identify virtually all factors influencing a given outcome. This is a standard that has never been achieved in practice by sociology and probably cannot be achieved in principle by any discipline dealing with complex, open systems where individual outcomes are shaped by heterogeneous and disparate forces.

⁹Suppes introduces this qualification in order to address the problem of distinguishing catalysts and causes (or, as he puts it, “indirect” and “direct” causes). But, as we will see shortly, it creates another set of problems.

from causation.¹⁰ Indeed, if applied without suppositions about the relevant causal mechanisms, probabilistic criteria would routinely lead us to reject good causal theories and prefer bad ones.¹¹

In sum, the hypothetico-deductive model works only for explanations containing a covering law of universal or statistical form. Social-scientific explanations are not—and probably cannot be—stated in this form. If the covering-law requirement is relaxed to fit social scientific explanation better, as in the probabilistic model, falsification becomes impossible and explanations must be evaluated according to nonempirical criteria, such as statistical inference rules. Thus the CLM does not seem to be a promising model of explanation in sociology—or more generally, in the social and human sciences.

I now examine two attempts to salvage the HDM for sociology without abandoning the CLM.

2. DEDUCTIVISM: HOLISTIC AND PROBABILISTIC REFORMULATIONS

The HDM asserts that a single, empirical counter-instance provides sufficient grounds for rejecting a theory. Clearly, this stricture is rarely (if ever) followed in sociology (or any science). And indeed, it would be rational to do so only if (1) evidence has unequivocal theoretical implications and (2) theories have deterministic empirical implications. These assumptions are challenged, respectively, by Michael Burawoy and Stanley Lieberson.

¹⁰Consider a simple example from meteorology (adapted from Salmon 1984: 43–44). The appearance of dark clouds is often followed by rain. Rain, however, is always preceded by a sudden drop in atmospheric pressure. Drops in atmospheric pressure, furthermore, are always followed by falling barometer readings. Applying Suppes procedures, we would be forced to conclude that rainstorms were not caused by rain clouds or even by drops in atmospheric pressure, but by falling barometer readings! Anyone with even a rudimentary understanding of the physical mechanisms that cause rain would not fall prey to this fallacy.

¹¹I do not mean to suggest that we can never infer causality from statistical data. On the contrary, when combined with theoretically grounded hypotheses about “mechanisms,” statistical analysis can be a very powerful tool for assessing the presence and relative strength of various causal influences.

2.1. *The Holistic Version*

The standard version of the HDM posits a fundamental disjuncture between theory and observation.¹² Adopting the position known as “holism,” Burawoy argues that, in fact, theory and observation interpenetrate one another (Duhem [1906] 1954; Quine 1954; Harding 1976). Theories, he contends, are like “lenses,” which shape our very perceptions (Burawoy 1989a: 95). Facts, accordingly, “are already interpretations” (Burawoy 1989b: 773). Every theory, in other words, will interpret the facts differently (Burawoy 1990: 776). If this is true, then there is no neutral, empirical basis for evaluating theories, much less “adjudicating” between them. Indeed, “crucial experiments” of the sort Stinchcombe proposes would be little more than rhetorical displays in which proponents of one theory used their interpretation of the facts to attack their opponents’ theory. Because they cannot definitively falsify competing theories, contends Burawoy, researchers attempt to falsify falsifications of their own theory. This process gives rise to “scientific research programs,” organized around a “core,” a set of fundamental axioms about the world that are not open to question (e.g., see Burawoy 1990: 780). The core is surrounded by a “protective belt,” a set of “auxiliary hypotheses” that account for “anomalies” at odds with the fundamental axia of the core. As researchers “refute refutations,” the protective belt becomes larger and the core better shielded from criticism.

But how can we know if these “refutations” are real or merely *ad hoc*? To address this problem, Burawoy adapts the distinction made by Lakatos (1970, 1978) between “progressive” and “degenerating” research programs:

In a *progressive* program the new belts of theory expand the empirical content of the program, not only by absorbing anomalies but by making predictions, some of which are corroborated. In a *degenerating* program successive belts are only backward looking, patching up anomalies in *ad hoc* fashion,

¹²Indeed, logical empiricists distinguished between a “theory language” in which laws were stated and the “normal language” in which observations were recorded. Few philosophers of science now regard this distinction as tenable.

by reducing the scope of the theory, or by simply barring counter examples. (Burawoy 1990: 778)

In other words, refutations are “progressive” only if they increase the potential empirical content of a theory without diminishing its actual empirical content.

Since every research program constitutes a seamless whole of theory and observation, there are no purely empirical grounds for preferring one over another: “extra-empirical considerations necessarily enter in” (Burawoy, 1989a: 86). Further, because “theories correspond to constellations of interest,” choosing to work on a particular research program involves certain political commitments (p. 88). Indeed, these commitments enter into the research process itself, making “the production of knowledge . . . an inescapably political process” (p. 88). A research program, in short, is not just a way of viewing the world; it is a worldview.

Burawoy’s “methodology of scientific research programs” would seem to offer an attractive alternative to pure, Popperian falsificationism. Unfortunately, his reconstructed version of the HDM rests on a holistic model of theory which—like the CLM—is deeply inadequate. To be sure, there exist within sociology a number of relatively distinct and well-elaborated theoretical traditions (e.g., Marxism, Weberianism, Durkheimianism). But it would be hard to reduce any of them to a set of axioms, much less to achieve a consensus about which ones were fundamental.¹³ Similarly, there are individual researchers who commit themselves to the defense of a particular theoretical tradition. But there are many—and probably more—who borrow freely from several. Finally, while our theoretical perspectives surely influence our political views, there is little indication that theoretical traditions constitute coherent world views. Even the “research program” that Burawoy regards as the most developed—namely, Marxism—looks more like a patchwork quilt than a seamless whole, at least to this observer. In short, contemporary sociology is not currently organized into “research programs,” and,

¹³Burawoy tries to deal with this problem by introducing the notion of “competing” cores within a single research program. But this merely displaces the problem, for it is not clear that the various interpretive schools within a theoretical tradition could be any more readily reduced to some set of fundamental axioms.

Burawoy's methodological exhortations notwithstanding, there is little indication that it will be any time in the future.

And even if it were organized into coherent research programs, it is not clear that theoretical holism would provide adequate standards of scientific progress. Consider the example of neoclassical economics. Unlike its sociological counterparts, neoclassical theory does have many of the hallmarks of a research program: it has a number of core axioms (e.g., methodological individualism and utility maximization) shared by virtually all researchers in the field; these axioms are relentlessly defended against all falsifications (e.g., by showing that apparently non-self-interested behavior can be understood in more self-interested terms) and are aggressively expanded to new areas of social life (e.g., electoral and political behavior); in addition, they are commonly (if not invariably) associated with certain political commitments (e.g., to free trade and small government). In sum, neoclassical economics probably comes closer to the research program model than any other theoretical tradition within the contemporary social sciences. How would Lakatos evaluate it? Is it "progressing" or "degenerating"? Pointing to the growth of "rational choice theory" within the social sciences, defenders of the neoclassical approach would undoubtedly argue that it is progressing. And indeed many "new belts of theory" have been added over the past 50 years; in fact, at this point in time, there are few areas of social life that have not been subjected to rational choice analysis. But have these new belts of theory also expanded the "empirical content" of neoclassical theory or generated "new predictions some of which have been corroborated"? Critics of rational choice theory would probably answer in the negative. For example, in their much discussed book on the *Pathologies of Rational Choice Theory*, Donald Green and Ian Shapiro (1994) argue that there is a "curious disjunction" between theory and application in rational choice work within political science:

On the one hand, great strides have been made in the theoretical elaboration of rational actor models. . . .
On the other hand, successful empirical applications of rational choice models have been few and far in between. (p. ix)

What is more, they continue, few of these applications have survived rigorous testing (p. 9). From this perspective, the last half-century of neoclassical theory looks like a litany of *ad hoc* efforts to patch over persistent anomalies (e.g., efforts to explain altruistic behavior by expanding utility functions to include altruistic preferences). Thus, what might look like progress to neoclassicists, looks like degeneration to their critics. Who is right? The problem is that it would actually be quite difficult to tell. For how would we know whether the “empirical content” of a theory was increasing or decreasing over time? And when is a theoretical adjustment “*ad hoc*” and when does it constitute a “reasonable” extension of the core axioms? Even if defenders and critics of a particular program were able to agree on what these terms meant and how they could be operationalized—which seems highly unlikely—it would still be extraordinarily difficult to develop an overall assessment of any given program. In my view, then, the holistic approach is inappropriate for contemporary sociology and would be difficult to apply to any social science.

I now turn to the second attempt to reformulate the HDM.

2.2. *The Probabilistic Version*

Newtonian mechanics posited a fully deterministic picture of the physical world.¹⁴ This picture was shattered by quantum mechanics, which reduced the relationship between cause and effect to one of statistical probability (*The Probabilistic Revolution*, 1987). Lieberman urges that we recast sociological theory in probabilistic terms as well, for in his view “it is unrealistic to act as if social life is driven by deterministic forces” (1991: 7). The most a theory can do is to specify “certain conditions” that “alter the *likelihood* of different outcomes” (1991: 8). Theories will then consist of statements such as: “If X_i is positive, then here is the probability of Y ; and if X_i is negative, then the probability of Y is different” (Lieberman 1991: 10).

¹⁴The classic statement on physical determinism is to be found in Laplace’s claim. If there were a demon who possessed knowledge of all physical laws and infinite powers of mathematical calculation, and he were told the location of all particles in the universe at a given moment in time, then, argued Laplace, he could foretell the future and reconstruct the past with absolute precision.

If theories only specify the *probability* of a given outcome when certain causal factors are present (or absent), then a single “anomaly” or counterinstance will never constitute sufficient grounds for rejecting a theory. Instead, evidence would support or not support a theory depending on whether “the frequency of expected outcomes is different from what occurs under other conditions” (Lieberson 1991: 9). To be falsifiable, a theory would have to provide a precise, numerical statement specifying the probability of a given outcome under some set of conditions (Lieberson 1991: 10).¹⁵ In principle, all that would then be required to test a theory or theories is “the frequency distribution for a set of observations.” By comparing the expected and actual frequencies, it would be possible to evaluate and even adjudicate between theories (Lieberson 1991: 9). Lieberson’s methodology of probabilistic falsificationism would seem to offer a reasonable alternative to the strict falsificationism of the HDM. But it requires a major assumption—namely, that the causal relations that sociologists study are inherently and irreducibly probabilistic.¹⁶ Now, quantum mechanics has in fact identified physical processes such as radioactive decay that seem to behave probabilistically. The probability that one atom of a radioactive isotope will decay during a certain interval

¹⁵But how are these probability values to be derived and tested? The ideal method would be experimental. But true experiments are rarely possible in the social sciences, and Lieberson is highly skeptical about sociologists’ attempts to simulate experimental procedure through statistical techniques or comparative study designs (Lieberson 1985:chaps. 2, 3). Perhaps this is why he tacitly retreats from the idea of probabilistic falsificationism and proposes that social science establish criteria of “adequate evidence” so as to assess “degrees of confidence in a theory” (Lieberson 1991:13). Unfortunately, Lieberson does not flesh out this proposal in any great detail, beyond the recommendation that sociologists use “multiple data sets,” “situational” rather than statistical controls, and inductive techniques of data analysis (Lieberson 1991:11–12).

¹⁶Lieberson sometimes uses probabilism in a second and weaker sense as implying that theories yield only partial explanations (Lieberson 1991:7–8). This would mean that there may exist several different (partial) explanations that are equally true. In this case, following the falsification procedures laid down in the standard version of the HDM would lead us to reject true theories! Consequently, the only reasonable way to evaluate theories is in terms of the supporting evidence. Our “confidence” in a particular theory would then be a function of the volume and variety of corroborating evidence. Stated in these terms, I find Lieberson’s proposal unobjectionable, though I suspect it does little more than render explicit the principles of commonsense reasoning. For a lucid discussion of these two concepts of probabilism and their applicability to social science, see Manis and Meltzer (1994). On probabilism and causality in general, see Salmon (1980), Fetzer (1988), and Humphreys (1989).

can be calculated with great accuracy. But there is no known explanation for why one atom decays and another does not—and there are mathematical reasons for believing that none exists. It may really be a matter of chance. At first blush, some social processes may also appear to operate in a similar manner. It is well known, for example, that the occupational attainment of fathers and sons are strongly correlated. Treating the correlation coefficient as a probability value, we could calculate the likelihood, say, that a son will attain a level of occupational prestige equal to or greater than his father's. But here the parallel with quantum mechanics ends, for given a sample of men, one could adduce a great many additional factors besides fathers' occupation which affect occupational attainment (e.g., education and race). Moreover, examining each man individually, we could adduce a further set of biological and biographical factors (e.g., intelligence and upbringing) that might shed even more light on the outcome. Indeed, given sufficient information, we might even be able to construct a fairly complete, narrative explanation for that man's career trajectory. Thus, the analogy between quantum mechanics and stratification theory, while illuminating, turns out to be rather superficial. Neither theory can predict individual outcomes. But in the case of occupational mobility, this has to do with the operation of subsocial processes that are not specified by the model, rather than the inherent and irreducible nature of the processes, themselves. The "probabilistic" nature of social explanations stems from the incompleteness of our theories, not the nature of cause and effect.

In sum, Burawoy's and Lieberman's efforts to overhaul deductivism and salvage logical falsificationism, while illuminating, are ultimately unsuccessful. Burawoy is right that the standard version of the HDM rests on an underdeveloped notion of theory, but his own holistic view of theory is highly *overdeveloped*. Similarly, Lieberman is right in rejecting the deterministic picture of causality implicit in the HDM as inadequate to social science, but his own probabilistic model is hardly adequate either. Indeed, Burawoy and Lieberman do not really challenge the deductivist premises that underlie the HDM. Since these assumptions are so rarely made explicit, it may be useful to summarize them here. Briefly, deductivism assumes the following:

1. A *regularity view of causality* as a constant (or at least "probable") conjunction between two *events*.

2. A *logical model of explanations* as arguments in which the explanans deductively entails (or at least inductively supports) the explanandum.
3. A *propositional view of theories* as laws (or “axia”).

I suspect that few sociologists, including those who advocate logical falsificationism, would be seriously willing to defend all (or perhaps any) of these assumptions. Clearly, the regular association of two events does not by itself provide grounds for inferring causality. Most sociologists would agree that a valid causal explanation must also identify theoretically specified “causal mechanisms” that connect or link the two events. And while some might accept the view that good explanations can (or even should) be stated in propositional form, few would argue that sociological theory consists of lawlike propositions. Sociologists, in short, appear to be of two minds regarding the deductivist model of science. While they are obviously attracted to falsificationism, they do not accept the deductivist assumptions it requires.

In the second half of this essay, I will spell out a realist model of social science that is more in line with the explanatory practices and intuitive understandings of social scientists and use it to develop empirical criteria for evaluating explanations.

3. EXPLANATION WITHOUT LAWS: A CONSTRUCTIVE REALIST MODEL OF SOCIAL SCIENCE

Realism is the view that science seeks to reveal the underlying structures of the world.¹⁷ The version of realism that I wish to advocate here is constructive, in that it construes explanations as *linguistic representations*, causal models constructed out of theoretical terms (Hacking 1983; Cartwright 1983; Van Fraassen 1980; Salmon 1984;

¹⁷Largely in reaction to the rise of relativist critiques of science, realism has enjoyed a considerable resurgence in recent years. As one prominent philosopher has put it, “realism is the only philosophy that doesn’t make the success of science a miracle” (Putnam 1975). If the relativists are right in arguing that the world “and such evidence as we have about that world do little or nothing to constrain our beliefs” (Laudan 1992: viii), then why are current scientific theories manifestly more successful than their predecessors? The most obvious—and plausible—answer is that we know more about the underlying properties and structures of the world than we used to.

Miller 1987; Suppe 1989).¹⁸ Constructive realism differs sharply from deductivism in two respects: it sees explanation as *causal* rather than logical and it draws a sharp distinction between explanation and theory. Indeed, realism breaks radically with the three major assumptions of deductivism enumerated above.

In the regularity theory of causality, events are the fundamental units of analysis. In the realist view, by contrast, *processes* are taken as basic.¹⁹ Unlike events, which are “relatively localized in space and in time . . . processes have much greater temporal duration, and in many cases, much greater spatial extent” (Salmon 1984: 139).²⁰ A causal process can, at least in principle, always be described as a series of events. But a series of events does not necessarily constitute a causal process. A causal process is distinguished from a mere series of events by its *ability to transmit influence* (Salmon 1984: 141).²¹ There are two basic modes of causal influence: (1) *propagation*, in which certain structures are transmitted across time and space through a neutral medium (as when a stone is cast into a pool of water), and (2) *production*, in which two or more causal processes interact bringing a new and durable structure into being (as when two waves in a pool flow into one another). Building on this distinction,

¹⁸This differs from what might be called *reflective* realism, the position that theories are linguistic *mirrors* of reality. Hilary Putnam, for example, argues that scientists “mirror the world—i.e., their environment—in the sense of *constructing a symbolic representation of that environment*” (Putnam 1978: 123, emphasis in the original; cf also Putnam 1981). The problem with theoretical realism, apart from its holistic overtones (cf. Boyd 1973; Newton-Smith 1978), is that it necessarily implies the *empirical* claim that there is a single, correct theory of the natural world toward which all theories will necessarily converge. Unfortunately, there is little evidence in the history of science to support this claim (Laudan 1981).

¹⁹The theory of causality advanced here is based on the work of Wesley C. Salmon (1977, 1980, 1982, 1984, 1988).

²⁰Most sociology concerns processes: gender socialization, class-formation, etc. Even those phenomena that sociologists sometimes code as events (e.g., crime, revolution) can be analyzed as processes, too.

²¹Consider Hume’s famous example. A billiard ball moving across a table is a causal process, which could also be described as a series of events (i.e., the ball occupying a succession of positions in space-time). Let us assume that the ball also casts a shadow. The movement of the shadow on the surface of the table could also be described as a series of events, but it hardly constitutes a causal process for if shadows of two balls crossed over one another, there would be no transmission of influence (unless the balls also struck each other).

we can identify two basic patterns of causal influence: (1) *wedges* (or *nets*), which occur when two or more causal processes interact to produce a single outcome (as when two chemicals are mixed to make a new compound), and (2) *forks* (or *fans*), which occur when one causal process generates two or more similar outcomes (as when a single bacteria makes several people ill).²²

In the CLM, explanation is a logical relation between two statements, explanans and explanandum. In the CRM, it is a semantic relation between causal models and causal processes (or “systems”). A causal model is a simplified, linguistic representation of one or more real causal processes, which contribute to some set or type of outcomes. *To explain something, then, is to represent, and thereby render more readily comprehensible, the principal processes which produced it.* Models may take two basic forms, which correspond to the two modes and patterns of causal influence described above. *Monocausal* models are used to represent processes of propagation and have a fanlike structure. The spread of an infectious disease, for example, may be understood in terms of a contagion model in which a single carrier transmits a bacteria or virus to other individuals and so on. Processes of social diffusion can be modeled in analogous ways. *Multicausal* models are used to represent processes of production and have a netlike structure. The contraction of an infectious disease may be comprehended in terms of a balance of forces model in which exposure to the bacteria or virus interacts with the immune response (itself a function of various factors—for example, previous exposure to disease, physical constitution, diet, stress) to produce (or not produce) infection. Processes of political contestation can be thought of in similar terms.

What sort of model is appropriate depends not on the nature of the causes involved but on whether we are interested in explaining a certain class of outcomes in particular or a certain type of outcome in general. Thus it is useful to distinguish between two basic strategies of explanation: (1) *conjunctive* explanations, which seek to account for a large number of (“small”) effects in terms of a (“big”) common cause,

²²Wedges and forks may also be understood as two different moments in the transmission of causal influence. Thus the formation of a new compound produces numerous identical changes in the molecules of the constituent chemicals. Similarly, the transmission of a bacteria may have numerous contributing causes. The distinction between wedges and forks is temporal, not analytical.

and (2) *interactive* explanations, which aim to account for a (“big”) effect in terms of a small number of (“little”) contributing causes. These two strategies of explanation are complementary, not contradictory (Ragin 1987).

In the CLM, a theory is just the set of all general statements or laws that scientists adduce in their explanatory arguments. In the CRM, a theory is a *symbolic construct*, stated in ordinary or mathematical language, which defines certain classes of objects and specifies their key properties. The objects are assumed to refer to *real entities in the world* and the properties to *actual qualities* of these entities. A theory, in other words, is a set of *ontological assumptions* that are used, explicitly or implicitly, in the *construction* of a causal model or models. (Note: In the terms used here, then, “theory” is different from, and logically prior to, “explanation.”) The object classes may be either natural or constructed. Natural classes are appropriated from the *natural language* of everyday speech. Constructed classes are formulated in the specialized language of scientific theory.²³ The properties specified by a theory may be either essential or relational. Essential properties are qualities that are taken to *inhere* in the objects themselves (e.g., “IQ”). Relational properties are qualities that objects possess by virtue of their relationship to other objects or classes of objects—i.e., by virtue of their *position in larger systems* (e.g., “social class”). Theories may be stated explicitly as axioms, or they may remain implicit within narratives. In narrative theories, the ontological assumptions remain embedded within a story or plotline, a “grand narrative.” In axiomatic theories, the assumptions are rendered explicit in a lexicon of carefully defined terms, a “grand theory.” In addition, theories may be either homogeneous or heterogeneous in structure. Homogeneous theories are ontologically parsimonious and are most often axiomatic in form. Heterogeneous theories are ontologically promiscuous and are usually narrative in form.

One of the signal failings of deductivism is that it provides no clear account of how explanations are constructed.²⁴ The CRM suggests

²³Theoretical objects in general, and social-scientific ones in particular, frequently undergo a process of “naturalization,” becoming a part of everyday, popular speech (e.g., system, structure).

²⁴Popper mystifies the process, locating it in the hidden mechanisms of genius. On the other hand, Lakatos trivializes it, portraying the construction of explanations as a formulaic process.

that explanations are constructed through a *work of interpretation*, in which theoretical terms are used to construct causal models of social processes. In formulating their explanations, researchers have a number of theoretical traditions at their disposal. They may choose to situate themselves within one—or, more commonly, at least within sociology—they may draw on several. By the same token, researchers often alter or reformulate existing theories in their efforts to construct coherent models. Fitting theoretical language to observed reality, tinkering with a model, is an ongoing process. It is neither an inborn gift nor a set of formal procedures but a craft that is mastered through apprenticeship and experience (Bloch 1974). This artisanal view of the sociological enterprise, I believe, is much more in line with the realities of research practice than the logical idealizations advanced by deductivists.

I will now show how constructive realism can be used to elaborate some basic criteria for evaluating explanations.

4. EVALUATION WITHOUT PREDICTION: EXPLANATORY REALISM

From a realist perspective, the only standard by which explanations can be evaluated is *empirical adequacy*. The interpretation of empirical adequacy that I wish to elaborate and defend here might be called *explanatory realism*.²⁵ Any explanation, it is argued, involves empirical claims at no less than three different levels: (1) there exist certain entities with certain properties that constitute (2) certain causal processes that produce certain effects that (3) have been or can be observed either directly or indirectly. Sometimes these observational, causal, and ontological claims can be definitively falsified. More

²⁵It should be distinguished from another version of realist evaluation, which focuses on the empirical adequacy of *theories*. Theoretical realism assumes that explanations are theoretically informed descriptions of causal processes. The validity of an explanation will therefore depend on the adequacy of the theoretical description of the world on which it is based. Explanations formulated in terms of a theory that is “one-sided” or “distorts the facts” will, of necessity, be inadequate, themselves. By the same token, theories presenting a more complete and nuanced picture of the world are bound to be better. Social scientists, especially those committed to a particular theoretical tradition, often use criteria of these sorts to evaluate explanations.

often, their *relative* adequacy must be assessed in relation to existing evidence and alternative explanations.

Social-scientific *evidence* rests on claims that certain sequences of events have been or at least could be observed, either directly (i.e., first hand, with the unaided senses) or indirectly (i.e., by means of instrumentation and/or by others) (Abbot 1990; Aminzade 1992, Griffin 1993). The validity of these observational claims can be challenged either by calling into question the verity or competence of the observer or the accuracy or reliability of the instrumentation. But they can only be definitively undermined (“falsified”) by showing that *one or more of these events did not occur or did not occur in the presumed sequence.*

Social-scientific *models* also claim that certain causal processes produced (or at least contributed to) certain effects. These causal claims may be relativized by showing that the effect is more strongly associated with another possible cause, that the effect still occurs in the absence of one of the putative causes, that the putative cause is itself the effect of something else or that the effect can be caused by something other than the specified interaction. But they can only be refuted *in toto* by showing that *the causal process did not occur or that it did not contribute, directly or indirectly, to the effect(s) in question.*

Finally, an explanation makes ontological claims about the existence of certain entities and properties. These claims, too, can be challenged in a variety of ways. Classes of entities may be denaturalized by showing how they are socially constructed (Schutz 1970; Schutz and Luckmann 1967), or deconstructed by showing that the unitary representation conceals real differences (Derrida 1967). Properties may be de-essentialized by showing that they are attributes of certain positions²⁶ or they may be de-contextualized, showing that they only inhere in some small subset of the entities.²⁷ However, the only way in which a theory can be unequivocally falsified is by producing strong evidence that *the entities do not exist* or that *they cannot possess the properties attributed to them.*

²⁶This, of course, is a classic rhetorical move that Marxists and sociologists in general make—seeing things in their “social context.”

²⁷This is a rhetorical move often made by historians: showing that some broad interpretive schema simply cannot encompass the true complexity of events.

By realist standards, it will be easy to produce counterevidence but hard to produce falsifying evidence. Even when such evidence is produced, falsification may be partial rather than total. To begin with, models that are multicausal, theoretically heterogeneous, and based on several “data sets,” as many sociological explanations are, will be inherently difficult to falsify, simply because they make multiple claims at multiple levels. Moreover, falsifying a low-level claim does not necessarily imply falsification of a higher-level claim. Falsifying an observational (evidentiary) claim is not always tantamount to falsifying an ontological (theoretical) one.²⁸ All of this suggests that “crucial experiments,” in which a single piece of counterevidence is sufficient to falsify an entire theoretical tradition should be rare events—as indeed they appear to be in social science.

Since definitive falsification may prove difficult or even impossible, competing explanations can often be assessed only in light of *how well-supported they are by the existing evidence, relative to other explanations*. Generally speaking, we should regard as most strongly confirmed those models that are (1) supported by the most direct and most continuous observations, (2) able to explain the largest proportion of all cases (breadth) and the largest degree of variation between cases (completeness), and (3) premised on a small number of theoretical assumptions (parsimony) from which a large number of models can be generated (range). To sum up, the (relatively) best models are those having (in descending order of importance) the strongest evidentiary basis, the greatest explanatory power, and the widest theoretical scope. Simply because one model appears relatively more *strongly confirmed* than another at a particular moment, however, does not necessarily imply that it is truer. Newer models will generally be less strongly confirmed than older ones, simply because there has been little time to gather evidence, extend the model, or simplify the assumptions. The relative adequacy of two models can only be assessed over time.

²⁸For example, falsifying a set of observations (e.g., by showing that some event did not occur) does not necessarily suffice to falsify the corresponding causal claims. The causal process specified by the model may still have occurred, albeit in a slightly different fashion than originally assumed. Similarly, falsifying a set of causal claims (e.g., by showing that they did not contribute to the outcome in question) does not necessarily suffice to falsify the corresponding theoretical claims. Simply because some set of entities and properties were not operative in producing a particular does not imply that they are never causally relevant.

While deductivism suggests that scientific progress should be relative to the rate of falsification (or falsification of falsifications), realism implies that it should be proportional to the rate of confirmation. Social science, in other words, progresses primarily through the construction of better and better explanatory models rather than the falsification of bolder and bolder theories. Thus, while deductivism suggests that there has been little or no progress in sociology, realism implies that there has been a great deal. If few sociological theories (or falsifications of sociological theories) have been definitively falsified, a great number of more and more strongly confirmed models have been set forth.²⁹

5. ASSESSING SOCIOLOGICAL EXPLANATIONS: AN EXAMPLE FROM HISTORICAL SOCIOLOGY

Both deductivism and realism claim to be models of how social-scientific explanation and research actually work. However, deductivism has in fact become a *methodological prescription* about how social-scientific explanation and research *should* work. Explanations, it is argued, must be falsifiable. As the most consistent defenders of deductivism have realized, this means that explanations must be stated in a *logical form*. The logical consequence is *methodologism*, the view that scientific and unscientific explanations can be distinguished on purely formal grounds. Realism, by contrast, seeks to recapture the essence of falsificationism, making empirical adequacy the sole criterion by which explanations are judged. For that reason, it is ideally suited to extricating sociology from the methodological dead end into which deductivism has led it. To illustrate the point, I will now examine Theda Skocpol's *States and Social Revolutions* and Burawoy's and Lieberman's "methodological critiques" of it, and I will also present a realist analysis of my own. I begin with a look at Skocpol's inductive "methodology" and the interpretive methods that underlie it.

In her methodological essays, Skocpol (1979) uses John Stuart Mill's well-known "canons of inductive logic" or "methods of similarity and difference" to explicate the process through which sociologists can

²⁹I would challenge anyone who seriously doubts this to compare contemporary research in the major subfields of sociology with that done 50 years ago.

elicit “causal regularities in history.” Where several outcomes are similar, she argues, they can only be explained by a common causal factor. Conversely, where the outcomes are different, common factors cannot provide the explanation. The method of similarity serves to identify causally relevant factors, the method of difference to eliminate causally irrelevant ones.³⁰ Skocpol argues that by applying these basic logical operations to historical outcomes and cases, the comparative-historical researcher can “identify invariant causal configurations that necessarily (rather than probably) combine to account for outcomes of interest” (p. 378). These formal methodological pronouncements about constructing explanations, however, are at odds with Skocpol’s informal anecdotal account of the genesis of *States and Social Revolutions*. In the latter, Skocpol describes a reasoning process that is less logical than interpretive. The historical sociologist begins with “ideas about causal regularities” drawn either from “pre-existing theories” (e.g., of revolutions) and/or inference “about causally significant analogies between instances” (Skocpol 1984: 375). The sociologist then “moves back and forth between aspects of historical cases and *alternative hypotheses* that may help to account for those regularities” (emphasis in original, p. 374).³¹ In this vein, Skocpol invokes Stinchcombe’s comparison of the historical sociologist to the carpenter who “builds, adjusting the measurements as he [or she] goes along, rather than as an architect builds, drawing first and building later” (p. 385, from Stinchcombe 1978: 7). Here, Skocpol portrays the “comparative method” as a sort of dialogue between theory and observation, an interpretive reasoning process in which an explanatory model is gradually fine-tuned to fit a set of historical cases.³² Thus, there exists a clear disjuncture between

³⁰It should be noted that Mill himself warned against the use of these methods for studying history, on the grounds that they would frequently lead to erroneous inferences. As we will see, this is Lieberman’s principal objection to Skocpol’s comparative-historical method.

³¹Of course, we might “explicate” this process in logical terms: The hypotheses are deduced from a theory applied to some set of cases. Inferences are drawn, the hypotheses are reformulated, and the process repeated until the explanation “fits” the cases. As several of her critics have been quick to point out, Skocpol’s cases do not strictly speaking support the inferences that she draws from them (Nichols 1986; Burawoy 1989b).

³²There is a great deal of anecdotal evidence suggesting that many researchers, including quantitative modelers, construct their explanations in much the same way.

Skocpol's stated methodology and her actual method—a disjuncture that, lamentably, has sown a great deal of confusion.

Burawoy, for instance, characterizes Skocpol's method as "inductivist."³³ By "inferring causal explanations from 'pre-existing facts'" (1989b:759), he charges, Skocpol overlooks the role that theories play in shaping our interpretation of the facts and makes method "a substitute for theory" (1989b:769). He contrasts Skocpol's method unfavorably to Trotsky's, which he sees as "deductivist." In his analysis of the Russian Revolution (Trotsky [1933] 1977), Trotsky

roots himself in Marxism and sees his task as resolving the anomalies generated by Marxism, that is, turning counterexamples into corroborations of the Marxist hard-core premises by building new theories. (Burawoy 1989b: 786)

In other words, by "refuting refutations" of Marxist "theories" (in the terms used here: explanations) of revolution, Trotsky produces a new set of (logically) falsifiable "predictions." By contrast, Skocpol's work has no "core," for it is not situated within a single, coherent theoretical tradition. Hence, no "testable predictions" can be derived from it (Burawoy 1989b:772–76). Skocpol, charges Burawoy, thereby immunizes her theory from empirical criticism.

The problem with Burawoy's critique is that it wrongly equates Skocpol's Millsian methodology with her actual *modus operandi*. Skocpol, however, is not an "inductivist." She does not conjure up her explanation of social revolution *ex nihilo*, but constructs it out of elements drawn from two major theoretical traditions: Marxism and (old) institutionalism. It is precisely this alloying of traditions that

³³Curiously, Burawoy also includes Hempel and Popper among the ranks of the "inductivists" (1989: 759–60). In his eyes, only Lakatos's methodology of research programs qualifies as bona fide deductivism. In Burawoy's terms, inductivists infer theories from preexisting facts, while deductivists use preexisting theories to derive predictions about facts. Burawoy's peculiar characterization of Hempel and Popper as "inductivists" therefore stems from his unorthodox use of "inductive" and "deductive" to refer to two different *procedures* for constructing explanations. As we saw earlier, these terms have generally been used to characterize the *logical form* that scientific explanations take. As Burawoy defines the term, however, anyone who thinks there is a world of facts independent of theory is an inductivist.

Burawoy objects to, for in his terms an explanation is (logically) falsifiable only if it has a single, unified “core,” from which new “predictions” can be (logically) derived. In a word, Burawoy prefers Trotsky’s model because it is more parsimonious. Other things being equal, this is quite legitimate from a realist perspective. But other things are clearly not equal. To put it baldly: Trotsky explains one successful revolution, while Skocpol explains three successful ones and three failed ones. This comparatively greater explanatory power is the central reason why *States and Social Revolutions* should be (and generally has been) seen as superior to earlier works on revolution, including Trotsky’s. In sum, Burawoy’s methodological “critique” of Skocpol boils down to the formalistic requirement that explanations be theoretically homogeneous. This implies that any model not having a theoretical label attached to it is *prima facie* unscientific.

Like Burawoy, Lieberman (1992) takes Skocpol’s references to Mill at face value. Mill’s methods, he argues, will work only if we assume the social world to be “deterministic” and “monocausal.” To illustrate the difficulties involved when the methods of similarity and difference are applied to complex, multicausal processes. Lieberman uses the example of drunk driving, which frequently, but not invariably, leads to accidents. If we array drunk driving alongside a number of other factors likely to be associated with accidents and compare even a small number of cases, there is no guarantee that a “clean” and unambiguous result will be obtained. For instance, applying the method of similarity to cases “1” and “2,” we might conclude that accidents invariably occur when a drunk driver runs a red light as a car is entering from the right. On the other hand, applying the method of difference to cases “2” and “3,” we might conclude that running a red light is never a cause of getting into an accident.³⁴ Lieberman concludes: “In a probabilistic, multivariate world, it will be impossible [sic! Substitute “unlikely”!] to make valid inferences based on a small sample.” In other words, to be falsifiable, an explanation must state the exact probability of y if x and reliable values can be attained only if n is large.

³⁴Note, however, that if we apply the methods of similarity and difference jointly, as Skocpol recommends, we obtain the “correct” result—namely, that drunk driving caused the accidents in question.

As should be evident by now, Lieberson also takes Skocpol's invocations of Mill far too literally. There is actually little indication that Skocpol applies Mill's methods with any real rigor in *States and Social Revolutions*. If nothing else, it would be quite impractical—indeed impracticable—to sift through all the possible causes of revolution in this way.³⁵ Comparative-historical researchers, as Skocpol stresses, must make “strategic guesses,” theoretically grounded assumptions about “what causes are likely to be operative” (1979: 39). Only then does “inductive” (better: interpretive) reasoning come into play as the researcher attempts to isolate key causal processes and to construct an explanatory model that can account for the set of cases under consideration, what sociologists commonly refer to as a “theory of the middle-range” (Merton 1949). This limitation in “range” is, I think, at the heart of Lieberson's skepticism about Skocpol's work (and probably about comparative-historical sociology generally). For unless the number of cases is large, it will be not be possible to compute a “probability value” that is reliable and hence falsifiable. These criticisms would be justified if there were another model of revolution explaining a larger number of cases than Skocpol's *and* doing so with the same precision. But if there is, Lieberson does not tell us about it. As it stands, then, his critique amounts to little more than the formalistic requirement that models explain a “big *n*”—that they have great scope. This would disqualify virtually all nonquantitative research in sociology as unscientific.

Burawoy and Lieberson direct their criticisms at the (putative) logical procedures that Skocpol uses to construct her explanation of revolution. From a realist perspective, however, there is a fundamental distinction between the “context of discovery” and the “context of justification” (Popper [1934] 1965). An explanation must be “justified” on empirical, not procedural, grounds. The proper starting point for evaluating *States and Social Revolutions* is Skocpol's model, not her methodology.

In *States and Social Revolutions*, Skocpol summarizes her argument in the (quasi-deductivist) language of multivariate, statistical

³⁵Indeed, it is difficult to imagine how she would have done so. Had she really put together a table such as that shown in Lieberson (1992, fig. 1), arraying all the known cases of social revolution along with all its potential causes, it is unlikely she would have ever developed such a clear and parsimonious model.

analysis. The outbreak of “social revolutions,” she contends, is invariably associated with the presence of two “explanatory variables”: “state breakdown” and “peasant insurrection.” She supports this claim by showing that both variables were “present” in the French, Russian, and Chinese Revolutions, and that social revolution failed to occur in a number of cases where one or both factors were “absent” (Stuart England, Meiji Japan, Hohenzollern Prussia). Revolution, she concludes, has a “generalizable logic” that can be stated in quasi-lawlike form.

Implicit within the case studies, however, is a powerful causal model, in which social revolution is seen as the product of a particular historical conjuncture (Nichols 1986). The model contains two theoretical entities, states and classes, having two properties: (organizational) strength and control over (material) resources. These entities constitute two major causal processes: “state breakdown” and “peasant insurrection.” State breakdown occurs when a state comes under external military pressure and lacks the administrative strength to overcome noble opposition and mobilize the necessary resources. Rural insurrections occur when central authority collapses and peasants have the organizational strength to rise up against the nobility. This combination of administrative collapse and social conflict engenders a revolutionary situation, in which “marginal political elites” (e.g., intellectuals) become agents of mass mobilization. Social revolutions, in sum, result from a complex, historical conjuncture that may be visualized as a double-wedge structure.

Pace Burawoy, this model makes a number of falsifiable empirical claims. First, it is asserted that certain sequences of events can (not) be observed (*a posteriori*) in each of the cases: e.g., military defeat followed by fiscal crisis followed by administrative collapse. It is further claimed that these events were part of a coherent process that contributed to the outbreak of revolution: e.g., state breakdown. Finally, it is assumed that these processes were constituted by the variable properties (e.g., organization, resources) of certain entities (e.g., states, classes). Are any of these claims manifestly false? How strong are they in comparison to the claims made by other models? These are the critical questions that one must ask in evaluating *States and Social Revolutions*. They cannot, however, be answered by “methodologists” (or in the context of this essay) but only by historians and sociologists of revolution familiar with the evidence.

It should now be evident that deductivism and realism differ in the very way they conceive of methodology and its relationship to method. For deductivists, methodology is normative. It prescribes what explanations *should* do. Actual methods, accordingly, should be *derived* from methodology. Methodology is prior to method. Its function is to separate science from pseudo-science. For realists, by contrast, methodology is descriptive. It simply makes explicit what scientists do. Methodology is anterior to method. Its aim is simply to clarify what sorts of methods—data-gathering strategies, modeling techniques, and interpretive processes—social scientists can and do use.

6. CONCLUSION: THE ABUSES AND USES OF DEDUCTIVISM

One of sociology's worst-kept secrets is that the HDM does not work. The reason, I have argued, is that social-scientific explanations do not fit the CLM. They do not contain universal or statistical laws that could form the premise of a deductive argument. Consequently, there are no logical grounds for inferring, via *modus tollens*, that a social theory is false based on the falsity of its "predictions." Attempts to redefine deductivism in holistic or probabilistic terms do not solve the problem. They merely replace one inadequate set of assumptions with another. Social theories cannot be reduced to a set of "fundamental axioms" or "probability statements" any more than they can be stated as laws.

Explanations, I have argued, are better viewed as causal models: linguistic representations of the causal processes that produce certain outcomes. Seen in these terms, explanations make falsifiable claims—observational, causal, and ontological—whose adequacy can be evaluated relative to existing evidence and in comparison with alternative models. While these claims can be assessed only empirically, realism, as I have tried to show, can be used as a diagnostic tool to strip a model down to its barest assumptions. Realism, to paraphrase Wittgenstein, does not change an explanation in any way: it leaves it just as it is. Its only purpose is to clear the empirical ground on which any explanation stands—or falls (Wittgenstein [1953] 1993).

Given that deductivism is such a poor description of how social science actually works, it is itself a bit of a puzzle how its influence could have waxed so great in the first place. The principal reason is perhaps suggested by Stephen J. Gould's remark that the "soft sciences" have at least one thing in common: a bad case of physics envy. Physics, with its formidable powers of prediction and control, still represents the ideal toward which many researchers in other sciences aspire. Like other social scientists, sociologists have long sought to replicate the successes of physics by appropriating its methods, as interpreted by philosophers of science. So far, this strategy does not appear to have worked.

One possible explanation for this failure is that, as Popper put it, "social science has not yet discovered its Galileo." Another, more in line with the realist perspective developed here, is that sociology differs from physics not so much in its methods as in its objects. Physics—or rather Newtonian mechanics, for that is what most sociologists mean by "physics"—deals with the simplest, most uniform structures of the physical world. Sociology by contrast deals with the most differentiated and complex structures of the social world. Building good causal models is therefore inherently more difficult in sociology than it is in physics. Since sociological models will, of necessity, be more approximate and less complete, they will also have considerably less predictive power or instrumental applications.

This does not mean that causal generalizations are impossible in the social sciences, but it does suggest that their scope and power will, of necessity, be limited. Unlike Newtonian physics, sociology studies structures that are inherently historical—and in a double sense. The structures themselves, as variously defined by different theoretical traditions—classes, groups, societies—emerge, change, and disappear over time. In this sense, sociology is akin to natural science disciplines such as evolutionary biology, cosmology, or geology, whose objects are historical. But the structures of the social world are also historical in another sense, insofar as they are themselves structured by the interpretations of the past or visions of the future that animate social actors (Bourdieu 1980; Giddens 1984). In this sense, social science is fundamentally different than natural science, for it forms part of the very object that it studies. Social

science, then, really *is* history. Social science *is* “nothing but history.” The real error was ever to think it could be anything more.

To say that deductivism has been abused by methodologists is not to say that it should not be used by social researchers. Indeed, I believe that deductivism is a powerful tool that can be quite helpful in constructing and evaluating explanations. Deducing empirical predictions from our own causal models allows us to refine them—and to see their limitations. Deducing empirical predictions from alternative causal models allows us to see their limitations—and to refine them. As a heuristic and rhetorical tool, deductivism clearly has its uses. Abuses arise when we make the leap from explanation to theory.

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