Historical Sociology

Introduction

The idea of a historical sociology is surely back on the agenda. Books on the topic by Stinchcombe (1978), Tilly (1981, 1984), Skocpol (1984), and Dennis Smith (1991), not to mention a host of important original works in historical sociology by both younger writers and well-established authors, demonstrate both a new vitality and, I hasten to add, a continuing disagreement over "strategies" for joining history and sociology. There is now even a "counter-revolution" against the very idea of joining the two (Goldthorpe, 1991). Perhaps symptomatic of some confusion over the relevant issues, writers disagree even on their understanding of what some of the better-known studies are doing. At one extreme we find Arthur Stinchcombe arguing that "the difference between Trotsky's Marxism, Smelser's functionalism, and de Tocqueville's conservative dispair makes hardly any difference to any important question of sociological theory" (p. 2). Stinchcombe may be correct that "when they do a good job of historical interpretation, Marx and Weber and Parsons and Trotsky and Smelser all operate in the same way," but either some of them, at least, rarely "do a good job" or what counts as "the same way" is very fuzzy--despite Stinchcombe's interesting readings of the author's he discusses. Although entirely different in aim and approach, Dennis Smith's *The Rise of Historical Sociology* (1991), which gives an account of eighteen more recent noteworthy writers, puts him, ultimately, near to Stinchcombe as regards differences between historical sociologists. He gives evenhanded descriptions of each of his eighteen selected writers, usually in pairs, usually arranged by topics, for example under "old empires, new nations," he discusses Eisenstadt and Lipset, and under "two critical rationalists," we find Barrington Moore and E.P. Thompson. In his last chapter, then, he identifies four relevant issues: "whether historical sociologists have operated as 'outsiders', or as members of the relevant 'establishment'; the way they handle problems of involvement and detachment; third, their orientations toward theory, empirical generalization and primary exploration of historical data; and fourth, the strategies of explanation they adopt." Important as these notions are, they seem to be almost afterthoughts. They do not drive the accounts of the writers he discusses. More important, perhaps, he is very uncritical as regards notions of theory, generalization, and explanation. And on all four of the issues, he finds no clear lines at all. For example, he offers four "strategies of explanation," competitive selection, system contradictions, infrastructural capacities and dominant routes of social change. For me, these are not strategies of explanation, but theoretical orientations, predicated very much on very different notions of history and society. In any case, he finds <u>some</u> combination of these in all the writers he has discussed. Presumably, Smelzer's work exemplifies the dominant route strategy--a stage theory, but in a less pure form, it is found in Runciman, Wallerstein, Lenski, Moore and Anderson. With Moore and Anderson, however, it is complemented with "evolutionist assumptions," <u>and</u>, in the case of Moore (Anderson is not mentioned?), "great attention to the infrastructural capacities of dominate and subordinate classes within agrarian polities." He thus meets Mann, who, as it turns out, shares with Lenski "a location between infrastructural capacities and dominant routes." One could on. But the upshot is the feeling that historical sociologists are quite messy beasts and that as far as method or strategy is concerned, an eclectic anarchy is to be recommended. On the other hand, one wonders whether the problem resides more in the way that the material is being conceptualized by these commentators. It may be that as with much talk about science, the problem stems from an inapt theorizing of the practices.

A similar generosity is found in Tilly's suggestive, but finally unhelpful classification. Tilly (1984) identifies four approaches to historical sociology. For him the key difference is the mode of comparison: individualizing, universalizing, variation finding, and encompassing comparisons. As he would acknowledge, I think, these are all very wobbly notions. Consider, e.g., this remark:

> If we needed a pedigree for individualizing comparison, its use by Max Weber would suffice. When Weber started elaborating his great taxonomies, he bowed toward generalization. When he spoke of rationalization and charisma, he gestured toward universalizing comparison. But his wide comparisons of religious systems served mainly to specify the uniqueness of the achieving, accumulating, rationalizing bureaucratic West. To a large degree, Max Weber used comparison for the purpose of individualizing (p. 88).

As regards individualizing and generalizing, one is tempted to say here that he could hardly have avoided this! In the first place, generalization is quite indispensable. Whenever we use an abstract noun, we are committed to a generalization, however vague, however "open textured" or inexact. Thus, when we identify an event as a revolution or a place as city, etc., we assume that there are some properties connoted by the term which enables us to say that a revolution occurred in France in 1789 but not in 1968, that Madras is a city, but Albion, New York is not. It may be there are attributes held in common, what Weber called "generic" terms: "For all (or many) x, if x is F, then it is G," or perhaps one can get only only family resemblances, the famous "games" of Wittgenstein: ""For all x, x is F if it is G.H.J <u>or</u> H.J.K <u>or</u>..." If so, then perhaps "ideal types" are demanded (Manicas, 1992).

There are several real questions here. One, clearly seen by Tilly, is the question of whether our interest is in what is distinctive or in what is common. Weber insisted, rightly, I think, that theories about what is true of all bodies are be interesting to us; but when it comes to the human sciences, it is concrete in all its individuality which interests us. Another, not clearly seen by him, is what one does with generalizations? In particular, is it the main goal of sociology to seek generalizations (and law-like statements) *in order to* explain? Presumably, it is just this that distinguishes sociology as a science and separates it from history--the familiar "nomothetic /idiographic" divide. Even more ambitiously, should sociologists be seeking universal generalizations, e.g., James Davies' J-curve model, generalizations true of all societies?

Tilly offers that "the relative value" of the strategies he identifies "depends upon the intellectual task at hand." But he also sees that the value of strategies "depends on the nature of the social world and the limits to our knowledge of that world." But might it be that depending on such considerations, some strategies, for example, "universalizing" are not valuable at all? Indeed, one would have thought that assumptions about the nature of the social world and the limits of our knowledge of it was fundamental, so fundamental that once commitments on this were clear, there would severe limits to one's generosity about approaches. For example, either history proceeds in stages, or it repeats itself in some useful sense, or it does neither. Either there are useful transhistorical "universals" or there are not. Either explanation in the human sciences requires

methods different from methods employed in the physical sciences or it does not. Etc. Finally, Tilly, as most commentators, finds that no historical sociologist is consistent, inevitably, if not consciously, pursuing alternative strategies in the same inquiry.

The account by Theda Skocpol (1984) confirms the suspicion that there are some considerable unresolved difficulties in our understanding of the character and task of historical sociology. Skocpol is also unwilling to risk dogmatism. She offers a trichotomy of stratgies. She focuses on different notions of what counts as explanation and, like Tilly, she argues that her three strategies "are not hermetically sealed from one another," that "creative combinations are and always have been practiced." If the objection to Tilly is that if individuating and generalizing are inseparable, he does not have a useful principle of classification, Skocpol has a different problem. Explanation strategies are not mutually compatible. If one mixes them, then, one is left a worse mess than the pauper's broth. How, for example, does one combine explanation by appeal to covering laws with narrative? And it may be also that what seem like "creative combinations" are thought to be combinations only because something is amiss as regards the classification. Thus, Skocpol asserts that Wallerstein and Perry Anderson combine two strategies, a general explanatory model with interpretative historical investigation. But if, as I shall argue, there is an *inevitable* hermeneutic moment in any *causal* historical explanation, we need to rethink her trichotomy.

In this essay, I propose what seems to me to be a coherent classification of strategies (or approaches), and offer, in this light, a less generous account of alternative strategies. There is, I think, one very much preferred approach, but my defense of it will be less than complete, emerging as part of my effort to clarify some of what I see as confusion. It is not that one cannot find valuable insights in most of the major efforts in historical sociology. It is rather that, these are often accidents in the sense that they were not only not promoted by the explicit strategy of the author, but would in fact, have not been there at all had the author been clear about his or her commitments.

My focus is issues in the philosophy of science, including here philosophy of history. Indeed, one suspects that the rediscovery of history by social scientists was due, in part at least, to the onslaught against the conventional wisdom in the philosophy of science which beginning in the 1950's has resulted in a near total re-evaluation of the status of the human sciences as sciences (Cole, 1994).

Four main conceptions of historical sociology defined in terms of how historical explanation is conceived are distinguished. The first, which I call "generalizing historical sociology" accepts the (positivist or empiricist) idea that explanation in history proceeds by subsuming what is to be explained under general laws, the "covering law" model. It thus has a generalizing and universalist thrust.

The second, usually termed "realist," rejects this idea of explanation along with the idea that explanation and prediction are symmetrical. It holds instead that explanation in <u>both</u> the human

and physical sciences is always concrete and particular, and because of this, we are often able to explain when we could not have predicted. It assumes a non-Humean notion of causality, of causes as productive powers. Explanation then proceeds by providing a causal history, exhibiting the particular set of causes at work in producing the particular event or outcome.

The third, which we can call (following Theda Skocpol) "interpretative historical sociology" shares in its emphasis on the concrete and particular but eschews explanation in either of the foregoing two senses. Interpretativists quite rightly insist on the meaningfulness of human action, but still caught by the false dualisms which are the legacy of positivism (Bhaskar, 1978), interpretivists characteristically reject causal models altogether. Instead, following an influential, but in my view, incorrect reading of Weber, they seek "meaningful interpretations of history" (Manicas, 1992a).

The fourth conception, which we call (following Tilly) "encompassing," is, strictly speaking, neutral between the three previous strategies. Its distinguishing feature is the idea that world-history must be conceived as a whole and that differences and similarities are explained as consequences of differing relations to the whole. Examples of encompassing realists include Wallerstein (1974) and Wolf (1992).

Generalizing Historical Sociology: Two Sub-Types

The principle here is the notion that outcomes are explained as instances of a more general body of laws (or theory). The laws are "universal" in the sense that as conditionals, they apply everywhere and anywhen: If the antecedents are satisfied, then (ceteris paribus) the conditional is satisfied. Thus, if true, the conditional, "For all x, if x is F, then x is G" is true of everything in the universe at any time or place. (It is trivially true of all x's which are not F, a straightforward consequence of the standard (positivist) reading of universal conditionals and, not incidentally, the fatal obstacle to providing a positivist analysis of laws of nature (Chisholm, 1946). Both Tilly and Skocpol have something like this category, but because they are unclear (and evidently in disagreement about its character) fill it with very different cases. Tilly sees, rightly, that while in *Social Origins of Dictatorship and Democracy* (1966), Moore "feinted" in the direction of providing a general structural explanation for revolution, he did not pursue it. Skocpol, of course, did. Skocpol, however, disconnects her own work from work which applies "a general model to history." Included in this category for her is work by Smelzer (1959), Charles, Louise and Richard Tilly (1975), and Wallerstein (1974).

Skocpol is correct to see important differences between her work and that of Smelzer (and other functionalists), but fails to see the importance of her explanatory commitment. We argue that it far more useful to say that there are two sub-types of generalizing historical sociology, the first, popular during the heyday of structural-functionalism, is "functionalist universalist," the second is the sort practiced by Skocpol. While different, they share in thinking that explanation proceeds by formulating general explanatory hypotheses. For Skocpol, "the investigation assumes that causal regularities--at least regularities of limited scope--may be found in history" (1984: 374).

Functionalist Universalism

The basic orientation of the first sub-type, "functionalist universalism" is well-expressed by S.M. Lipset:

> From an ideal-typical point of view, the task of the sociologist is to formulate general hypotheses, hopefully set within a larger theoretical framework, and to test them. His interest in the way in which a nation such as the United States formulated a national identify is to specific propositions about the general process involved in the creation of national identities in new nations. Similarly, his concern with changes in the pattern of American religious participation is to formulate and test hypotheses about the function of religion for other institutions and for the social system as a whole...These are clearly not problems of the historian. History must be concerned with the analysis of the particular set of events or processes. Where the sociologist looks for concepts which subsume a variety of particular descriptive categories, the historian must remain close to the actual happenings...(Quoted in Tilly, 1984: 5).

The basic idea is clear. The task of the (historical) sociologist is to use theory to generate some general hypotheses which if true would explain the particular event under examination. For the functionalist, the theory will be version of "structural functionalism."

First, it is easy to show that functionalist explanations are appropriately rendered as a species of the covering law model (Nagel, 1961: 403):

> Every system S with organization C and in environment E engages in process P;
> If S with organization C and in environment E does not have A, then S does not engage in P;
> Hence, S with organization C must have A.

In Lipset's example, we have hypotheses about "the general process" of nation-building. We might argue that a key modernizing process is differentiation, including the increasing division of labor in society, increased institutional separation and thus accentuated individualism. For example: "Whenever a society undergoes modernization, there is an increasing division of labor in society," etc. Coupled with other hypotheses, e.g., "When religions institutions are weakened, there is a loss of normative control," we are led to the conclusion that an essential requirement for the continuing stability of the social system is the development of organs of "authoritative interpretation and enforcement"--a legitimated legal system and the coercive forces of the police. With this theory, then, one goes into history and examines nation-building in a variety of contexts.

Comparison to the Second Sub-Type

For the functional universalist, following on Parsonian notions regarding a general theory of society, histories are case studies meant to elaborate and demonstrate the validity of universally valid theoretical ideas. Case studies allow the theorist to move from the abstract to the concrete. By contrast, the analytic sociologist "aspires to generate new explanatory generalizations

through comparative historical analysis," or alternatively, to "discover causal regularities that account for specifically defined historical processes or outcomes, and explore alternative hypotheses to achieve that end" (Skocpol, 1984: 362). For example, while Skocpol hopes to induce a general theory of modern revolution from studies of cases, Smelzer hopes to test his general theory against cases. They thus differ fundamentally in the use to be made of history. They differ also as regards functionalism and, less clearly, regarding claims about the scope of their explanatory generalizations. But they share in their commitment to the covering law model of explanation.

Skocpol's criticism of the functional universalists is pertinent. First, as noted, "the model itself has to be taken as given prior to its historical application." It is important to stress what this must assume: It assumes that there are universal principles of social reproduction and social change, even if the particular forms they take are historically variable. If, however, as Mills (1959) long ago insisted, we do not know of any such any universal principles, that these vary with the social structure we are examining, then this criticism is fatal. Second, how can we be sure that different investigators would concretize such abstract concepts as "differentiation" or "mass organization" in the same way? Here the problem is the wobbly character of critical general terms. Perhaps indeed, in order to "save" functionalist assumptions about change, almost anything can be made to count. Finally, and following on this, perhaps historical facts are omitted or distorted to fit the preconceived theory. There are other problems of functionalist theory, some familiar since at least Nagel and then Hempel (1965) scrutinized reigning functionalist theory. Neither believed, we may note, that social science was capable of providing convincing functionalist explanations. One wonder why it has been so difficult to exorcise them (Giddens, 1980).

For Skocpol, analytic historical sociologists take a different tack. While they "acknowledge the desirability of generalizable explanatory principles" (1984: 375), they stand between those who seek "a single overarching model" and those who restrict themselves to "the meaningful exploration of the complex particularities of each singular time and place..." (1984: 374).

We shall look a bit more carefully at Skocpol's own work as an analyst, but first we must notice that much Marxist historical sociology is, in our terms, functional universalist (Giddens, 1981). It has been a continuing and valid criticism of Marxist historiography that it is "schematic" and forces, against the facts, history into the causal boxes and stages so much favored by orthodox Marxism.

Analytic Historical Sociology

While it is quite possible to pursue the hypothesis-testing strategy of analytic historical sociology with but a single case, far more typically, comparative studies are called for. Comparative hypothesis-testing follows the "logic" of Mill's Methods, codified by John Stuart Mill in 1843. It will pay to be clear about what they are--and what are their uses and limitations.

Mill's Methods

First, we need to notice that Mill followed Hume in holding that "causal regularities" were to be understood, as Comte put it, as "relations of invariant succession and resemblance." Although rarely articulated, it is an absolutely critical background assumption of almost all "nomothetic" sociological inquiry. Mill's methods, then, were specifically designed to identify "causes" in this sense.

Mill distinguished five methods. Let us begin by stating what these methods are, using Mill's own words.

The Method of Agreement

> If two or more instances of a phenomenon under have one circumstance in common, the circumstance in which alone all the instances agree is the cause (or effect) of the given phenomenon.

Schematically,

ABC (x_1, x_2...) ? E (y_1, y_2....)
ADF (x_1, x_2...) ? E (y_1, y_2....)
Probably A is cause (or determining condition) of E

Capital letters ABC etc. are hypothetically identified possible causes of E. The small letters in parenthesis represent unknowns present in the situation but not part of our analysis. There is no way to avoid these no matter how sophisticated or complete our analysis of the possible relevant factors.

It is critical to be clear about what it means here to say that A is the probable cause of E. It is probable cause in that B,C,D and F have been eliminated as causes in the sense of necessary conditions for E. (We can define "necessary condition" as follows: X is a necessary condition for Y if when x is absent Y is absent. For example, oxygen is a necessary condition for combustion.) That is, since E occurred in the *absence* of B, C, D and F, they are not necessary for E.

Notice in the schema the relevance of the unknowns, x_1, x_2... We just don't know whether or how they are functioning in bringing about E. Hence we can say strictly only that B, C, D and F are eliminated as necessary conditions.

The Method of Difference

> If an instance in which the phenomenon under investigation occurs and an instance in which it does not occur have circumstance in common save one, that one occurring in the former, the circumstance in which alone the two instances differ is the effect, or the cause, or an indispensable part of the cause, of the phenomenon.

Schematically,

ABC (x_1, x_2...) ? E (y_1, y_2....)
BC (x_1, x_2...) ? E (y_1, y_2....)
Probably A is the cause (or determining condition of E

There is a symmetry to the method of agreement, except in this case, A is the probable cause of E in the sense that B and C have been eliminated as *sufficient conditions* of E.(We can define 'sufficient condition' as follows: X is a sufficient condition for Y if whenever X occurs (or is present), Y occurs (or is present). For example, cessation of heart functioning is a sufficient condition for death.) Again, the unknowns x_1, x_2, etc. may well be involved in the outcome E and in the non-outcome not-E.

The Joint Method of Agreement and Difference

The joint method is frequently thought to be nothing more than a combination of the methods of agreement and difference; but it is not. Indeed, where it is not possible to use either the method of agreement or difference, the joint method may often be employed. As Mill rightly recognized, this is almost always the case as regards the social sciences where a controlled experiment is not possible. In Mill's (rather convoluted, but precise) formulation:

> If two or more instances in which the phenomenon occurs have only circumstance in common, while two or more instances in which is does not occur have nothing in common save the absence of that circumstance, the circumstance in which alone the two sets of instances differ is the effect, or the cause, or an indispensable part of the cause, of the phenomenon.

Schematically,

ABC (x_1, x_2....) ? E (y_1, y_2....)
ADF (x_1, x_2....) ? E (y_1, y_2....)
GH (x_1, x_2....) ? not-E (y_1, y_2....)
MN (x_1, x_2....) ? not-E (y_1, y_2...)
Probably A is cause (or determining condition) of E

Following on the logic of the previous two methods, A is probable cause of in the sense that B, C, D, F are eliminated as not necessary conditions for E and G,H, M, and N are eliminated as not sufficient.

Finally, Mill offers what are two variations of the method of difference. Only one needs here to be mentioned.

The Method of Concomitant Variations

> Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner is either a cause or an effect of that phenomenon, or is connected with it through one fact of causation.

This is, we suspect, the sort of business that Tilly had in mind in talking about "textbooks and learned essays" which hold that all valid comparison is variation-finding. It is also the basic logic of those quantitative methods which talk of "dependent" and "independent" variables. One may

guess that it is just this which drives the thought of text writers, some "learned" essayists and, we should add, sociologists who remain in the grip of Humean notions of causality.

Application of Mill's Methods in Analytic Historical Sociology

Comparativists have, we think, often misunderstood the pertinence and limits of these methods. To fix our discussion consider two examples of the use of Mill's method, one from the natural sciences, the other from the social sciences.

Suppose that you are Louis Pasteur interested in determining the cause of fermentation. You select a variety of fermented liquids, including beer, wine, vinegar and cider. Microscopic examination shows that each has a characteristic microorganism (which, as it turned out was *Mycoderma aceti*, a kind of yeast). You conclude that this microorganism is the cause of the fermentation.

Consider then Skocpol's use of Mill's methods. She has three positive cases to be explained, the social revolutions in France, in 1789, in Russia, 1917 and in China, 1911. In two very interesting chapters she undertakes a comparative-historical analysis in which she considers these and though briefly, three "negative" cases, or situations where there were no social revolution. (While her emphasis is on the positive cases, strictly, she employs Mill's joint method.) She concludes that the three positive cases have in common "(1) state organizations susceptible to administrative and military collapse when subjected to intensified pressures from more developed countries aboard and (2) agrarian sociopolitical structures that facilitated widespread peasant revolts against landlords..." (1979: 154). Taken together, she concludes that these are "the sufficient distinctive causes" of these revolutions.

First, in both examples, it is clear that considerable theory is involved since theory is essential if we are have an idea of what to look for, of what are the character of the likely causes and how they are to be identified. Pasteur looked for microorganisms; Skocpol looked for "structural conditions" and included as features of these, facts about the political economy, the institutions of the state and the international political and economic environment. She could have surely looked elsewhere. She might, e.g., have not looked at "structural conditions" or merely put them in the background; she might have theorized "structures" differently, or instead of her focus on political economy, she might have considered long-term cultural facts, e.g., the role of the Catholic Church, the Reformation, etc.

Second, there are important differences as regards causation in the two cases. *Mycoderma aceti* is the cause of fermentation in a very ordinary sense. It is an identifiable "thing" which, in appropriate circumstances, *produces* fermentation. It is the difference in the prevailing state of affairs which *brings about* change in the wine. Is there an analogy in Skocpol's account? Skocpol recognizes, of course, that her "sufficient distinctive causes" are not all like Pasteur's *mydcoderma.* Indeed, if anything compares to the *mycoderma*, it might be King Louis XVI! She herself writes: "...as everyone knows, the summoning of the Estates-General [by the King] served not to solve the royal financial crisis but to launch the Revolution" (p. 65). Could not one say, as

many historians would, that this was a cause of the revolution (Hexter, 1971). Of course, he did not intend "to launch a Revolution" and we can only guess at how things would have developed had the King chosen to do otherwise (as he surely could have.) On the other hand, there is something very right about Skocpol's interest in structural conditions.

We need to get clearer about the *mycoderma* example. We can then pursue the analogy a bit further. Causes are sometimes analysed in terms of necessary and sufficient conditions. We noted earlier that the method of agreement eliminated conditions as not necessary. In this case, can we say that the *mycoderma aceti* is a necessary condition for fermentation? It may be unless (as in the case), there are other agents which will bring about fermentation. That is, there is a whole set of organisms which, in the right environments, produce fermentation. ('Fermentation' is an abstract term and we need to aware that there are also very different kinds of fermentation. We see, accordingly, the pertinence of the unknowns in our schema, y_1, y_2...

Nor (even assuming we now employ the method of difference) can we say that *mycoderma aceti* is a sufficient condition for fermentation for in addition to a fermenting agent, fermentation requires the presence (or non-occurrence) of many other conditions as well. Some of these may be too obvious make a fuss over, e.g., the presence of a fermentable liquid; others may be less obvious, e.g., a temperature range which will permit the process to begin and proceed, or a host of non-events, e.g., the absence of an accident in the laboratory which would affect the outcome.).

In general, when seeking the causes of happenings in the real world, we will almost never be in position to say that have the cause in the sense of the full set of necessary and sufficient conditions. (Very strict laboratory controls, of course, can give us considerable confidence about what is necessary and what is sufficient for laboratory outcomes.)

This also raises serious questions about the relation of the use of Mill's methods and the problem of generalizing. That is, it is one thing to say on the basis of an inquiry which employed the methods that, in the particular case at hand, C is (probably) the cause of E and quite another to say: '(Probably) whenever C, E' or 'whenever non-C, non-E.' We may have very good reason to assert the former and have very little reason to assert the latter--unless, that is, we have secured experimental closure.

Moreover, and important here, singular causal explanation is not to be analyzed in terms of the covering law model. As Woodward (1984) writes:

> Singular causal explanations wear the source of their explanatory effect on their face--they explain not because they tacitly invoke a "hidden" law or statement of sufficient conditions, but because they identify conditions such that changes in these conditions would make a difference for whether the explanandum phenomenon or some specified alternatives to it would ensue (p. 253).

In his critique of "generalizations" as explanatory, Weber, no Humean, warmly endorsed singular

"causal propositions" precisely because they denominate "circumstances and change within concrete reality" as "effected" and as "effective." (See Manicas, 1987: 130.)

What then of Skocpol's analysis of the causes of social revolution? While (unsurprisingly, given the foregoing), it does not provide the hoped for "valid, complete explanation of revolution," the analysis does give us deep insight into *conditions for modern revolution*. Notice here that we did not say necessary or sufficient conditions. There is little reason to believe that, lacking the capacity to experiment, we could arrive at knowledge of either the necessary of sufficient conditions for revolution. Thus a collapse of the state coupled with widespread peasant revolts, along with inevitable other conditions, *could* lead to a restoration of the old order and not "the emergence of new sociopolitical arrangements." Consider, e.g., Iran in 1953 where Mohammed Mossadegh's revolutionary attempt was thwarted with assistance from the U.S. CIA. On the other hand, one could have a social revolution even where the state has not "collapsed," e.g., the Cuban or Sandinista revolution which succeeded because armed insurgents were able to defeat the forces of the existing state. That is, while Skocpol has given us conditions for revolution, they need not be either necessary or sufficient.

In turn, this raises questions about her "explanation." The analytic historian is seeking explanatory generalizations. These have the following form:

If C_1 (and C_2...), then E (explaining generalization)
$\underline{C_1 \text{ (and } C_2\text{...)}}$
Hence, E (the event to be explained).

In this case,

> If a state organization susceptible to administrative and military collapse is subjected to intensified pressures from developed countries abroad <u>and</u> there is widespread peasant revolt facilitated by agrarian sociopolitical structures, then there will be a social revolution.
> In 1789, France was subjected to such pressures and had an agrarian social structure which facilitated widespread peasant revolt.
> Hence, France in 1789 had a social revolution.

The first premise is the "explanatory generalization." We can replace China or Russia for France in the second premise and thus also "explain" their social revolutions.

There are two problems. First, the argument is a perfectly valid deduction; but if the conditions are not sufficient, the first premise is false. Indeed, *it is not clear whether any such explanatory generalization could be true*--without being trivial. As David Lewis writes:

> Nobody thinks that real-life explainers commonly serve up full D-N arguments which they

hope are correct. We very seldom do. And we seldom could--it's not just that we save our breath by leaving out the obvious parts. We just don't know enough (1986: 203).

The point holds as regards not only the human sciences, but as regards the physical sciences as well.

But if so, we should ask whether *any* scientific explanation proceeds in this fashion. While the covering law model is a *defining* attribute of "empiricist" (positivist, neo-positivist) understandings of science, there is now a substantial critical literature which has subjected this assumption to fatal criticisms (Scriven, 1959, 1962; Harre, 1970, 1986; Dretske, 1977; Bhaskar, 1975; Salmon, 1978, 1984; Achinstein, 1981; Aronson, 1984; Woodward, 1984; Lewis, 1986; Kim, 1987; Manicas, 1987, 1989). Here, I merely list some the main lines of such criticism.

First, after something has happened, we are often able to explain it while we could not have predicted it--even with a modest measure of probability. This is typical not only of social revolutions, but of the weather, a hotel fire, a fatal stroke, the emergence of a new species, a powerful upswing in the economy. The reasons for this are clear enough: There is both radical contingency in history, and time (as current complexity theory emphasizes) makes a difference: What happens has effects on what will then happen, and so on. Second, "laws" which subsume instances (still less "mere" generalizations!) cannot explain since "entails" is the wrong relationship. Thus, Dretske: 'The fact that *every* F is G fails to explain why *any* F is G.' While at least a true universal maintains the hold on the individual case, anything less makes the main point vivid. Perhaps 67% of people exposed to Herpes I contract it; but why did Sam contract the disease (and why didn't Harry who also was exposed?) Third, it is easy to construct counter-examples where *true* "explanatory arguments" are just plain silly: Thus:

Nobody who takes birth control pills regularly becomes pregnant.
John took his wife's pills regularly.
Hence, John did not become pregnant.

Finally, neither generalizations nor "laws" as analyzed by empiricists can support counterfactuals. If the antecedent of a conditional is false, then *for that reason* the conditional is true. For example, on the Humean analysis, the conditionals: "If Hitler had not invaded Russia, he would have won the war" *and* "If Hitler had not invaded Russia, he would not have won the war" are both true!

The examples suggest what is amiss. Causes explain. Everybody recognizes that good correlations need not be causal (even while, given their Humean presuppositions all our texts flounder in helping us to know when we have a cause and not a "mere" correlation). But we reject the Humean account: A causal relation presupposes a *nomic* and *necessary* connection. We need not balk at this. Indeed, Jaegwon Kim is prepared to say that "most philosophers will now agree that an idea of causation devoid of some notion of necessitation is not *our* idea of causation--perhaps not an idea of causation at all" (1987: 234). This is not, unfortunately the place to give a full-fledged analysis. (But see Harré and Madden, 1975). Still, the basic idea is clear enough.

Causes *bring about* their effects, either as events which initiate a change in circumstances, e.g., the match which lights the fire, or as mechanisms with causal powers, the combustible material which burns (and doesn't evaporate or become vinegar!) We can see the relation of causes to counterfactuals: If the match was the cause of the fire, then had it not been lit, there would have been no fire. If a cause of the German defeat was the second front in Russia (not an easy matter to decide!), then only one of the two preceding counterfactual can be true. (Both, of course, may be false.)

Once we have an adequate notion of causality, it is easy to see what is going on when we seek explanation. To explain, one needs to show what events and mechanisms *combined* to produce the outcome. Indeed, the presumed limitations of the human sciences are rooted in both fundamental misunderstandings of science and, as Weber saw, our dominating interest in the human sciences in explaining the concrete. The logic of explanation is the same. Explaining revolutions, like explaining the fermentation of particular samples of wine, requires a narrative which identifies the causes open-systemically at work in the world, where perhaps none are either necessary or sufficient conditions. In *none* of the sciences (pace the D-N "ideal") can we hope to find a "complete explanation" since there will always be yet to be identified bits and pieces in the unique causal history of any event. Indeed, there is a sense in which providing causal explanations in the human sciences is both easier and harder. It is easier in that human action is absolutely critical to what happens in history. But it is more difficult in that there are immense (theoretical) difficulties in identifying the "conditions" which enabled and constrained actors, a consequence of the absence of the capacity to construct system closure: to experiment.

Unfortunately, it is all too easy to obscure what is the critical point of difference between notions of causality and explanation assumed by analytic historical sociologists and by realists. As noted, the ordinary sense of causality is realist. Accordingly, even explicitly non-realist accounts are inevitably infected with realist notions regarding causality. (This leads to stunning contradictions in our textbooks.) In historical sociology this is exacerbated, as with Skocpol's *States and Social Revolutions*, where there are narratives. In a later essay, coauthored with Somers (1980), Skocpol and Somers write:

> It may be easier in articles [than in books] to highlight causal arguments...and to avoid the temptations of presenting lengthy descriptive chronologies *for their own sake* (1980: 194f., my emphasis).

But these narratives were not merely "descriptive chronologies for their own sake." On the contrary: It is just these which makes us think that her "causal arguments" do the explaining!

Agents and Structures as Causes

We noted earlier that it is widely acknowledged that the King's decision to convene the Estates-General "launched" the revolution. This is good causal language. Indeed, here we have the analogy of igniting a fire. But of course, to have a fire there needs to be (at least) something which will burn and oxygen. Obviously, these "conditions" do not always lead to fires. A host of other

"conditions" (causes) are necessary.

Pressing the analogy, if the King--without intention--"launched" the revolution, it was because his act provoked people, enabled and constrained by what we now can identify as revolutionary conditions, to engage in what turned out to be structurally transformative activity. As any historian would insist, to explain the revolution one needs to tell the story which begins at least with the King's decision. But as the sociologist would also insist, most historians fail to give attention to the structural conditions which set the problems and which enabled and constrained the actors. By recognizing that actors work with "materials at hand," the realist historical sociologist tries to do both.

Realist Historical Sociology

A fundamental assumption of realist historical sociology is the by now familiar idea that social structure is both medium ("the materials at hand") and product of action (the consequences, intended and unintended of action). The "materials at hand" are, to be speak broadly, the structural conditions prevailing in any given social order. These might include the basic forces and social relations of production, the prevailing gender relations, orders or classes, and the particular character of regimes, all thoroughly suffused with what is usually termed "culture" (Manicas, 1992b). These set the problems for actors and provide the medium for dealing with them.

The realist does not assume that there is a set of necessary and sufficient conditions which could explain some outcome. The realist attempts to get the clearest possible understanding of the particular conditions prevailing and how agents understood and employed these materials--almost always with unintended consequences. Because such conditions have their particular nature by virtue of their past, this will involve a complicated causal history.

We can briefly illustrate this with a look at the work of Barrington Moore Jr., Charlies Tilly and Perry Anderson.

Moore's set the task of his monumental *Social Origins of Dictatorship and Democracy* (1966) in the following terms:

> This book endeavors to explain the varied political roles played by the landed upper classes and the peasantry in the transformation of from agrarian societies...to modern industrial states. Somewhat more specifically, it is an attempt to discover the range of historical conditions under which either or both of these rival groups have become important forces behind the emergence of Western parliamentary versions of democracy, and dictatorships of the right and the left, that is, fascist or communist regimes' (p. xi).

After many years of immersion in a variety of historical materials, Moore saw a critical general process driving modernization. It was, as he saw it, the transition to commercial agriculture. As just noted, the key agents were peasants and the landed upper classes. The problem then becomes what in particular cases either aided or prevented this transition and how did these differences

help to explain the fact that some nations become parliamentary democracies, e.g, England or India, some fascist, Germany and Japan, and some communist, e.g., China. Like the historian, Moore seeks to understand the histories of specific countries, but as a sociologist, he insists on the need to identify, with theory, the enabling and constraining structural conditions of actors. Moreover, for him, comparisons serve the double advantage of sharpening one's understanding of the particular and of serving as a check on the conventional wisdom.

Between Germany, Japan, India, the United States, Russia, China, England and France there were significant differences in the conditions. For example, while most had "feudal pasts" (only the US did not), there were important differences in the character of what we abstractly call "feudalism" and these differences figure heavily in what occurred. There were differences also in the time that the processes was initiated and completed and that made serious differences for those countries which were late modernizers.

Perry Anderson's *Lineages of the Absolutist State* (1974) has a similar logic. He writes:

> ...the aim of this study is to examine European Absolutism simultaneously 'in general' and 'in particular': that is to say, both the 'pure' structures of the Absolutist State, which constitute it as a fundamental historical category, and the 'impure' variants presented by the diverse monarchies of post-mediaeval Europe' (p. 7).

Anderson rejects both the sociologists thirst for "universal categories" and the historian's rejection of "abstract models."

As with Moore, after years of immersion in historical materials, Anderson arrived, via abstraction, at a general model for the place and period under study. This led him to argue that Eastern Europe was not "merely a poorer copy of that of Western Europe, which can be added side by side to it, without affecting the study of the latter" (p. 9). In the first chapters of each of two parts of his book, Anderson then abstractly theorizes the emergence of the absolutistic state in both the West and the East. In summary:

> The Absolutist State in the West was the redeployed political apparatus of a feudal class which had the accepted the commutation of dues. It was a *compensation for the disappearance of serfdom*, in the context of an increasingly urban economy which it did not control and to which it had to adapt. The Absolutist State in the East, by contrast, was the repressive machine of a feudal class that had just erased the communual freedoms of the poor. *It was a device for the consolidation of serfdom, in a landscape scoured of autonomous urban life or resistance*(p. 195).

Having identified in these chapters the key structures employed by actors, Anderson then develops narratives of histories of states in Europe, employing his model to show how in each of these there were critical variations which help explain the very different outcomes. For example, in Italy, because mercantile capital had already developed in the north, Frederick II failed--despite his

"many assets"--to extend his relatively advanced baronial state from his base in the south. The consequences of this failure on subsequence development were significant. Or, "Swedish Absolutism...was built on a base that was unique, because--for historical reasons outlined earlier--it combined free peasants and nugatory towns." This absence of significant urban centers meant the absence of a challenge from the bourgeois. This combination was to give the Swedish monarchy both its stability and "its distinctive cast" (p. 180).

It is easy to fail to see what Anderson was up to. These chapters (10 in all) are not case studies meant to confirm the logic of the causal model; rather they are offered in their own right as, he hopes, better particular histories of European states in the period under study, better because by identifying key differences in inherited structures, they conjoin theory and history. In contrast to Skocpol, Anderson's narratives are not gratuitous. It is precisely these which do the explaining.

Interpretative Historical Sociology

Interpretative historical sociologists eschew causal explanation and seek what is usefully called "a meaningful interpretation." This leads such writers (as Skocpol writes) to pay especial attention to the intentions of actors and to put in the background (or omit) the sorts of structures theorized by Skocpol or Moore. Since causal explanation is rejected, description and explanation tends to collapse. Interpretative historical sociologists are skeptical of the sort of theory employed by both generalists and realists, but many find useful Weber's conception of the ideal type. As with realists, however, in their comparative work, interpretative historical sociologists are interested in individuating and in establishing significant differences between what is compared. As with realists (now quoting Reinhard Bendix):

> By means of comparative analysis I want to preserve a sense of historical particularity as far as I can, while still comparing different countries. Rather than aim at broader generalizations and lose that sense, I ask the same or at least similar questions of divergent materials and so leave room for divergent answers. I want to make more transparent the divergence among structures of authority and among the ways in which societies have responded to the challenges implicit in the civilization accomplishments of other countries.[1]

Bendix well represents the interpretative approach in theorizing in terms of "ideal types."

We can illustrate this briefly with reference to Reinhard Bendix's impressive, *Kings or People* (1978). Like Moore, Bendix is interested in modernization, but the difference is in orientation is clear from the very first pages:

> It is easiest to define modernization as a breakdown of the ideal-typical traditional

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[1] Quoted by Theda Skocpol, *Vision and Method in Historical Sociology*, p. 370, from Reinhard Bendix, 'The Mandate to Rule: An Introduction,' *Social Forces* 55 (1976), p. 247.

> order: Authority loses its sanctity, monarchy declines, hierarchical social order is disrupted. Secular authority, rule in the name of the people, and an equalitarian ethos are typical attributes of modern society (p. 10).

"The traditional order" is ideal-typically defined in the sense that the several features singled out are true more or less of pre-modern society, some may be present, some, sometimes absent. Ideal types do not pretend to be strict definitions. Indeed, while they are not fictions, nothing really "corresponds" to them. But they represent "some valid point of view" that is culturally signficant for us. Notice how his particular construction defines the inquiry: the focus is on structures of authority, not as in Skocpol of Moore on forms of agriculture or peasant rebellion. Accordingly, considerable emphasis is put on Christianity, Hinduism, Confucianism in his account. Moreover, as Tilly remarks, "ordinary people disappear from Bendix's history, except as a breeding ground for new elites and as a field in which those new elites sow their implicitly revolutionary ideas." Indeed, "the privatal events are not alterations in the structure of production or of power, but changes in prevailing ideas, beliefs, and justifications"(Tilly, 1984: 93).

Encompassing Comparison

All historical sociologists look at big structures and big processes and most make big comparisons, but not all of them insist on thinking of world-history as a whole and not all of them argue that one can only understand the "parts" by understanding their relation to the whole. While it is possible for such holists (or globalists) to be generalizers, realists or interpretativists, it is worthwhile to have a separate category for this orientation. Two contrasting examples will serve.

Immanuel's Wallerstein's *Modern World System* (1974) provoked a powerful shift away from most conventional sociological thinking. Most sociologists have taken the idea of "society" quite uncritically, thinking of it in terms of a "nation" or "nation-state." They then proceeded to talk about such units as if they were wholly isolated. Wallerstein went in exactly the opposite way. He wrote:

> ...I abandoned the idea altogether of taking either the sovereign state or that vaguer concept, the national society, as the unit of analysis. I decided that neither was a social system and that one could only speak of change change in social systems. The only social system in this scheme was the world-system (1974: 7).

Like Parsonians (and those influenced by General Systems Theory), the idea of a social system is critical for Wallerstein. He theorizes the world-system as a set of inter-related structures which define a global division of labor and a global system of exchange. But unlike Parsonians, not only was his unit of analysis the world-system, but he gave this a deeply historical understanding. He was committed to the task of explaining the origins of the "world system" in the European sixteenth century and he insisted that what happened before had profound effects on what happened later. For example, Western European industrialization had profound effects on the later efforts at industrialization, for example, in Africa or South America.

The world-systems approach has been criticized along a number of lines. Many have argued that Wallerstein went too far in his rejection of the nation-state as a unit of analysis. For these critics, it is one thing to acknowledge that there are global influences on events and processes in local units; another to deny that there are relatively independent structures and processes which define local societies and state-formations. Others have argued that his systems approach (like the approach of Parsonians) effectively omits agency and, ultimately, is an extreme form of determinism, even a fatalism. Others have said that the theory is one-sided and reductionistic, omitting too many critical aspects of societies, but especially culture. Finally, others have quarreled about the character of his Marxist theory. This last is conveniently discussed in comparison to our final example, Eric Wolf's *Europe and the People Without History* (1982).

Wolf agrees with World-Systems theorists that we need to understand the parts in terms of the whole. He writes:

> The central assertion of this book is that world of humankind constitutes a manifold, a totality of interconnected processes, and inquiries that disassemble this totality into bits and then fail to reassemble it falsify reality. Concepts like 'nation,' 'society,' and 'culture' name bits and threaten to turn names into things. Only by understanding these names as bundles of relationships, and by placed them back into the field from which they were abstracted, can we hope to avoid misleading inferences and increase our share of understanding (1982: 3).

Thus, he insists that "no one can understand Europe with a grasp of the role Africa played in its development and expansion." And as with Wallerstein, it was the expansion of capitalism which was the process which drove modernization. But Wolf separates himself sharply with Wallerstein as regards the understandings of capitalism. Wallerstein understands capitalism to be a system of exchange; Wolf understands it to be a system of production. The theoretical question is critical since a whole series of further differences result. For example, a consequence of Wallerstein's analysis is that subsistence farmers in (say) Africa or Central America are part of the capitalist world economy since via complicated routes of unequal exchange, they are "proletarians" producing profit for capitalists in London or New York. Wolf's "people without history," by contrast, are in no sense proletarians, since they do not stand in a wage relation to owners of the land. For Wolf, Wallerstein's mode of analysis obscures the critical differences in the way production is organized and surplus extracted, historically and globally. This preserves, versus Wallerstein, the idea that local societies maintain a unity and relative independence even while elements do stand in signficiant relations to other entities and thus, may be profoundly affected by decisions made in these other places. Moreover, Wolf tries to pay attention to agency. Thus after noting that we need to understand Africa's role in European development, he writes: "Leading participants in that growth were not only European merchants and beneficiaries of the slave trade but also its African organizers, agents, and victims."

Tilly, no Marxist himself, is on Wolf's side of the argument. For him (as for Moore), we need to see how "farmers" or "peasants" in pre-capitalist forms of agriculture are transformed into

agricultural proletarians and how resistence and rebellion as well as cooperation figured in this. On the other hand, Tilly finds Wolf's explanation of some of the pivotal transformations wanting. Indeed, holist orientations are constantly at risk in lapsing into a non-explanatory functionalism which omits efforts at concrete causal analysis.

Conclusion

There is something to be learned in all the important works in historical sociology, even perhaps functionalist accounts which fail to do much explaining. Although, plainly, I have not demonstrated this here, I have argued that if this is a main desiderata, then the only a realist orientation will suffice for the historical sociologist. This allows us to rid ourselves of false views of explanation in science, to find a critical place for agency in historical change and at the same time to recognize the high importance of an hermeneutic moment in all social scientific inquiry. It allows us to see in a clearer way why we value that work which remains an inspiration to current work in historical sociology.

Two final notes: First, since the present is specious, a good deal of explanatory sociological work dealing with contemporary issues and problems is "historical." Second, historical sociology is not the whole of sociology since the sociologist has other very critical tasks to perform. Primary among these is description, both qualitative and quantitative. On the present view, both are indispensable: Wanting accurate descriptions of social phenomena, there will be nothing to explain. On the other hand, considerable confusion has resulted from the failure to be clear about these two very different tasks. For reasons suggested in the foregoing, neither relations of quantified variables, nor qualitative descriptions, no matter how thick, can explain. Rather, it is just these which cry out for explanation (Manicas, 1989).

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