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Toward a Sociology
of the Event"**

William Sewell

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William H. Sewell, Jr.
University of Chicago

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Historical approaches have made remarkable strides in American sociology over the past two decades. Through most of the 1960s, sociology in the United States was utterly dominated by research on contemporary America. But the civil rights and antiwar movements made graduate students trained in the 1960s and early 1970s far more interested than their elders in questions of conflict, revolution, and social change. Rather than seeking timeless laws of the operation of American society -- which was implicitly equated with society in general -- a new generation began to ask how the world's different societies have been transformed under the impact of capitalism and Western domination. The ideas pioneered by such intrepid historical explorers as Barrington Moore, Charles Tilly, and Immanuel Wallerstein were, consequently, taken up by scores of young sociologists in the 1970s and 1980s.¹

By the early 1980s, historical sociology was recognizable as a major node of growth in the profession. Its prominence has been institutionalized by the formation of two historically inclined official sections of the American Sociological Association: a Comparative Historical Sociology Section, which a sociologist friend of mine aptly characterizes as "left Weberian," and a Political Economy of the World System Section, which is predominantly Wallersteinian.² Although it is doubtful whether historical approaches will ever become dominant in the discipline, their growing prominence already has significantly changed the contours of American sociology.

The leading manifestoes and programmatic statements of historical sociologists have generally been concerned with methodological issues, and above all with comparative method.³ The title "Comparative Historical Sociology," adopted by the historical sociologists as the label for their ASA section, is emblematic; it places as much emphasis on comparative method as on historical subject matter. In this respect, historical sociologists reveal themselves to be right in the mainstream of American sociology. By stressing comparative method, they participate eagerly in the discipline's obsessive concern to justify itself as a science; comparative method, after all, is the standard alternative to mainstream statistical methods when the number of cases is insufficiently large. This mode of self-presentation has helped to make historical research acceptable to the rest of the profession. Historical sociology, this rhetoric implies, poses no particular theoretical or epistemological threat; it is simply the sociology of the past, carried on by means as close as feasible to the sociology of the present.

It is not hard to see why historical sociologists have been so self-consciousness about method; after all, they have virtually always had to make careers in departments where they

were surrounded by skeptical positivists vigilantly on guard against humanist tendencies. The emphasis on methodology has surely helped historical sociology to establish its secure beachhead in the profession. But it has also served to obscure some of the potentially radical implications of sociology's "historic turn." I believe that historical sociology is now sufficiently secure to risk examining some of these implications in public. In this spirit, shall I attempt to spell out what I see as deep but as yet largely unvoiced challenges that historical sociology poses to the disciplinary mainstream.

Until recently, few historical sociologists have had much to say about what makes their sociology historical. If historical sociology is merely the sociology of the past, it is valuable above all because it increases the available number of data-points. Many social processes require a significant period of time to work themselves out; if we investigate such processes only in the present, we not only risk studying incomplete sequences, but greatly restrict the number of cases. But is history just a matter of more data-points? Doesn't making sociology historical imply introducing ideas of temporality that are radically foreign to normal sociological thinking?⁴ I maintain that the answer to this question depends on the how temporality is conceptualized. The currently dominant modes of conceptualizing temporality in historical sociology -- what I will call "teleological" and "experimental" temporality -- minimize the challenge to mainstream sociology. But a third, currently subordinate, conceptualization -- what I will call "evenemential" temporality -- is potentially much more subversive.⁵ I shall argue that the dominant teleological and experimental concepts of temporality are seriously deficient, indeed actually fallacious, and that historical sociology needs to adopt the much more subversive evenemential notion of temporality -- which sees the course of history as determined by a succession of largely contingent events.

Teleological Temporality

Sociology was born under the sign of teleology. The great nineteenth-century founders, for example, Comte, Marx, Durkheim, and Tonnies, saw history as the temporal working out of an inherent logic of social development -- from religious to metaphysical to scientific eras, or from feudalism to capitalism to socialism, or from mechanical to organic solidarity, or from *Gemeinschaft* to *Gesellschaft*. For these social theorists, history was shaped by transhistorical progressive laws. The direction and meaning of history were a consequence not of the largely contingent events that made up the surface of history, but of long-term, anonymous causal forces, of which particular historical events were at best manifestations. The waning of the nineteenth century's virtually universal faith in progress has gradually resulted in an abandonment of explicit teleology in sociological thought. But weaker forms of teleology are still very much with us.

A teleological explanation is the attribution of the cause of an historical happening neither to the actions and reactions that constitute the happening, nor to concrete and specifiable conditions that shape or constrain the actions and reactions, but rather to abstract transhistorical processes leading to some future historical state. Events in some historical present, in other words, are actually explained by events in the future. Such explanatory strategies, however fallacious, are surprisingly common in recent sociological writing and are far from rare in the works of social historians. They are implied, for example, by the common practice of labeling political or social movements as backward-looking and forward-looking. "Backward-looking" movements, in normal usage, are those that value some aspect of a given period's life and culture that the analyst, with her or his twenty-twenty hindsight, regards as doomed to the dust-bin of history, whereas "forward-looking" movements are those valuing aspects of a period's life and culture that turned out to have a bright future. The simple act of labelling movements in this way contains an implicit teleological explanation of their histories.⁶ Likewise the term "modern" often serves as a label for those processes or agents that are deemed by the analyst to be doing the work of the future in some present, while "traditional" labels those equally current forces in the present that the analyst regards as doing the work of the past. The entire modernization school of social science is based on such a teleological conceptualization of temporality. But the teleological fallacy is also widespread in the work of many historical sociologists who regard their work as arising out of an uncompromising critique of modernization theory. I shall try to demonstrate this claim by briefly examining the work of two historical sociologists who were particularly influential in overthrowing the theoretical approaches of the modernization school and replacing them with those of contemporary historical sociology: Immanuel Wallerstein and Charles Tilly.

Immanuel Wallerstein as Social Astronomer

Wallerstein is by far the clearer case. In his multi-volume history of the modern world-system, Wallerstein proves himself no less anxious than other sociologists to find a secure scientific warrant for his knowledge. But because his object of study is vast and singular -- the capitalist "world-system" -- the usual quantitative and experimental scientific models are hardly appropriate; there are no other units with which the modern world-system could appropriately be compared. Wallerstein discussed this dilemma in the introduction to his first volume. He worried out loud that there was "only one instance" of his "unit of analysis" and that if this were true he might be reduced to merely writing its history. He "was not interested in writing its history;" he wanted to discover its laws.

But can there be laws about the unique? In a rigorous sense, there of course cannot be. A statement of causality or probability is made in terms of a series of like

phenomena or like instances....There had only been one "modern world." Maybe one day there would be discovered to be comparable phenomena on other planets, or additional modern world-systems on this one. But here and now, the reality was clear -- only one.

Wallerstein rescued the scientific status of his enterprise by an inspired analogy. If the capitalist world-system is one of a kind, Wallerstein reasoned, its study can be modeled on a fully recognized and powerful natural science that investigates the unique development a singular system: astronomy, or, more precisely, cosmology, that branch of astronomy that studies the physical universe as a whole. "What," Wallerstein asked himself, "do astronomers do?"

As I understand it, the logic of their arguments involves two separate operations. They use the laws derived from the study of smaller physical entities, the laws of physics, and argue that (with perhaps certain specified exceptions) these laws hold by analogy for the system as a whole. Second, they argue a posteriori. If the whole system is to have a given state at time y, it most probably had a given state at time x.⁷

This analogy with cosmology, I would argue, crucially shaped Wallerstein's intellectual project, although it did so in part by creative misapplication. It suggested a close relationship between part and whole, where laws found in investigations of local phenomena are also assumed to operate at the level of the whole. This key assumption -- that local and global processes partake of the same causalities -- was profoundly enabling. It provided Wallerstein with a powerfully unified perspective, one that authorized him to see all sorts of local events in various times and places as determined not by the accidents of local conditions, but by the dynamics of the world economy of which they were a part. I believe that it was above all this unique perspective -- this ability to see the dynamic of the whole at work in the dynamics of the parts -- that established Wallerstein's reputation as a great historical sociologist and that attracted an entire school of followers.

But in applying the astronomers' assumption about the uniformity of causalities to the world system, Wallerstein essentially reversed the direction of the part-whole relationship. In astronomy the physical principles discovered in studies of small-scale earth-bound matter -- whether these be Galileo's laws of falling bodies or the findings of contemporary particle physics -- are assumed to apply equally to cosmic matter -- to the orbits of planets, or to nuclear reactions in stellar cores. Indeed, such laws are assumed to operate at the level of the universe as a whole: the great cosmological question of whether the universe will expand indefinitely or eventually collapse in on itself hinges on calculations of the aggregate gravitational pull of the entire mass of matter in the cosmos. Astronomy today, as at the time of Newton, remains an example of

reductionist science at its most awesomely successful. In contemporary astronomy, the key to the dynamics of the infinitely large is found in the dynamics of the infinitely small.

But Wallerstein rejects models, whether derived from behaviorist psychology or microeconomics, that would explain the dynamics of the world system by the principles governing its smallest entities -- human individuals. Nor does he argue that the dynamics of local communities provide the key to the understanding the development of world society. His point is precisely the opposite: that the fates of local societies are determined not by local causes but by the operation of global, system-level, causes. The key to understanding the history of Poland or Peru is to recognize their place in the world-systemic division of labor -- as peripheral societies dependent on the core. But once we have begun to explain spatially and temporally localized events as a consequence of their place in a totality of world evolution, we are perilously close to teleological explanation.

Wallerstein's misapplied astronomical analogy also encouraged teleology in another, more direct, way. He felt authorized by astronomy to argue a posteriori, to argue back from the recent or current state of the capitalist world-system to its prior state. Most spectacularly, the astronomers project the current velocities of galaxies backward to argue for the existence of a "big bang" at the beginning of time, a primal cosmic event that determined the subsequent character and evolution of the physical universe. Wallerstein the social astronomer devises what in effect is a "big bang" theory of the origins of capitalism. A European economy already in crisis as a consequence of the disintegration of feudalism was decisively launched on a new and inexorable dynamic by the European geographical expansion known as "the great discoveries." The discoveries, according to Wallerstein, established the key economic, geographical, and political relationships on which the subsequent development of capitalism has been predicated -- a spatially differentiated world economy too large to be controlled by any of the competing political units of which it was composed.

Once again, Wallerstein's misapplication of the analogy with astronomy has served him both well and badly. I am convinced that the particular economic-geographical-political dynamic identified by Wallerstein is indeed crucial in the development of world capitalism, and that it was decisively set in motion by the discoveries. But Wallerstein's vision of all the subsequent development of capitalism as somehow inherent in his initial big bang warps his understanding both of the discoveries and of subsequent developments. His work contains some astute evenemential analysis of the political and economic history of Europe in his period, although his rhetoric suppresses the narrative's evenemential qualities. His discussions of how marginal and tiny Portugal became the initiator of the voyages of discovery, of how the Hapsburgs attempted but failed to gain a

political hegemony that would encompass the world economy, or of how the Dutch Revolt made possible the development of crucial new commercial and financial institutions in the Netherlands are actually full of contingency, unanticipated consequences, and fateful choices. But in Wallerstein's analysis, the contingencies, choices, and consequences are foreordained by the necessity built into the world-system from the moment of its creation. Hidden behind Wallerstein's big bang theory is a far more interesting account of how the crucial but open-ended event of the discoveries initiated a long chain of subsequent open-ended events -- that eventually and far from inevitably led to the emergence of a capitalist world economy.

It was the misapplication of Wallerstein's analogy that led him down the path to teleology. What makes the astronomers' a posteriori reasoning scientifically acceptable is the plausibility of the assumption that just as the laws of physics hold true across space they also hold true across time. If the laws of motion, gravity, and high-energy physics can be projected backwards in time, then it is possible to deduce the timing and characteristics of the big bang that propelled the universe into its current dynamics, or the state of the universe ten minutes after the big bang or a hundred billion years from now. But we know that human beings and the societies they create are far more perverse than physical matter. Humans, unlike planets, galaxies, or sub-atomic particles, are capable of assessing the structures in which they exist and of acting -- with imperfectly predictable consequences -- in ways that change them. While there certainly are turning points or crucial events in human history, there can not be big bangs. To construct historical arguments on an analogy with astronomy results in a teleology in which some crucial past event is misconstrued as a pure origin that contains the entire future of the social system in potentia, and in which the partially contingent events that occur subsequently are robbed of their effectivity and reduced to the status of markers on the road to the inevitable future.

Charles Tilly and the Master Processes of History

Teleology plays a far less obvious role in Tilly's work than in Wallerstein's. Nevertheless, I shall try to demonstrate that two of his most influential contributions -- his book on the Vendee rebellion and his work on the history of French collective violence -- contain strong doses of teleological temporality.⁸ This might seem particularly curious in the case of the The Vendee, which focuses on a particular event, the great counter-revolutionary revolt that erupted in western France in 1793. But Tilly's book is not a narrative history of the revolt; in fact his argument is introduced by a very effective polemic against the sociological naivete of the countless existing narrative histories.⁹ Whereas these narrative histories spoke about the cause of the revolt by rather cavalierly invoking the presumed motives of the rebels, Tilly insisted on asking properly sociological causal questions. He wanted to know what it was

about the social organization of the Vendee region that led to a revolt there. Tilly's principle analytical device was to compare two adjacent areas in western France, the Val-Saumurois, which supported the revolution, and the Mauges, which supported the counter-revolution. The principal sociological concept he used to analyze the difference in the social organization of these two areas was "urbanization" -- which in Tilly's somewhat expanded usage was "a collective term for a set of changes which generally occur with the appearance and expansion of large-scale coordinated activities in a society."¹⁰ Urbanization, hence, implied not just the growth of cities, but an "increased involvement of the members of rural communities in sets of activities, norms, and social relationships that reach beyond the limits of their own localities."¹¹

Tilly argued that the crucial difference between the Mauges and the Val-Saumurois was the extent and the recentness of their urbanization. The Val-Saumurois was "thoroughly and evenly urbanized;" even its rural inhabitants had long lived in sizeable agglomerated villages and sold their produce in regional and national markets.¹² This thorough and even urbanization made the Val-Saumurois well adapted to the more rational and centralized bourgeois regime introduced by the Revolution. The Mauges, by contrast, was much less urbanized, but it had experienced very rapid urbanization -- especially in the form of rural textile manufacturing -- in recent decades. This recent but incomplete urbanization made the social organization of the Mauges far less uniform and led to intense confrontations when the Revolution shifted power to the urban bourgeoisie and its agents in the countryside.

Tilly's analysis of how the different forms of social organization of these two regions led to different political and social experiences in the revolution is superb. But his sociological interpretation of these differences is marred by a gratuitous introduction of teleological temporality. For Tilly, the Mauge and the Val-Saumurois represent different points on a single developmental continuum from less to more urbanized. His procedure, as he puts it, is one "of comparing communities at roughly the same point in time as if they were at different stages in a progression from a common origin."¹³ The obvious advantage of this procedure is its generality; it means that differences found between two regions are not just a local peculiarity, but are comparable to differences in level of urbanization in other places and times. Introducing a single continuum makes it possible to envisage this local study as one contribution to a general scientific sociological account of the effects of urbanization on politics.

The problem is that the difference between the social organization of the Val-Saumurois and that of the Mauges demonstrably is not a matter of different stages in a single master process. The contrasting forms of social organization that Tilly attributes to differences in a progressive development

-- large nucleated villages surrounded by open fields in the Val-Saumurois as against more isolated small villages and hamlets scattered over hedged fields in the Mauges -- were actually constant and virtually unchanging features of the rural environment. The line dividing the Val-Saumurois from the Mauges was an ancient territorial division between what Marc Bloch characterized as distinct "agrarian civilizations" whose characteristics were already in place by the early Middle Ages.¹⁴ Tilly, in short, committed the fallacy of transmuting a fixed socio-geographical difference in social organization into putative stages in the linear development of the abstract master process of urbanization.

Tilly's use of urbanization as a linear teleology did not actually spoil his comparative study of the political effects of regional social structures, but it did misrepresent the book's contribution -- by casting its subject as a local instance of a universal social process. By doing so, it left unvoiced what I regard as the book's most original accomplishment: its acute analysis of how variations in local social structures made possible a smooth transition from old regime to revolutionary government in the Val-Saumurois, but enabled the French Revolution to reconfigure and give new meaning to existing social networks and social cleavages in the Mauges, touching off an escalating and unpredictable chain of confrontational events that culminated in a massive and durable shift in collective identities. It hid a masterwork of evenemential sociology behind a veil of misconstrued universalizing science.

One might object that The Vendee was Tilly's first book and that his mature work avoids these youthful errors. After all, he subsequently abandoned his overly abstract concept of urbanization, breaking it down into the two more specific notions of state centralization and capitalist development. But in his long and evolving project on French collective violence, which he took up after finishing The Vendee, he essentially retained that book's teleological fascination with underlying master processes, while abandoning its superb but insufficiently voiced evenemential analysis. Charmed by his own universalizing rhetoric, he pursued the notion that acts of political contestation arise from gradual evolutionary changes in large and anonymous social processes -- rather than the alternative theme that changes in political regimes reconfigure and give new meaning to existing social networks and cleavages, thereby creating new collective identities.

Tilly argues in his various books and articles on collective violence that the change in forms of violence over the past three centuries -- in brief, a change from "reactive," backward-looking, locally oriented to "proactive," forward-looking, and nationally oriented violence -- was the consequence of the gradual and inexorable rise of state centralization and capitalism.¹⁵ Such an argument is not necessarily teleological. Teleology is not implied, for example, when Tilly argues that

change in the targets and goals of violent protest arise in part from the particular and changing nature of the state presence in localities. But the argument frequently takes on a teleological quality, largely because the asserted causes -- capitalist development and state centralization -- occur off stage, outside of Tilly's texts, where they are essentially assumed as ever-present and ever-rising forces, a kind of eternal yeast.¹⁶ The violent incidents that Tilly describes in great number thus figure only as consequences of invisible causes; they are not events in the full sense because they are only effects, never causes, of change. A particularly clear indication that Tilly has abandoned evenemential analysis in his more recent work is his denial that even the historians' mega-event, the French Revolution, significantly transformed the nature of collective violence: in his account, it merely caused a certain acceleration of already existing trends.¹⁷ Thus, in spite of the inspired evenemential analysis contained in The Vendee, the dominant rhetoric of Tilly's work has not broken with a teleological conceptualization of temporality.

Theda Skocpol and Experimental Temporality

If Wallerstein and Tilly exemplify the continuing grip of teleological temporality in historical sociology, Theda Skocpol is the leading prophet and exemplar of experimental temporality. In States and Social Revolutions, Skocpol explicitly embraces the standard "scientific" methodology of mainstream American sociology, extending it to historical studies. "Comparative historical analysis," she asserts,

is distinctively appropriate for developing explanations of macro-historical phenomena of which there are inherently only a few cases. This is in contrast to more plentiful and manipulable kinds of phenomena suitable for experimental investigations, and in contrast to other phenomena where there are the large numbers of cases required for statistical analyses. Comparative historical analysis is, in fact, the mode of multivariate analysis to which one resorts when there are too many variables and not enough cases.¹⁸

Skocpol applies her comparative method to the three great social revolutions of modern times -- the French, the Russian, and the Chinese. Her analysis attempts to set up comparative "natural experiments" capable of sorting out the causal factors that explain the occurrence of social revolutions. She explores the histories of the French, Russian, and Chinese revolutions, but also of two major political crises that never became revolutions (the Prussian Reform Movement of 1807-1814 and the Japanese Meji Restoration of 1868-73) and of two political revolutions that did not become social revolutions (the English Civil War and Glorious Revolution of 1640-1688 and the German Revolution of 1848-50). On the basis of her comparative investigation of these seven cases, Skocpol develops a powerful and sophisticated theory of the causes of social revolutions.

She begins by noting that the pre-revolutionary French, Russian, and Chinese states had all fallen behind their rivals in military competition. This gave rise to attempts to catch up by instituting far reaching administrative, economic, and fiscal reforms. But these reforms were resisted in all three states by the dominant landlord class -- which had a firm enough foothold in the state to block, slow, or subvert the reforms. The consequence was a deep fiscal and political crisis that was broken only by a revolution. These considerations led Skocpol to posit two conditions for social revolution: the existence of a state that fell behind rival states in military competition and a dominant class of landlords who were sufficiently powerful to block state-initiated reforms. She strengthens this specification of causes by examining the Prussian and Japanese cases. In both cases the states were driven into crisis by failure in military competition, but because the Prussian and Japanese dominant classes had little political leverage against the state, reformers from within the state apparatus managed to revitalize the states without the intervention of political revolution.

But Skocpol also finds another necessary condition for the occurrence of social revolutions. Fiscal crises based on military backwardness and exacerbated by the resistance of recalcitrant landlords may have been enough to touch off political revolutions, but for these to become social revolutions -- that is, to bring about a transformation of the country's class structure -- something more was required: a massive uprising of the peasant class. Social revolutions, therefore, also required the existence of well-organized and autonomous peasant communities capable of taking advantage of the breakdown of state authority in a political revolution. Once again, Skocpol shows how this condition was present in her three cases but absent in the seventeenth-century English and mid-nineteenth-century German revolutions, which never advanced from political to social revolutions. In short, Skocpol uses a quasi-experimental, inductive method to discover the three factors necessary for social revolutions: (1) military backwardness, (2) politically powerful landlord classes, and (3) autonomous peasant communities. As I understand the argument, these factors are conceptualized as sequenced: factor one induces a political crisis; the addition of factor two turns the political crisis into a political revolution; and the further addition of factor three turns the political revolution into a social revolution.

The explanation Skocpol develops by considering these seven cases is extremely powerful and, quibbling aside, quite convincing. But the power of her explanation cannot derive, as Skocpol claims, from her application of quasi-experimental inductive method. As Michael Burawoy has pointed out in a recent article, a careful examination of Skocpol's logic and evidence indicates that her explanation is by no means proven by the "natural experiments" carried out in her book.¹⁹ In fact, her evidence fails in more than one way. First, as Burawoy remarks,

the seventeenth-century English and mid-nineteenth-century German cases actually seem to refute the first stage of her causal theory, since they were genuine political revolutions that were not provoked by military failures: the English revolution of 1640 certainly arose out of a fiscal crisis, but not a fiscal crisis that was provoked by military reverses, and the German revolution of March 1848 was provoked by neither military reverses nor fiscal crisis. Skocpol's comparisons actually demonstrate that far-reaching political revolutions may arise in the absence of both of her first two factors.²⁰

Second, the array of cases compared by Skocpol does not demonstrate that the full sequence of three factors is necessary to produce a social revolution. To do so, Skocpol would have had to find a case in which military failure and landlord resistance led to political revolution, but in which a social revolution failed to develop due to the absence of autonomous peasant communities. But she has no such case: the Meiji Restoration and the Prussian Reform Movement had only the first factor, the English Revolution only the second, and the German Revolution of 1848 none of the three. On the basis of Skocpol's evidence, it remains logically possible that a combination of military backwardness and a powerful landlord class was by itself sufficient to produce a social revolution.

Indeed, Skocpol's analysis of the Chinese Revolution could actually be read as supporting this proposition. There a social revolution took place in a country where only the first two of Skocpol's conditions were initially present. Skocpol treats the case as confirming her theory. As she tells the story, the peasantry's lack of autonomy from landlords long prevented the political revolution initiated in 1911 from becoming a social revolution. It was only after 1940 that the Chinese Communists organized an autonomous peasantry in the districts they controlled, thereby creating the agrarian striking force necessary to carry through a social revolution.²¹ But this argument is actually highly ambiguous. One could just as easily say that the long standoff between state and landlords, exacerbated by the Japanese invasion in 1935, created the conditions for a successful social revolution in the areas which the Koumintang could no longer control. The creation of peasant communities autonomous from landlords was, in this telling, less a precondition for social revolution than a consequence of a locally successful Communist-led social revolution touched off by a collapse of the stalemated state in the wake of military failure.

These two flaws in Skocpol's argument invalidate her claim to have confirmed empirically her theory of social revolutions. She has not shown either that political revolutions are explained by a combination of military reverses and effective landlord resistance to reforms or that autonomous peasant communities are necessary for a political revolution to be transformed into a social revolution. It is true that these flaws are not intrinsic

to the comparative method per se; Skocpol is working on such a rare phenomenon that she has been unable to amass an array of cases sufficient to test out all the logical possibilities inherent in her theory. It is, perhaps, embarrassing that she jumped to conclusions unwarranted by a strict evaluation of the comparative evidence, but it is hardly fatal for the method she espouses. A historical sociologist working on a somewhat more common phenomenon could surely devise more adequate empirical tests.²² Of course, limiting ourselves to more tractable phenomena would save the comparative method only at a very high cost, inasmuch as it would restrict sociology's ability to say anything valid about rare but world-shaping events like social revolutions. At best, the evidence and arguments presented in States and Social Revolutions hardly justifies Skocpol's confidence that comparative historical analysis is a panacea for sociologists working on problems where "there are too many variables and not enough cases."²³

Nor do Skocpol's logical difficulties end here. The most troubling flaws of quasi-experimental comparative method come not from the difficulty of amassing enough cases, which affects only some research problems, but from the unhistorical assumptions about temporality that strict adherence to experimental logic requires. The experimental conception of temporality, I shall argue, is inseparable from conventional comparative method, and it can be imposed only by what Burawoy aptly dubs "freezing history" -- and, I would extend the metaphor, by fracturing the congealed block of historical time into artificially interchangeable units.

In order for Skocpol's revolutions to be subjected to her comparative method, they must be conceptualized as analogous to separate "trials" of an experiment. This means that the trials must be both equivalent and independent. The principle of equivalence implies that each new trial (in this case, new revolution) must be a genuine replication of earlier trials, with all relevant variables held constant. This implies definite assumptions about temporality. The relevant temporality in experimental logic is purely internal to the trial: the posited causal factors must exist prior to their posited consequence. By contrast, the external temporality of historical time -- whether one trial precedes or follows another and by how much -- must, by definition, be considered irrelevant in order to meet the requirement that experimental trials be strictly equivalent.

This requirement that trials be equivalent poses considerable difficulty for Skocpol's arguments about the causes of social revolutions. In order to use inductive comparison, Skocpol must assume that her three great social revolutions are in fact a uniform class of objects governed by identical causal laws. But this is a highly dubious assumption, in part because new classes and new class relations arise over time. This, in turn, might well alter the conditions necessary and sufficient for social revolution. To take a pertinent example, the

industrial revolution intervened between the French Revolution and the Russian Revolution, giving rise to a new industrial proletariat. One might consequently assert, with some plausibility, that the revolt of the Petersburg and Moscow proletariat was a necessary condition for social revolution in Russia in 1917, even if it was not a condition in France in 1789. But, as Burawoy points out, Skocpol cannot consider this possibility without breaking the crucial assumption of equivalence between the revolutions. Thus, instead of examining empirical evidence about the role of the industrial proletariat in Russia, she dismisses the possibility out of hand on the grounds that because a proletarian revolt was not necessary in her other two cases it cannot have been in Russia either.²⁴ The assumption of equivalence, which is a logical foundation of Skocpol's comparative method, does not allow her to pursue questions about how events intervening between revolutions might affect their occurrence and outcome.²⁵

The second fundamental assumption of experimental logic, that experimental trials must be independent of one another, also poses serious problems for Skocpol's analysis. For trials to be independent, the outcome of any given trial must have no effect on the outcome of a subsequent trial.²⁶ But it is absurd to assume that earlier revolutions had no influence on later revolutions. After all, the leadership of the Bolshevik party self-consciously patterned its own revolutionary efforts on what it regarded as the lessons of the French Revolution, and the Chinese Communists not only modelled themselves explicitly on the Bolsheviks but received direct aid from them. Once again, this assumption can only be sustained by "freezing" and "fracturing" history, by treating the histories of the three revolutions as if they took place in isolation from one another rather than as a sequence of historically connected events. In short, Skocpol's comparisons are fundamentally logically deficient if viewed from the perspective of experimental method. They fail both the requirement of equivalence and the requirement of independence.

Although it may occasionally be possible to identify a universe of historical objects that simultaneously satisfies the assumptions of equivalence and independence, such occasions are likely to be unusual. With rare exceptions, attempts to assure equivalence in historical cases will actually result in decreasing the independence between cases -- and vice versa. The obvious way to assure independence is to compare phenomena that are widely separated in space and time; one can be reasonably sure, for example, that similarities between royal ceremonies in eighteenth-century Japan and ancient Mesopotamia cannot be accounted for by imitation. But the very remoteness that assures independence makes the assumption of equivalence impossible to sustain: in societies with radically different economies, systems of social stratification, religious beliefs, conceptions of gender, and so on, how could we ever be confident that the relevant differences have been controlled for?

It was for precisely this reason that Marc Bloch, in his seminal article on comparative history, cautioned against studying "societies so widely separated in time and space that any analogies observed between them...can obviously not be explained either by mutual influence or by a common origin."²⁷ Bloch believed that such comparisons were too imprecise and therefore opted for

a parallel study of societies that are at once neighboring and contemporary, exercising a constant mutual influence, exposed throughout their development to the action of the same broad causes, and owing their existence in part to a common origin.

Bloch preferred comparisons of neighboring societies essentially on the grounds that in such societies the assumption of equivalence could reasonably be approximated.²⁸ I suspect that Bloch, as usual, made the wiser choice. But his choice obviously moves comparative history farther from the no less logically necessary assumption of independence between the cases; because the societies Bloch studied exercised a "constant mutual influence" on one another, it is in principle impossible to determine whether a similar outcome in two cases resulted from a posited set of causal factors or from the play of influence. With rare exceptions, equivalence between historical cases is bought only at the price of decreasing independence. This paradox, I submit, makes history a singularly unpromising territory for the kind of rigorous experimental induction that Skocpol advocates, but cannot really practice, in States and Social Revolutions.

It is remarkable, in view of the logical and empirical failure of Skocpol's program of experimental induction, that her analysis of social revolutions remains so powerful and convincing. This implies that, as was true of both Wallerstein and Tilly as well, something important and valuable is accomplished in the book that remains unvoiced in its explicit theoretical and methodological statements.²⁹ I would contend that much of this unvoiced work occurs in her handling of events. The bulk of her book is composed not of a rigorous weighing of comparative evidence, but of carefully constructed causal narratives specifying how social revolutions are brought about in her three cases. Skocpol's best statement of her narrative strategy is, symptomatically, tucked away in a footnote, where she complains that "social-scientific analyses of revolutions almost never...give sufficient analytic weight to the conjunctural, unfolding interactions of originally separately determined processes."³⁰

Specifying the "conjunctural, unfolding interactions of originally separately determined processes," is the distinctive narrative strategy of her book. It is distinct not only from the usual strategy of sociologists, but from the usual strategy of historians as well. Sociological analyses of revolutions tend to emphasize the primacy of some single cause of revolutions, systematically subordinating other causes to the chosen

explanatory factor; historical analyses typically attempt to recount the course of a revolution in some semblance of its original complexity, discussing different causal features of the revolutionary process only as they make themselves felt in the unfolding of the story. The problem with the historical strategy is that crucial causal processes tend to get lost in a muddle of narrative detail and are never separated out enough to make their autonomous dynamics clear. The trouble with the sociological strategy is that although it successfully specifies the causal dynamics of one factor, it tends either to conflate other causal factors with the chosen cause (as Marxian treatments of revolution have often viewed the state as merely an expression of class power) or to treat them as mere background (as most studies of revolution have done with the international military setting). Skocpol's strategy is an inspired compromise. It appropriates the power of the sociological strategy, but applies it to not one but several distinct causal processes. Yet by emphasizing "conjunctural, unfolding interactions," it also appropriates the historical strategy's concern with events, sequence, and contingency. Quite apart from considerations of comparative experimental induction, Skocpol has elaborated in States and Social Revolutions an extremely effective strategy for what might be dubbed multiple causal narrative.³¹ She has, to put it otherwise, worked out a kind of incipient theory of revolutionary process, of how events, by straining or rearranging structures, open the door to further transformative events. But this significant rhetorical and theoretical innovation is never signaled in her introduction or conclusion, and is only formulated in passing in a footnote in the third chapter. Skocpol's misplaced obsession with quasi-experimental comparative method has virtually obscured her highly original contribution to evenemential sociology.

Skocpol's formal comparative method, with its experimental conception of temporality, makes little contribution to her innovations in evenemential sociology. Nevertheless, I believe that serious comparative thinking played a crucial role in developing her incipient theory of revolutionary process. The formal logic of comparative method has been developed exclusively as a means of assessing the empirical accuracy of theoretical propositions -- to deal with the phase of scientific research that Lacatos has termed the "context of justification."³² I suspect, however, that the most important role of comparison in States and Social Revolutions was actually in the "context of discovery" -- that phase of research concerned with generating theoretical ideas. Skocpol's own description of the history of her project suggests as much. She began, she tells us, with the history of the Chinese revolution, then found that the Chinese developments suggested unsuspected analogies with the French case, and finally used an analytic scheme worked out from the Chinese and French cases to interpret Russian history.³³ One suspects that this mutual reading of each of the cases in terms of the others continued and kept spiraling back: that, for example, Trotsky's emphasis on backward Russia's unsuccessful

military competition with the European powers must have suggested parallels in the crisis of the French old regime, or that Georges Lefebvre's analyses of the crucial contribution of aristocratic resistance and peasant revolts to the French Revolution must have thrown a sharp light on the roles of landlords and peasants in Russia and China.³⁴

I suspect that Skocpol formulated and deepened her interpretations of key revolutionary events by just such a process of critically extending causal narratives from each of the cases to each of the others. A rough causal logic certainly guided such analogical extensions: if attempts to reform the sprawling agrarian state of Imperial Russia arose in response to the threat of German military prowess, is it not likely that comparable attempts to reform a roughly similar French state might have arisen from repeated defeats at the hands of England? But Skocpol's presentation of comparative method as a means of testing already formulated general propositions gets it the wrong way around. It might be more accurate to say that comparison generated propositions whose potential generality was tested by their ability to illuminate the conjunctural unfolding of causal processes in each of the particular cases. Deep analogies about evenemential sequences, rather than rigorous experimental tests of abstract generalizations, may be the true payoff of Skocpol's comparative history.

Evenemential Temporality

The evenemential conception of temporality may be defined in contrast to the experimental and teleological conceptions. The experimental conception rests on two fundamental assumptions: a uniformity of causal laws across time and a causal independence of every sequence of occurrences from previous and subsequent occurrences. The evenemential conception of temporality denies both of these assumptions. Rather than assuming causal independence through time, it assumes that events are normally "path dependent," that is, that what has happened at an earlier point in time will affect the possible outcomes of a sequence of events occurring at a later point in time. However, path dependence does not necessarily imply that causal laws change over time. In fact, the notion of path dependence was initially formalized by economists who argued that different but equally rational choices among alternative technologies at one point in time would imply a divergence in choices at later times even under the standard economists' causal assumption that all actors at all points in time pursue their advantage rationally.

A fully evenemential conception of causality must also deny the assumption that causal laws are uniform through time. Events must be assumed to be capable of changing not only the balance of causal forces operating but the very logic by which consequences follow from occurrences or circumstances. A fully evenemential account of the fate of nobles in the French Revolution, for example, would have to argue that nobles lost power not only

because the loss of some of their assets -- land, tax privileges, feudal dues, offices -- reduced their resources relative to those of other classes, but also because the rules of the social and political game were radically redefined, making what had previously been a prime asset -- their noble status -- into a powerful liability by the time of the Terror. In this case, and I would argue in general, events bring about historical changes in part by transforming the very cultural categories that shape and constrain human action. Since the causalities that operate in social relations depend at least in part on the contents and relations of cultural categories, events have the power to transform social causality.³⁵

An evenemential concept of temporality, then, assumes a causal dependence of later occurrences on prior occurrences and assumes that social causality is temporally heterogeneous, not temporally uniform. Evenemential temporality therefore differs fundamentally from experimental temporality. It also differs from teleological temporality. Teleological and evenemential temporality share an assumption of path dependence, but teleological temporality denies temporal heterogeneity, or at least general temporal heterogeneity. (Stage theories, one of the subtypes of teleological theories, assume causal homogeneity within stages, but may accept radical heterogeneity between stages.) However, teleological and evenemential concepts of temporality differ most sharply on the question of contingency. Teleological temporality is compatible with a certain contingency at the surface of social relations, but it is incompatible with the assumption of radical contingency that I regard as fundamental to evenemential temporality. For example, a teleological Marxian account might argue that the particular situation in which a conflict between workers and bourgeois occurs may affect the outcome of their struggle and may even result in a local victory for a retrograde form of social organization -- say for artisanal over factory production. But no combination of such local victories can "turn back the clock" definitively. The built-in directionality of underlying causal forces guarantees that local variations are mere surface perturbations with no long-term effect on the course of history. By contrast, an evenemential concept of temporality assumes that contingency is global, that it characterizes not only the surface but the core or the depths of social relations. Contingent, unexpected, and inherently unpredictable events, this view assumes, can undo or alter the most apparently durable trends of history. This does not, of course, imply that human societies are in permanent and universal flux, that social change is easy to accomplish, or that historical changes display no regularities. I am not arguing that capitalism or the global division of labor or sexual inequality would go away if only we wished it, or that history is a tale told by an idiot. History displays both stubborn durabilities and sudden breaks, and even the most radical historical ruptures are interlaced with remarkable continuities. An assumption of global contingency

means not that everything is constantly changing, but that nothing in social life is ultimately immune to change.

The evenemential conception of temporality, then, assumes that social relations are characterized by path dependency, temporally heterogeneous causalities, and global contingency. This is close to the implicit intellectual baggage of most academic historians, and it would not be hard to find works by historians that incorporate an evenemential approach to temporality. Yet most historians take the effectivity of events so much for granted that their accounts of events tend to lack a theoretical edge. Marshall Sahlins, citing Ruth Benedict's aphorism that if deep sea fish could speak the last thing they would name is water, points out that historians "live in the narrative element" and consequently are remarkably unselfconscious about the event as a theoretical category.³⁶ By contrast, those few sociologists who argue for the significance of events tend to do so with particular clarity and force -- thanks both to their highly developed methodological consciousness and to their need to convince antievenemential sceptics in their profession. I will try to demonstrate the promise of evenemential sociology by analyzing works by two sociologists: Mark Traugott and Howard Kimeldorf.³⁷

Traugott's Organizational Hypothesis

Traugott's book may be characterized as a study of the differential effect of a great event, the French Revolution of 1848, on two groups of French workers: those enrolled in the government's unemployment relief organization, the National Workshops, who formed the core of the great workers' revolt of June 1848, and those recruited into the paramilitary Mobile Guard, who were instrumental in suppressing it. Traugott's task is to explain why workers associated with these two organizations wound up on opposite sides of the barricades. The leading explanation of their divergent political trajectories has been Marx's. Marx claimed that the Mobile Guard had no scruples about shooting down the proletarian insurrectionaries because it had been recruited exclusively from the rootless lumpen-proletariat, the scum of the urban poor. Traugott spends much of his book -- and doubtless spent even more of his research time -- subjecting this argument to a painstaking quantitative test, which shows that the pre-February occupations of the June rebels and the Guardsmen were in fact virtually identical.³⁸ The divergent political behavior of Guards and Workshop members cannot be explained by differences in their class backgrounds.

The failure of this standard sociological explanation led Traugott to what he calls the "organizational hypothesis" -- that the divergent actions of guardsmen and insurrectionaries were the result not of characteristics of their pre-1848 lives but of their collective experiences between February and June 1848.³⁹ Traugott tests this hypothesis by means of a "paired comparison" of the organizational histories of the Mobile Guard and the

National Workshops. There are many parallels between the two institutions: both were improvised in response to working-class pressures after the February Revolution, and both were intended by the Provisional Government simultaneously as a means of alleviating unemployment and of coopting potentially rebellious workers to moderate their political sentiments.⁴⁰ Moreover, both institutions were deeply mistrusted by the conservative bourgeoisie, which feared they would become hotbeds of socialist agitation. By means of his paired comparison, Traugott shows that both were initially highly successful cooptive institutions, and that the National Workshops became a nursery of rebellion only when they were organizationally decapitated by a hostile government.

Traugott's organizational analysis borrows from Katherine Chorley's Armies and the Art of Revolution.⁴¹ By April of 1848 the initially rag tag Mobile Guard had met Chorley's three crucial conditions for successful military repression of a revolutionary movement: a unified corps of officers, effective isolation from the civilian population, and prompt attention to practical grievances in the ranks. The Mobile Guards' officer corps was supplied by the regular army, supplemented by carefully managed elections from the ranks; by April it became a highly professional and unified body. The guardsmen were isolated from the civilian population because they were housed in barracks, usually not in their own neighborhoods. One practical grievance -- a long delay in the provision of uniforms -- seriously threatened to undermine the guardsmen's morale, but it was resolved well before June. Hence the potentially unruly Mobile Guard was moulded into a disciplined military force that was willing to face down fellow workers in several confrontations in April and May, and to shoot them down when the insurrection broke out in June.⁴²

Although the National Workshops were not a military force, Traugott uses Chorley's model in his analysis of their organizational history as well. The Workshops were actually organized on a military model -- with squads, brigades, and companies and a uniformed officer corps. Emile Thomas, the youthful director of the workshops, assured the unity of his officer corps by recruiting its upper echelons from students at a national engineering school of which he was an alumnus. Lower level officers were chosen by election from the ranks, but subjected to close administrative supervision. The unified officer corps not only enabled Thomas to maintain firm administrative control of the Workshops, but contributed to his personal popularity in the ranks. Thomas was less successful in his efforts to overcome practical grievances -- mainly because the Workshops were never supplied with enough meaningful work to occupy their full contingent of unemployed laborers (about 120,000 by June). Nor could members of the Workshops be fully separated from the general population -- they made up about a third of the working class of Paris and continued to live in their own neighborhoods. But Thomas did his best, insisting that

members report to their brigades every day, whether they had work or not, and posting them to peripheral locations when there were demonstrations or political troubles. He also established a National Workshops political club, attempting with considerable success to isolate workshop members from the political clubs of their neighborhoods.⁴³

This surprisingly effective isolation, together with the unity of the officer corps and Thomas' personal popularity, assured the Workshops' political moderation until nearly the end of May -- and might have done so indefinitely had the government not decided to sack Thomas and phase out the Workshops. This drove the elected squad and brigade leaders to the left, released them from the political and organizational tutelage of Thomas and his loyal school-mates, and transformed them from conduits of moderation to a revolutionary cadre.⁴⁴ When the full destruction of the Workshops was announced on June 21, they led their squads and brigades in armed revolt. In short, members of the National Workshops and the Mobile Guard took opposite sides in June not because of divergent class backgrounds but because of the divergent organizational histories of the two institutions. The "organizational hypothesis" best explains the observed behavior.

We should be clear, however, that Traugott's organizational hypothesis is not just another sociological hypothesis. He is advocating not merely that we consider another explanatory factor parallel to class background, income, religious preference, or cohort, but that we entertain a new and essentially evenemential form of explanatory argument. The organizational explanation of why the National Workshops rebelled and the Mobile Guard put down the rebellion is in fact a causal narrative of how these institutions were shaped through time, and it has a characteristically evenemential temporality. It incorporates path dependency: the timing of incidents crucially affects their consequences. The fact that the Mobile Guard's deeply felt grievance about lack of uniforms had been rectified well before June assured the Guard's loyalty to the government; had a revolt broken out before the resolution of this grievance it is uncertain whether the Guard would have followed orders to march against fellow workers. Traugott's temporality is also causally heterogeneous. Consider the role of the Workshops' squad and brigade leaders, who formed the leadership cadre of the insurrection. Their positions of leadership were produced by Emile Thomas' paternalist strategy of cultivating their personal and political loyalties and integrating them tightly into the Workshops' officer corps. This paternalism had the intended effects of assuring the moderation of the Workshop members as long as Thomas and his schoolmates ran organization. But when Thomas was sacked, the squad and brigade leaders were also in position to organize the rank and file's resistance to the closing of the Workshops, by armed revolt if necessary. Paradoxically, the organizational structure erected by Thomas to insure the workers' moderation had the effect of exacerbating the crisis when it came. In brief, the creation of this working

class organizational infrastructure changed the causal dynamics of the situation, greatly amplifying the extent, intensity, and effectiveness of resistance to the government's closing of the Workshops.

It should be clear that the temporality of Traugott's account is interlaced with contingency. Like classical narrative historians, Traugott emphasizes the importance of significant persons. The Workshops were organized as they were largely because of the personal decisions of Emile Thomas, and his removal from the directorship was a decisive cause of their radicalization. A forceful and magnetic person like Thomas, placed in a position of strategic importance, can have a remarkable effect on the course of history. Conscious choice also figures importantly in Traugott's account. Thomas purposefully drew his officer corps from his schoolmates so as to enhance its solidarity. The conservative government purposefully provoked a crisis by abolishing the Workshops. For all these reasons, the course of the events Traugott analyzes is contingent, not necessary. Had the government maintained the Workshops in existence and kept Thomas as their director, the insurrection might never have happened. Had someone less capable been chosen as director, the revolt would probably have been less effective, but it might well have happened considerably earlier.

Traugott's embrace of evenemential temporality does not mean that he has abandoned sociology for narrative history. He is driven to evenemential explanation by the austere logic of his sociological method, and he carefully specifies the structural limits within which timing, personality, choice, and contingency operate. Although he comes down in this case for evenemential rather than etiological explanations, he does so not to dismiss etiological factors, but to specify their mode of effectuation. Class, as he points out, may have an influence, but "any class-based propensities of actors are conditioned by a set of contingent organizational forces."⁴⁵ He is arguing not that history is a sequence of pure contingencies, but that "an intervening [I would add evenemential] level of analysis must demonstrate by what mechanisms macrosociological structures are converted into forms of consciousness and the probability of collective action."⁴⁶ Traugott's book, with its careful methodology and its focus on the relationship between structures and events, points the way toward an evenemential sociology that remains an evenemential sociology.

Kimeldorf's Multiple Registers of Causation

Kimeldorf's book is a study of the divergent political evolution of longshoremen's unions in New York City and on the West Coast from the 1930s through the 1950s. Kimeldorf, like Traugott, uses a strategy of paired comparison, combines narrative history with structural analysis, and provides powerful arguments for the significance of events in the shaping of workers' politics. Kimeldorf attempts to explain why East Coast

and West Coast dockworkers, who did similar work under similar technological and economic conditions, nevertheless formed sharply contrasting labor unions -- the durably socialist International Longshoremen's and Warehousemen's Union (ILWU) on the West Coast and the politically conservative and chronically racket-ridden International Longshoremen's Association (ILA) on the East Coast.

Kimeldorf's explanation of the difference between the New York and West Coast longshore unions is complex, multicausal, and irreducibly historical. He shows that the differences cannot be attributed to any single, underlying factor, and have historical roots that go back well before the 1930s. The explanatory factors are of several different types. First the recruitment and the cultures of the labor forces differed substantially. A high proportion of the West Coast longshoremen were recruited from the lumbering and seafaring industries, which because of their work structures very commonly give rise to high levels of worker solidarity and class conflict, and certainly did so on the American West Coast in the early twentieth century. This labor force, whose prior work experience already inclined it to labor militancy, was widely but only temporarily organized by the radical IWW in the teens and twenties. By the 1930s West Coast dockworkers already had been exposed to a radical work culture that made them ready to heed Harry Bridges' call.⁴⁷ These predisposing factors were entirely absent among the New York dockworkers, where the labor force was recruited from two successive peasant immigrant groups, the Irish and Italians, who, by the 1930s, had established ethnically divided enclaves on the docks. Moreover, the political and work culture was far more conservative -- dominated by the Catholic Church in the case of the Irish and by an exploitative (sometimes Mafia-ridden) padrone system in that of the Italians.⁴⁸

These differences in labor recruitment were reinforced by differences in the structure and consequently in the class capacities of shipping capitalists. On the West Coast three American companies dominated the carrying trade and were consequently able to carry on a particularly ferocious and ultimately radicalizing battle against longshore unions; in New York the carrying trade was divided between a larger number of American and European companies and a large government-owned line. This divided employer class was unable to sustain a unified front against longshore unions and the companies therefore negotiated separate deals with different union locals.⁴⁹ There were, hence, structural factors of demographic, cultural, and economic natures that made radical unionism more likely on the West Coast than in New York.

But while these structural factors might have been sufficient to rule out radical unionism on the New York docks, they were hardly sufficient to guarantee it on the West Coast. In a chapter entitled "The Strategic Pivot," Kimeldorf moves from relatively stable structural conditions to more contingent

conjunctural and volitional causes. The conjuncture, largely shared by the two coasts, was the depression and the new political climate and nationwide labor militancy it engendered. But the radical potential of this period was seized successfully on the West Coast and missed in New York largely because of the very different strategies of the two areas' Communist Parties. In San Francisco, which became the center of radical longshore unionism, the local Communists ignored the national party's strategy of supporting separate left-wing unions, opting to develop a leftist force within the existing moderate union instead. The consequence was that the Communists and allied left-wingers such as Bridges were in a position to assume leadership in the dramatic and violent strike of 1934 and move the rank and file definitively to the left.⁵⁰ By contrast, the New York party stuck obstinately to an utterly unsuccessful policy of separate left unionism and was thereby deprived of any chance to play a radicalizing role in the New York strike movements of the same year.⁵¹ Finally, the dramatic radicalization of the West Coast union was rendered enduring by the cultural codification of the "Big Strike" and "Bloody Thursday" (July 5, 1934), which was embodied in a highly self-conscious cohort of "34 men" who remained a solid block of support for the leftist leadership right through the 1950s, and whose prestige among the rank and file created a pervasively leftist and militant work culture on the docks.⁵²

Kimeldorf's book, as I read it, provides a largely implicit but potentially generalizable model of explanation in historical sociology. The model not only specifies multiple causes, but sorts out what might be characterized as different registers of causation: preexisting structural conditions (cultural, social, and economic), conjunctural conditions (such as the generalized labor militancy of the 1930s), and contingent strategic or volitional actions -- which in turn may reconfigure preexisting structural conditions and create new structural conditions (by, for example, forming the solid block of influential '34 men who maintain the leftism of the ILWU.) Particularly interesting is Kimeldorf's discussion of how the Big Strike and the '34 men attained mythic stature -- through, among other things, annual work stoppages on July 5 to commemorate the union's formative battle.⁵³ As Kimeldorf's account shows, the Big Strike not only was an objectively important event in the formation of a radical union, but was also constructed as a subjectively important event by the radical union in subsequent months and years. This example demonstrates that the question of how events are retrospectively appropriated to reproduce structures is just as important for historical sociology as the question of how conjunctures and strategic action make transformative events possible in the first place. Events, Kimeldorf demonstrates, are constituted as well as constitutive.

An Evenemential Sociology?

The work of Traugott and Kimeldorf points toward an historical sociology in the evenemential mode. Such an evenemential sociology would recognize the path dependent, causally heterogeneous, and contingent nature of temporality, and would put the question of how structures are transformed or reconfigured by social action at the center of its inquiries. As my analysis of Wallerstein, Tilly, and Skocpol is intended to show, adopting an evenemential approach would not require jettisoning the work of those who have employed teleological or experimental conceptions of teleology. Rather than calling for a return to the drawing boards, I am arguing that we need to rethink the classical teleological and experimental studies, seek out the valuable evenemential analyses that have been masked by misconstrued scientific rhetoric, and rehabilitate such works as The Modern World System, The Vendee, and States and Social Revolutions as rhetorically flawed masterworks of evenemential sociology. Here we might well find assistance in surprising quarters. Charles Tilly has recently published an article in which his central case for historical sociology rests on one of the central claims of the evenemential view of temporality: that "social processes are path dependent."⁵⁴ And Immanuel Wallerstein has recently written an extremely radical and opened paper in calling for an "unthinking of the nineteenth century," in other words of the deeply held assumptions about social process that we have inherited from the nineteenth century. Although he does not include teleology as one of the assumptions that needs to be unthought, he does include "science itself," and our preference for "elegant sparse laws" over "complex, dense interpretive schema."⁵⁵ These recent statements suggest that we may ourselves be in the midst of an event in the history the social sciences, one that will reconfigure previous structures of thought in sociology and other fields -- in the midst, in short, of an "historic turn."

1. Barrington Moore, Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World (Cambridge, Mass.: Harvard University Press, 1966); Charles Tilly, The Vendee (Cambridge, Mass.: Harvard University Press, 1964); Immanuel Wallerstein, The Modern World System: Capitalist Agriculture and the Origins of the European World-Economy in the Sixteenth Century (New York: Academic Press, 1974). My contention that sociology's historic turn had its origin in a particular moment of domestic and world politics is corroborated by the fact that the work of such liberal historical sociologists as Reinhard Bendix and Neil Smelser has had far less influence on the current generation than that of Moore, Tilly, and Wallerstein. Reinhard Bendix, Work and Authority: Ideologies of Management in the Course of Industrialization (New York: Wiley, 1956) and Nation-Building and Citizenship: Studies of Our Changing Social Order (New York: Wiley, 1964); Neil Smelser, Social Change in the Industrial Revolution (Chicago: University of Chicago Press, 1959). For an interesting autobiographical account of the impact of sixties politics on a young sociologist, see Theda Skocpol, "An 'Uppity Generation' and the Revitalization of Macroscopic Sociology: Reflections at Mid-Career by a Woman from the Sixties," Theory and Society 17 (1988/89):627-44.

2. I owe the "left Weberian" tag to Terry Boswell. It should be noted that some of the remaining sections of the ASA, especially those devoted to Political Sociology and Cultural Sociology, include many sociologists who do historical work.

3. Some prominent examples are Theda Skocpol, ed. Vision and Method in Historical Sociology (Cambridge: Cambridge University Press, 1984); Charles Tilly, Big Structures, Large Processes, Huge Comparisons (New York: Russell Sage Foundation, 1984); Theda Skocpol and Margaret Somers, "The Uses of Comparative History in Macrosocial Inquiry," Comparative Studies in Society and History 22 (1980):174-97; Arthur Stinchcombe, Theoretical Methods in Social History (New York: Academic Press, 1978).

4. Recent exceptions to sociologists' silence about this issue include John R. Hall, "Temporality, Social Action, and the Problem of Quantification in Historical Analysis," Historical Methods, 17 (Fall, 1984):206; Andrew Abbott, "Transcending General Linear Reality," Sociological Theory 6 (1988):169-86; and Ronald Aminzade, "What Is Historical About Historical Sociology?" Paper presented at the American Sociological Association Meetings in August 1989.

5. The term "evenemential" is an anglicization of the French "evenementiel." The French historian Lucien Febvre coined the term "histoire evenementielle," but it is particularly associated with Fernand Braudel, who contrasted "histoire evenementielle" with "histoire structurelle" and "histoire conjoncturelle." Fernand Braudel, La Mediterranee et le monde mediterraneen a l'epoque de Philippe II, 2 vols. (Paris: Armand Colin, 1982

[1966]). For Febvre and Braudel, "histoire evenementielle" was essentially a term of abuse. I follow Marshall Sahlins in both my anglicizing of the term and in my attempt to rehabilitate it. Marshall Sahlins, "The Return of the Event, Again; With Reflections on the Beginnings of the Great Fijian War of 1843 to 1855 between the Kingdoms of Bau and Rewa," in Clio in Oceania, ed by. A Biersack (Washington, D. C.: Smithsonian Press, forthcoming), p. 2.

6. Sometimes the judgements embodied in the usage of the backward/forward looking dichotomy are moral rather than merely anachronistic, with "forward-looking" meaning that which all "progressives" should favor, and "backward-looking" meaning that which they should abhor. Least common is the literal and in my opinion quite acceptable usage which would designate as "backward-looking" only those movements that explicitly pattern themselves on what they regard as a past historical condition and as "forward-looking" those that pattern their actions on an imagined future. The dichotomy has, however, been so spoiled by teleological usages that employing it literally for such supposedly "forward-looking" but actually backward-looking movements as the Renaissance or such supposedly "backward-looking" but actually forward-moving movements as early utopian socialism would merely breed confusion.

7. Immanuel Wallerstein, The Modern World System, Vol. 1, Capitalist Agriculture and the Origins of the European World-Economy in the Sixteenth Century (New York: Academic Press, 1974), p. 7.

8. Tilly, The Vendee. Tilly's writings on French collective violence include the following. "Collective Violence in European Perspective," in Violence in America: Historical and Comparative Perspectives, ed. Hugh Davis Graham and Ted Robert Gurr (Washington: U. S. Government Printing Office, 1969), pp. 5-34; "How Protest Modernized in France," in The Dimensions of Quantitative Research in History, ed. William Aydelotte, Allan Bogue, and Robert Fogel (Princeton: Princeton University Press, 1972), pp. 192-255; "The Modernization of Political Conflict in France," in Perspectives on Modernization: Essays in Memory of Ian Weinberg, ed. Edward B. Harvey, (Toronto: University of Toronto Press, 1972), pp. 50-95; "Getting it Together in Burgundy," Theory and Society 4 (1977):479-504; "The Web of Contention in Eighteenth Century Cities," in Class Conflict and Collective Action, ed. Louise Tilly and Charles Tilly (Beverly Hills: Sage, 1981), pp. 27-52; The Contentious French (Cambridge, Mass.: Harvard University Press, 1986). See also the following jointly authored works: James Rule and Charles Tilly, "1830 and the Unnatural History of Revolution," Journal of Social Issues 28 (1972):49-76; David Snyder and Charles Tilly, "Hardship and Collective Violence in France, 1830-1960," American Sociological Review 37 (1972):520-32; A. Q. Lodhi and Charles Tilly, "Urbanization, Criminality, and Collective Violence in Nineteenth-Century France," American Journal of Sociology 79

(1973):296-318; Edward Shorter and Charles Tilly, Strikes in France, 1830 to 1968 (New York: Cambridge University Press, 1974); Lynn Lees and Charles Tilly, "Le Peuple de juin 1848," Annales: Economies, Societes, Civilisations 29 (1974):1061-91; Charles Tilly, Louise Tilly, and Richard Tilly, The Rebellious Century, 1830-1930 (Cambridge, Mass.: Harvard University Press, 1975).

9. Tilly, The Vendee, pp. 6-13.

10. Tilly, The Vendee, p. 16.

11. Tilly, The Vendee, pp. 11-12.

12. Tilly, The Vendee, p. 57.

13. Tilly, The Vendee, p. 12, emphasis in original.

14. Marc Bloch, French Rural History: An Essay on Its Basic Characteristics, trans. by Janet Sondheimer (Berkeley and Los Angeles: University of California Press, 1970).

15. Tilly's terminology shifted more than once over the nearly two decades he devoted to this project, but the underlying categorization of violence has remained essentially constant. For a fuller discussion, see William H. Sewell, Jr., "Collective Violence and Collective Loyalties in France: Why the French Revolution Made a Difference," Politics and Society 18 (forthcoming, December 1990).

16. Conscience dictates that I attribute this felicitous term to Carl Schorske, who applied it to the ever-rising bourgeoisie in a lecture on European intellectual history at Berkeley in the 1960s, when I was a graduate student.

17. This argument is stated most baldly in Tilly, "Getting It Together in Burgundy." I attempt to refute his claim at length in Sewell, "Collective Violence and Collective Loyalties."

18. Theda Skocpol, States and Social Revolutions: A Comparative Analysis of France, Russia, and China (Cambridge: Cambridge University Press, 1979), p. 36.

19. Michael Burawoy points out some flaws in Skocpol's argument in "Two Methods in Search of Science: Skocpol versus Trotsky," Theory and Society 18 (1989), pp. 765-9. In what follows, I shall borrow freely from Burawoy's arguments, but will add some of my own where appropriate. The term "natural experiments" is mine, not Burawoy's or Skocpol's'.

20. Burawoy, "Two Methods," pp. 767-8.

21. Skocpol, States and Social Revolutions, pp. 252-62.

22. Here one might cite the example of Jeffery M. Paige's Agrarian Revolution: Social Movements and Export Agriculture (New York: Free Press, 1978), which analyzed statistically a large number of cases of agrarian revolts.

23. Skocpol, States and Social Revolutions, p. 36.

24. Burawoy, "Two Methods," p. 769. The relevant passage in Skocpol is States and Social Revolutions, p.113.

25. This unwillingness to consider intervening events also gets her into muddy water in her discussion of why the French Revolution consolidated private property when the Russian and Chinese revolutions abolished it. See William H. Sewell, Jr., "Ideologies and Social Revolutions: Reflections on the French Case," Journal of Modern History 57 (1985):59.

26. For example, if performing an experiment on the effects of fertilizer on corn yields increases the concentration of helpful soil bacteria on a plot of land, a second trial on that plot will not be independent of the first because the fact that a prior trial has been carried out there will have a positive effect on crop yields.

27. Marc Bloch, "A Contribution Towards a Comparative History of European Societies," p.46. For an exposition of Bloch's comparative method that espouses the sort of experimental logic I have criticized here, see William H. Sewell, Jr., "Marc Bloch and the Method of Comparative History," History and Theory 6 (1967): 208-18.

28. Bloch, "Toward a Comparative History," p. 47.

29. Once again, my assessment runs closely parallel to Burawoy's conclusion that Skocpol's work is at its most powerful precisely where it deviates from her announced methodology. Burawoy, "Two Methods," p. 778.

30. Skocpol, States and Social Revolutions, p. 320, n. 9.

31. Perhaps the best previous model of such a multiple causal narrative is Georges Lefebvre's The Coming of the French Revolution, trans. by R. R. Palmer (Princeton: Princeton University Press, 1971 [1949]), which recounts how four distinct revolutions -- an aristocratic revolution, a bourgeois revolution, a municipal revolution, and a peasant revolution -- combined to make what we call the French Revolution of 1789. One wonders how much influence this book had on Skocpol's thinking.

32. Imre Lacatos, The Methodology of Scientific Research Programmes (Cambridge: Cambridge University Press, 1978).

33. Skocpol, States and Social Revolutions, pp. xii-xiii.

34. Leon Trotsky, History of the Russian Revolution (Ann Arbor: University of Michigan Press, 1960); Georges Lefebvre, The Great Fear: Rural Panic in Revolutionary France (New York: Pantheon, 1973) and The Coming of the French Revolution.

35. This has been demonstrated most eloquently, and with the most theoretical clarity, by the works of Marshall Sahlins. Marshall Sahlins, "The Return of the Event"; Historical Metaphors and Mythical Realities: Structure in the Early History of the Sandwich Islands Kingdom (Ann Arbor: University of Michigan Press, 1981); and Islands of History (Chicago: University of Chicago Press, 1985).

36. Sahlins, "The Return of the Event," p. 2.

37. Mark Traugott, Armies of the Poor: Determinants of Working-Class Participation in the Parisian Insurrection of June 1848 (Princeton: Princeton University Press, 1985) and Howard Kimeldorf, Reds or Rackets? The Making of Radical and Conservative Unions on the Waterfront (Berkeley and Los Angeles: University of California Press, 1988).

38. Traugott, Armies of the Poor, pp. 56-77.

39. Traugott, Armies of the Poor, p. 83.

40. Traugott, Armies of the Poor, p. 115.

41. Katherine Chorley, Armies and the Art of Revolution (London: Faber and Faber, 1943).

42. Traugott, Armies of the Poor, pp. 86-113.

43. Traugott, Armies of the Poor, pp. 148-65.

44. Traugott, Armies of the Poor, pp. 165-8.

45. Traugott, Armies of the Poor, p. 184.

46. Traugott, Armies of the Poor, p. 189.

47. Kimeldorf, Reds or Rackets?, pp. 20-37.

48. Kimeldorf, Reds or Rackets?, pp. 37-50.

49. Kimeldorf, Reds or Rackets?, pp. 60-75.

50. Kimeldorf, Reds or Rackets?, pp. 81-92.

51. Kimeldorf, Reds or Rackets?, pp. 92-7.

52. Kimeldorf, Reds or Rackets?, pp. 100-10.

53. Kimeldorf, Reds or Rackets?, pp. 109-10.

54. Charles Tilly, "Future History," Theory and Society 17 (1988): 710.

55. Immanuel Wallerstein, "Should We Unthink the Nineteenth Century?" Paper prepared for the 10th Political Economy of the World-System Conference, San Francisco State University, March 6-8, 1986, pp. 10-1.

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