



To Explain Political Processes

Charles Tilly

American Journal of Sociology, Volume 100, Issue 6 (May, 1995), 1594-1610.

Stable URL:

<http://links.jstor.org/sici?sici=0002-9602%28199505%29100%3A6%3C1594%3ATEPP%3E2.0.CO%3B2-J>

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/about/terms.html>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

American Journal of Sociology is published by The University of Chicago Press. Please contact the publisher for further permissions regarding the use of this work. Publisher contact information may be obtained at <http://www.jstor.org/journals/ucpress.html>.

American Journal of Sociology
©1995 The University of Chicago Press

JSTOR and the JSTOR logo are trademarks of JSTOR, and are Registered in the U.S. Patent and Trademark Office. For more information on JSTOR contact jstor-info@umich.edu.

©2003 JSTOR

To Explain Political Processes¹

Charles Tilly

New School for Social Research

Analysts of large-scale political processes frequently invoke invariant models that feature self-contained and self-motivating social units. Few actual political processes conform to such models. Revolutions provide an important example of such reasoning and of its pitfalls. Better models rest on plausible ontologies, specify fields of variation for the phenomena in question, reconstruct causal sequences, and concentrate explanation on links within those sequences.

Asked to explain particular instances of vigilante violence, social movements, citizenship, wars, nationalism, or transformation of states, sociologists search almost instinctively for general, invariant models of those phenomena to which they can assimilate the cases at hand. Reflecting on why the disintegration of the Soviet Union and its Warsaw Pact allies so surprised Western analysts, sociologists immediately wonder: Into what general category of recurrent events should they have put the Eastern European experience of the 1980s? Does it belong to revolution, nationalism, democratization, political modernization, imperial disintegration, or something quite different? Sociologists suppose that if they had recognized the category when the process began they would have been able to predict its outcome.

S. N. Eisenstadt (1992, p. 21), for example, places the breakdowns of communist regimes in parallel with other revolutions: "Are these revolutions 'great revolutions'—the English civil war, the American, French, Russian, and Chinese revolutions—which in many ways ushered in modernity and created the modern political order? Are they likely to lead—after a possibly turbulent period of transition—to a relatively stable world of modernity, with liberal constitutionalism heralding some kind of 'end of history'? Or do they tell us something of the vicissitudes and fragilities of modernity, even of democratic-constitutional regimes?"

¹ Correspondence may be addressed to Charles Tilly, Center for Studies of Social Change, New School for Social Research, 64 University Place, New York, New York 10003-4520.

Eisenstadt replies that some part of each of these questions is true. The Eastern European transitions qualify as revolutions, he says, but unlike their predecessors they set their faces against, rather than for, modernity. In the very process of identifying differences, Eisenstadt reinforces the idea that a useful model of revolution specifies similarities, invariant general processes. There he joins the majority of other analysts.

As Eisenstadt does, moreover, sociologists usually assume that the processes in question occur within self-contained social units—societies, states, aggrieved populations, or something of the sort—in a self-propelled way. They assume coherent, durable monads rather than contingent, transitory connections among socially constructed identities. We can hardly blame them for it; we veterans taught them to do it in graduate school, because that is also what we learned to do in graduate school years earlier. We learned and in turn taught a practice of this sort: (1) assume a coherent, durable, self-propelling social unit; (2) attribute a general condition or process to that unit; (3) invoke or invent an invariant model of that condition or process; (4) explain the behavior of the unit on the basis of its conformity to that invariant model.

The most egregious examples of invariant thinking appear in comparative-historical analyses where nations, states, or societies serve as the objects of comparison. Even methodological individualists frequently follow the same logic, albeit on a smaller scale. They model the necessary/sufficient conditions under which a rational decision maker (or, in other versions, the follower of a unitary vision, illusion, or impulse) would take steps to create a state, start a war, rebel, secede, vote, join a social movement, or carry on some other well-defined political performance.

Similar reasoning appears frequently in studies of nationalism, democratization, the disintegration of empires, social movements, transformations of states, wars, revolutions, and other large-scale political phenomena. In the case of nationalism, available theories range from primordialist to constructivist, from realist to subjectivist, but a surprising proportion of them claim not to account for the variable degrees or qualities of nationalism but to place most or all nationalisms in the same box (for convenient surveys, see Anderson 1991; Comaroff and Stern 1993; Connor 1987; Feschbach 1987; Gellner 1983; Haas 1986; Hobsbawm 1990; Kearney 1991; Lerner 1991; Lowi 1992; Löwy 1989; Segal 1988; Williams 1989). The study of social movements offers more promising recent trends, since a number of scholars have taken to relating variation in the organization of movements systematically to differences and fluctuations in political opportunity structure (e.g., Duyvendak 1994; Giugni and Kriesi 1990; Koopmans 1993; Kriesi 1993; Tarrow 1993). Yet even in this area much theorizing has proceeded as if all social movements fell into just two internally homogeneous categories: old and new (Cohen

1985; Diani 1992; Eyerman and Jamison 1991; Mayer 1991; Melucci 1985, 1989).

In analyses of democratization and state formation, similar simplifications prevail (for surveys, see Alker 1992; Barkey and Parikh 1991; Bratton 1989; Caporaso 1989; Dahl 1989; Diamond and Marks 1992; Di Palma 1990; Gurr 1988; Gurr, Jagers, and Moore 1990; Hall and Ikenberry 1989; Held 1987; Kirby and Ward 1991; Krasner 1984; Lee 1988; Mann 1990; Mitchell 1991; O'Donnell and Schmitter 1986; Poggi 1990; Rueschemeyer, Stephens, and Stephens 1992; Schmitter and Karl 1991). Most and Starr have long since offered the same kind of complaint about studies of war (1983; see also Levy 1989; Starr 1994). In short, invariant models concerning self-motivating social units continue to wind like honeysuckle through the study of large-scale political processes.

If it is examined closely, the standard practice makes little sense. Coherent, durable, self-propelling social units—monads—occupy a great deal of political theory but none of political reality. Ostensible general conditions such as revolution, nationalism, or war always turn out to fall not at a single point but to stretch along a whole range of positions on some intersecting set of continua. The employment of invariant models, furthermore, assumes a political world in which whole structures and sequences repeat themselves time after time in essentially the same form. That would be a convenient world for theorists, but it does not exist.

Although the assumption of sharply bounded, self-motivating social units deserves equal criticism, William H. Sewell, Jr. (1992), Margaret Somers (1992), Harrison White (1992), and others have recently criticized monadic thinking so effectively that—however much I disagree with some of their proposed remedies—I have little to add to their critiques. Let me therefore concentrate here on the assumption of invariant conditions and processes. The general structure runs like this:

1. All *A*'s have characteristics *X*, *Y*, and *Z*.
2. Case α is an *A*.
3. Therefore α has characteristics *X*, *Y*, and *Z*.

A can translate as “revolution,” “nationalism,” “war,” or something else, while *X*, *Y*, and *Z* can constitute necessary conditions, sufficient conditions, standard sequences, correlates, or consequences. A statement in this form can easily reduce to a definition, merely affirming that there exists a set of instances sharing properties *X*, *Y*, and *Z*. The statement need not reduce to a definition, however, since the argument can readily incorporate causal, sequential, or transactional links among the elements. The argument does not assert that all instances of *A* are identical, but it does assert that they share essential properties setting them off from all cases of non-*A*; those essential universals mark any such model as invari-

ant. Analysts often arrive at this sort of argument through empirical comparison of cases α , β , Γ , and so on, searching for the cases' common properties that qualify them all as A 's. In the domain of large-scale politics, at least, such reasoning so badly describes what actually occurs as to hinder sociological analysis.

"Invariant" does not equal "general." Laws concerning variation sometimes cover a very general range. For an example, consider Boyle's law: at a given temperature the pressure of a certain mass of confined gas varies inversely with its volume. Although we have no well-established sociological laws with the elegance of Boyle's formulation, broad and robust empirical generalizations concerning variation—for example, that over large populations infant mortality declines as literacy rises—abound in different realms of social life. I am not challenging the possibility of more explicitly causal laws of extremely general scope, just so long as they stipulate variation. I am instead challenging the common, if often implicit, claim for essential, invariant universals.

Such claims appear frequently in macrosociology. Take the case of revolutions (for helpful reviews, see Berejikian 1992; Boswell 1989; DeFronzo 1991; Dunn 1989; Goodwin and Skocpol 1989; Hobsbawm 1986; Keddie 1992; Kimmel 1990; Knight 1992; Outram 1992; Rice 1990; Schutz and Slater 1990; Taylor 1984; Wickham-Crowley 1991; Zimmerman 1983). Generations of scholars have pursued the chimera of an invariant general model of revolution. Fixation on invariant models gives rise to a common but logically peculiar sociological performance we may call "improving the model." It consists of (1) outlining a widely accepted model of phenomenon A , (2) identifying an instance of A that fails to fit the model in one or more ways, (3) modifying the model so that it now accommodates the previously exceptional instance as well as those instances that already belonged to its domain. Most often the crucial modification respecifies a condition postulated as necessary in the model's previous version. Thus improving the model expands the claimed scope of the alleged invariance. The procedure is peculiar both because it makes implausible allegations of invariance and because it attenuates whatever empirical grasp the previous model attained. Yet as a reviewer for professional journals I read a half-dozen drafts each year that follow just such reasoning.

Similar reasoning motivates whole books. When Farideh Farhi compares the Iranian and Nicaraguan revolutions that began in 1979, for example, she explicitly sets up the analysis as an extension of Theda Skocpol's *States and Social Revolutions* on the grounds that Skocpol's book is "perhaps the most comprehensive attempt to bring together the new concerns about the role of the state, the structure of peasant communities, and the role of international factors in understanding the processes

and outcomes of revolutions" (Farhi 1990, p. 5). Her methodological declaration runs this way: "The essence of comparative history is to maintain the particularity of each case while accepting that each particularity is shaped by general forces operating at the societal or global level. Accordingly, the intention is to expose these forces as they impinge on quite specific and unique circumstances in the hope of shedding light on historical specificities as well as the changing structures in the larger world-historical context that make contemporary revolutions not utterly unlike 'classic' revolutions but also not totally similar to them" (Farhi 1990, p. 2). Thus all revolutions share attributes *X*, *Y*, and *Z*, even if they differ with respect to a great many other attributes; an effective analysis combines specification of universals with enumeration of particulars.

True to the challenge, Farhi works with a checklist drawn directly from Skocpol: conditions favoring class coalitions against the regime, circumstances promoting the mobilization of those coalitions for revolutionary action, factors making the state vulnerable to attack, and so on. Almost inexorably, this leads her to propose one-for-one substitutes for the factors Skocpol emphasized—for example, Farhi offers the connectedness and proximity to power of capital cities as a substitute for Skocpol's solidarity of peasant villages. She finally seeks to build a bridge from Skocpol's model to her own by (*a*) showing how the world development of capitalism has altered class structures since the times of Skocpol's revolutions and (*b*) attributing more importance to ideology, including religious belief, than Skocpol was ready to concede in 1979.

Such an analysis aims to generalize Skocpol's model rather than to extract from it principles of variation. But it misses the mark: the collapsing agrarian bureaucracy overburdened by international pressures and the autonomous peasant communities aligned against their landlords—keystones of Skocpol's theory—disappear from view, with their replacements in Farhi's analysis by no means members of the same causal categories. Thus Farhi draws useful questions from Skocpol, but in pursuit of those questions tacitly abandons the effort to generalize an invariant model of revolution. Indeed, she has no choice; the model will not, cannot, generalize that far.

Another recent example marks even more precisely the blind alley into which the quest for invariant models has led analysts of revolution. In their excellent compilation on Third World revolutions, Jack Goldstone, Ted Gurr, and Farrokh Moshiri (1991) offer an "analytical framework" that continues the quest for invariance. En route, however, they make two turns that send them in precisely the opposite direction, toward broad and incessant variation.

In his theoretical introduction, Goldstone singles out a trio of recurrent

causes for revolution: “Declining state resources relative to expenses and the resources of adversaries, increasing elite alienation and disunity, and growing popular grievances and autonomy” (Goldstone et al. 1991, p. 49). The list echoes Goldstone’s *Revolution and Rebellion in the Early Modern World*, published the same year as the edited volume, but written over many previous years. In that book, Goldstone gives a strong tone of breakdown to his most dramatic statements:

The causes of revolutions and major rebellions operate in ways that seem remarkably similar to the forces that build up to cause earthquakes. That is, in the years before such a revolution or major rebellion, social pressures for change build. Yet the existing social and political structures for some time resist change (even though pressures and deformations may be visible). Suddenly, however, some response to the mounting pressure—a state bankruptcy, a regional rebellion—occurs which weakens that resistance (like a block breaking off along the fault). At that point, there is a sudden release of the pent-up forces and a crumbling of the old social structures—a revolution or major rebellion. More concretely, the Scots and Irish rebellions in Great Britain in 1637–1641, and the state bankruptcy and calling of the Estates General in France in 1789, were themselves responses to the mounting social and fiscal pressures in those societies. Yet these particular events also served to unleash far greater social pressures, which overwhelmed these states and led to revolutions. (Goldstone 1991, p. 35)

Note several features of this statement: its emphasis on sudden collapse in response to long-term change, its claims to generality, its insistence on uniformity rather than variation.

In his first contribution to *Revolutions of the Late Twentieth Century*, Goldstone claims continuity in these terms: “In my work on early modern revolutions, I identify three conditions whose *conjunction* led to state breakdown: fiscal distress, elite alienation and conflict, and a high potential for mobilization of the populace. Although the particular forces that create these conditions may be quite different in contemporary societies than in earlier ones, I believe these conditions remain central to the development of revolutionary crises” (Goldstone 1991, pp. 37–38). Leaping nimbly past the problem of specifying how an observer would know in advance of a revolution’s actual occurrence when the three bundles of causes were approaching critical mass, Goldstone immediately concedes that these conditions “may be produced by a variety of forces, depending on how they interact with the institutions and structures in particular societies” (p. 49). Population pressure, that powerful propellant of state breakdown in Goldstone’s *Revolution and Rebellion*, now fizzles to a force that “may have either positive or negative impact” (Goldstone et al. 1991, p. 40).

Goldstone et al. also propose three general stages of revolution: state crisis, the struggle for power, efforts at reconstruction. These stages,

however, constitute no verifiable theory; they follow tautologically from the book's definition of revolution as "the forcible overthrow of a government followed by the reconsolidation of authority by new groups, ruling through new political (and sometimes social) institutions" (Goldstone et al. 1991, p. 37). At the end of the first turn, then, our voyagers have little more baggage than the explication of a definition.

Then they arrive at the second turn: a recognition that post-1945 revolutions occur in quite different ways from their predecessors because geopolitical settings, dominant ideologies, and international intervention have changed fundamentally. Indeed, the cases they consider—Vietnam, Nicaragua, Iran, Poland, Afghanistan, the Philippines, Cambodia, Zimbabwe, South Africa, and Palestine—amply demonstrate different patterns from the great revolutions of England, France, Russia, or China. "We think now that a state crisis should not be defined as a specific objective condition but rather as a situation in which significant numbers of elites and popular groups believe that the central authorities are acting in ways that are fundamentally ineffective, immoral, or unjust" (Goldstone et al. 1991, pp. 330–31). Thus one of the three general conditions (relative decline of state resources) collapses into the other two (elite alienation and popular grievances) while injustice, previously invisible, squeezes its way into the argument. By this time, the initial promise of an invariant general model has vanished. Just as the once-hot search for crisp predictors of earthquakes has given way to more general debate about the variable operation of plate tectonics (Acton and Gordon 1994; Girdler and McConnell 1994), the search for unique, invariant properties of revolution has ceded to the conception of a variable field within which revolutions occur.

I have not chosen my example because I think Goldstone et al. are obtuse or empirically mistaken. On the contrary, they have drawn correct conclusions from their evidence: the conditions for revolution are not uniform, but vary from region to region and period to period. The conditions vary as politics in general varies. Because within a given region and period many states share political arrangements, national and international, rough similarities and explicable variations appear in the experiences of connected states with revolution. The search for comparisons close at hand therefore advances understanding, while the attempt to build transhistorical models of revolution is doomed to eternal failure. Goldstone et al. only err in refusing to recognize the general implications for method and theory of their own compelling analyses.

Similar conditions prevail in the study of social movements, nationalism, democratization, and a wide variety of other political phenomena, as well as in the zones of organizational behavior, crime, or urban structure. Over and over sociologists assume coherent, durable, self-propelling

social units, attribute general conditions or processes to those units, invoke or invent invariant models of the relevant conditions or processes, then explain the unit's behavior on the basis of its conformity to that invariant model. It is time to expunge that intellectual procedure.

I am making no plea for historical particularism, much less for epistemological relativism or postmodern linguisticism. I am arguing that regularities in political life are very broad, indeed transhistorical, but do not operate in the form of recurrent structures and processes at a large scale. They consist of recurrent causes which in different circumstances and sequences compound into highly variable but nonetheless explicable effects. Students of revolution have imagined they were dealing with phenomena like ocean tides, whose regularities they could deduce from sufficient knowledge of celestial motion, when they were actually confronting phenomena like great floods, equally coherent occurrences from a causal perspective, but enormously variable in structure, sequence, and consequences as a function of terrain, previous precipitation, built environment, and human response.

For hydrologists, a flood is a wave of water that passes through a basin; a severe flood is one in which a considerable share of the water overflows the basin's perimeter. For our purposes, the equations hydrologists use to compute water flow in floods have three revealing characteristics: they reduce floods to special cases of water flow within basins rather than making them *sui generis*, their results depend heavily on the hydrologist's delineation of the basin, while estimation of the flood's parameters requires extensive empirical knowledge of that basin. Yet the equations embody very general principles, the physics of incompressible fluids in open channels (Bras 1990, pp. 478–82).

Note several implications of the analogy. First, every instance of the phenomenon—flood or revolution—differs from every other one; the test of a good theory is therefore not so much to identify similarities among instances as to account systematically and parsimoniously for their variation. Second, in different combinations, circumstances, and sequences, the same causes that produce floods or revolutions also produce a number of adjacent phenomena: smoothly flowing rivers and stagnant swamps on the one side, coups d'état and guerrilla warfare on the other. Third, time, place, and sequence strongly influence how the relevant processes unfold; in that sense, they have an inescapably historical character. Finally, the events in question are far from self-motivating experiences of self-contained structures; they are local manifestations of fluxes extending far beyond their own perimeters. Floods and revolutions have no natural boundaries; observers draw lines around them for their own analytic convenience. In these regards, they resemble a number of other complex but lawful phenomena: traffic jams, earthquakes, segmented labor mar-

kets, forest fires, and many more. I suppose, indeed, that most interesting social phenomena have exactly these characteristics.

How, then, should we search for the causes of revolutions? Arthur Stinchcombe has long since described one version of the explanatory program: to identify deep causal analogies across detailed features of ostensibly different historical sequences. (The cause of event *X* is the minimum set of antecedents that [1] actually occurred, [2] is generally sufficient to produce events of type *X*, and [3] without which *X* would not have occurred in this setting.)

Concepts are the things that capture aspects of the facts for a theory; they are the lexicon that the grammar of theory turns into general sentences about the world. The argument is that the power and fruitfulness of those sentences is determined by the realism and exactness of the lexicon of concepts, and not by the theoretical grammar. The problem of eliminating false sentences by research, the traditional problem of epistemology, is not as problematic as the problem of having sentences interesting enough to be worth accepting or rejecting. And this is determined by whether or not our concepts capture those aspects of reality that enter into powerful and fruitful causal sentences. (Stinchcombe 1978, p. 115)

For this purpose, Stinchcombe recommends ignoring the "epochal theories" invoked by a Trotsky or a Tocqueville in favor of the causal reasoning by which these thinkers chain together narratives. That means breaking down big events into causally connected sequences of events, and examining each link in the chain. More generally, Stinchcombe advocates a shift of attention away from a priori theorizing toward rigorous examination and reduction of analogies, step by step within causal sequences (Stinchcombe 1978, p. 28). At that level, says Stinchcombe, much of the apparent disagreement between a Trotsky and a Tocqueville dissolves. Great historical analysts employ far more similar causal accounts than their competing epochal pronouncements suggest.

Stinchcombe stresses epistemology, conditions for the generation of knowledge. I am stressing ontology, the nature of that which is to be known. But our programs dovetail. If the social world actually fell into neatly recurrent structures and processes, then epochal theories, invariant models, and the testing of deductive hypotheses would become more parsimonious and effective means of generating knowledge. Because the social world does *not* conform to that prescription, we need other programs on both ontological and epistemological grounds. Our programs converge in the historically embedded search for deep causes operating in variable combinations, circumstances, and sequences with consequently variable outcomes. Most of the work therefore concerns not the identification of similarities over whole structures and processes but the explanation of variability among related structures and processes. In studies of

revolution, the work entails explaining why and how different sorts of social settings produce different varieties of forcible seizures of power over states.

There is hope. Not everyone who analyzes revolutions and related phenomena resorts to invariant models. In a wide-ranging synthesis written before the Soviet Union collapsed, David Laitin sketched a promising theory of variation in the readiness of different national elites to break with Moscow. It argued in part that

the historical dimension that accounts for distinctions between the national movements in the Soviet Union is based upon a single variable—the degree to which elites in the peripheral nationalities received most-favored-lord status in Russia. The historical data show that in the territories west of Moscow, most-favored-lord status was readily granted, even when there were no indigenous lords. Lords in the Turkic areas were often given elite privileges, but they were not given access to positions of high status by right. In intermediate cases like Georgia and Estonia, elite mobility was possible but circumscribed. Certain predictions follow from this: (1) in the most-favored-lord regions there would be powerful symbolic unity among titulars for full independence but a waning of resolve as the conflict of interest among two branches of the titular elites begins to manifest itself; and (2) in the non-most-favored lord regions, the pressure for independence would come more slowly (but once set in motion, there would be unity among the titular elites, with only settled minority populations seeking to slow the process down). (Laitin 1991, p. 157)

Laitin simplifies his work by grouping Soviet regions into two categories, but he clearly invokes a continuous principle of variation. He does not, obviously, provide a complete account of the process by which the Soviet Union collapsed, or by which any particular state emerged from the collapse. On the contrary, he offers a promising candidate for one of the many general principles a properly constructed causal account would invoke.

By his rational choice analysis of conditions for secession, Michael Hechter opens another avenue. Hechter argues that secession results from the intersection of four partly independent processes: (1) creation of regions, (2) mobilization for collective action, (3) development of support for secessionist programs, (4) acceptance of independence by the previously dominant state (Hechter 1992, p. 269). In each case Hechter identifies conditions affecting the extent of two factors: shared or imposed interest in acting to facilitate secession and the capacity to do so. Under the heading of support for secession, for example, he proposes (*a*) low regional dependence on the host economy and (*b*) perception of the host state's weakness as major promoters of interest and capacity.

Hechter employs a model of logical concatenation rather than of sequence or political processes; except in the sense of logical necessity, he

neither offers propositions concerning the interaction of his four processes nor postulates a dynamic in which identities, connections, interests, and capacities alter as a function of struggle or accommodation. Within the standard a priori limits of rational choice analysis, nevertheless, Hechter's discussion does provide a framework that lends itself to the analysis of choice making in territorial segmentation, civil war, and regionally based revolution (Berejikian 1992; Connor 1987; Gurr 1993; Licklider 1993; Lustick 1993; Strang 1990, 1991). By depicting secession as a highly contingent outcome of interacting political processes, Hechter breaks sharply with invariant models.

In an inquiry that deals more explicitly with structure and sequence than Hechter's does, Peter Bearman (1993) looks closely at changing relations among gentry in Norfolk, England, during the century before the Civil War, which began in 1640. Using the formal techniques of network analysis, Bearman shows that a kinship-based regional structure of power gave way to one based much more heavily on patron-client chains connecting local actors to national centers of power, that gentry experiencing blocked or downward mobility clustered together disproportionately in patron-client networks forming distinctive, antiregime religious identities, and that these shared identities-cum-networks became major bases of political mobilization (Bearman 1993; Bearman and Deane 1992). At no point does Bearman suggest that blocked mobility, patron-client networks, or the other factors he analyzes generally produce revolution; he promulgates no invariant model. But he does provide another illustration of a program that invokes powerful general causes in a particular reconstruction of revolutionary processes.

My own version of that enterprise concentrates on variation within Europe over the last five centuries (Tilly 1993). It distinguishes between revolutionary situations (moments of deep fragmentation in state power) and revolutionary outcomes (rapid, forcible, durable transfers of state power), and it designates as a full-fledged revolution any extensive combination of the two. Chronologies of revolutionary situations in multiple regions of Europe demonstrate the great variation and change in revolutionary processes since 1492. The changes include, for example, an impressive rise in frequency of "national" revolutionary situations: state-fragmenting mobilizations in which at least one party made its claim to state power on the grounds that it represented a coherent, culturally distinct population that was currently receiving unjust treatment.

More important, the revolutionary chronologies illustrate—prove would be too strong a word—how regionally and temporally variable forms of international relations, state power, administrative structure, military activity, extraction, and repression shaped the character of European revolutions, not to mention other forms of political conflict. To the

extent that governmental succession depended on warrior-kings recruited from intermarrying royal patrilineages, for example, revolutionary situations were concentrated at those points when a child or an incompetent came to the throne. In regions of intense commercial activity, for another example, revolutionary situations commonly took the form of urban resistance to princely authority. Revolution turns out to be a coherent phenomenon, but coherent in its variation and in its continuity with nonrevolutionary politics, not in any repetitious uniformity. Its sequences and outcomes turn out to be path, time, and situation dependent, not constant from one revolution to the next.

I do not claim to have been the first to notice this degree of variation; in their practical work, as opposed to their introductions and conclusions, most students of real revolutions proceed as if they were dealing with path-, time-, and situation-dependent phenomena whose individual features—but perhaps not whose totality—can be explained by general political principles, given sufficient information about the context. Nor do I claim that my own recent work provides all the answers to the big questions that students of revolution have been pursuing for centuries. I make only three simple claims: (1) The construction of invariant models of revolution—which remains a major activity among American sociologists—is a waste of time. (2) The poor fit between such models and the actual character of revolutions helps account for the slow accumulation of knowledge on the subject, a problem about which Rod Aya (1990) and James Rule (1989) have recently and properly complained. (3) The same conclusions hold for a wide range of social phenomena, including most or all large-scale political processes.

How, then, can we recognize useful alternatives to invariant models of political processes? Valid analyses of political processes rest first of all on plausible ontologies—representations of what is to be explained in terms of a given process's boundedness, continuity, plasticity, and complexity (e.g., recognizing that nationalism consists of some actors' claim to act authoritatively on behalf of a coherent and solidary people, a claim whose origins, makers, forms, and effects vary enormously over time and space). For variant phenomena, valid analyses specify the fields of variation within which they fall, which means specifying their relation to connected but different phenomena (e.g., delineating the continuum along which lie army-to-army interstate war, covert military intervention, full-scale civil war, guerrilla activities, and terrorism). These valid analyses break complex sequences into events, each of which invokes its own configuration of causes including the cumulative effects of previous events (e.g., separating the conditions under which a cycle of intense social movement activity begins or ends from the conditions under which one movement or another sees some of its demands realized). Their gen-

eral propositions consist of principles of variation for analytically separable aspects of the phenomena under examination (e.g., after noticing that democracy entails broad, relatively equal citizenship that grants citizens substantial collective control over governmental personnel and policies as well as significant protection from arbitrary state action, formulating or invoking separate theories of breadth, equality, control, and protection).

Such analyses immediately yield counterfactuals, specifications of what else could have happened if the causal configuration had occurred differently; thus a valid theory of democratization yields propositions about the conditions for authoritarianism and oligarchy. Within limits, such analyses of variation also yield contingent predictions. By this I do not mean the unconditional predictions of invariant models, in which the appearance of sufficient conditions X , Y , and Z invariably produce outcome A , but contingent predictions applying phrases such as "insofar as" to variable conditions, their interactions, and their outcomes. In instances such as Eastern Europe's struggles of 1989, then, we might reasonably hope to specify the fields of variation within which they were occurring, then to anticipate the likely outcomes under various still-contingent conditions.

Mine, then, is no counsel of perfection or cry of despair. For, taken separately, these methodological injunctions have the same comfortable familiarity as invariant models. For all their other weaknesses—vulnerability to spatial autocorrelation, assumptions of boundedness and independence of observations, commitments to linearity, and so on—standard sampling designs and multivariate statistics actually presume some such world. Within these limits, which theories of causality and variation analysts should choose remains just as open as before the elimination of invariant models.

Fortunately, we have no obligation to choose right now; we can wait for results of a productive rivalry, perhaps even of a synthesis from these contentions. For the present, anyone who believes what I have said about invariant general models and who cares about the validity of broad political analyses has plenty of work to do. The crucial theoretical and empirical work should eventually reduce the likelihood that the next major change in world politics will baffle sociologists.

REFERENCES

- Acton, G. D., and R. G. Gordon. 1994. "Paleomagnetic Tests of Pacific Plate Reconstructions and Implications for Motion between Hotspots." *Science* (March 4), pp. 1246–54.
- Alker, Hayward. 1992. "The Humanistic Moment in International Studies: Reflections on Machiavelli and Las Casas." *International Studies Quarterly* 36:347–72.

- Anderson, Benedict. 1991. *Imagined Communities: Reflections on the Origin and Spread of Nationalism*, rev. ed. London: Verso.
- Aya, Rod. 1990. *Rethinking Revolutions and Collective Violence: Studies on Concept, Theory, and Method*. Amsterdam: Het Spinhuis.
- Barkey, Karen, and Sunita Parikh. 1991. "Comparative Perspectives on the State." *Annual Review of Sociology* 17:523–49.
- Bearman, Peter S. 1993. *Relations into Rhetorics: Local Elite Social Structure in Norfolk, England, 1540–1640*. New Brunswick, N.J.: Rutgers University Press.
- Bearman, Peter S., and Glenn Deane. 1992. "The Structure of Opportunity: Middle-Class Mobility in England, 1548–1689." *American Journal of Sociology* 98:30–66.
- Berejikian, Jeffrey. 1992. "Revolutionary Collective Action and the Agent-Structure Problem." *American Political Science Review* 86:647–57.
- Boswell, Terry, ed. 1989. *Revolution in the World System*. New York: Greenwood.
- Bras, Rafael L. 1990. *Hydrology: An Introduction to Hydrologic Science*. Reading, Mass.: Addison-Wesley.
- Bratton, Michael. 1989. "Beyond the State: Civil Society and Associational Life in Africa." *World Politics* 41:407–30.
- Caporaso, James A., ed. 1989. *The Elusive State: International and Comparative Perspectives*. Newbury Park, Calif.: Sage.
- Cohen, Jean. 1985. "Strategy or Identity: New Theoretical Paradigms and Contemporary Social Movements." *Social Research* 52:663–716.
- Comaroff, John L., and Paul C. Stern, eds. 1993. "Perspectives on Nationalism and War." Working Paper 163, Center for Studies of Social Change, New School for Social Research.
- Connor, Walker. 1987. "Ethnonationalism." Pp. 196–220 in *Understanding Political Development*, edited by Myron Weiner and Samuel P. Huntington. Boston: Little, Brown.
- Dahl, Robert A. 1989. *Democracy and Its Critics*. New Haven, Conn.: Yale University Press.
- DeFronzo, James. 1991. *Revolutions and Revolutionary Movements*. Boulder, Colo.: Westview.
- Diamond, Larry, and Gary Marks, eds. 1992. "Comparative Perspectives on Democracy: Essays in Honor of Seymour Martin Lipset." Special issue of *American Behavioral Scientist* 35:352–629.
- Diani, Mario. 1992. "The Concept of Social Movement." *Sociological Review* 1992:1–25.
- Di Palma, Giuseppe. 1990. *To Craft Democracies: An Essay on Democratic Transitions*. Berkeley and Los Angeles: University of California Press.
- Dunn, John. 1989. *Modern Revolutions, an Introduction to the Analysis of a Political Phenomenon*, 2d ed. Cambridge: Cambridge University Press.
- Duyvendak, Jan Willem. 1994. *Le poids du politique: Nouveaux mouvements sociaux en France*. Paris: l'Harmattan.
- Eisenstadt, S. N. 1992. "The Breakdown of Communist Regimes and the Vicissitudes of Modernity." *Daedalus* 121:21–42.
- Eyerman, Ron, and Andrew Jamison. 1991. *Social Movements: A Cognitive Approach*. University Park: Pennsylvania State University Press.
- Farhi, Farideh. 1990. *States and Urban-Based Revolutions: Iran and Nicaragua*. Urbana: University of Illinois Press.
- Feshbach, Seymour. 1987. "Individual Aggression, National Attachment, and the Search for Peace: Psychological Perspectives." *Aggressive Behavior* 13:315–25.
- Gellner, Ernest. 1983. *Nations and Nationalism*. Ithaca, N.Y.: Cornell University Press.
- Girdler, R. W., and D. A. McConnell. 1994. "The 1990 to 1991 Sudan Earthquake

- Sequence and Extent of the East African Rift System." *Science* (April 1), pp. 67–70.
- Giugni, Marco G., and Hanspeter Kriesi. 1990. "Nouveaux mouvements sociaux dans les années '80: Evolution et perspectives." *Annuaire Suisse de Science Politique* 30:79–100.
- Goldstone, Jack A. 1991. *Revolution and Rebellion in the Early Modern World*. Berkeley and Los Angeles: University of California Press.
- Goldstone, Jack A., Ted Robert Gurr, and Farrokh Moshiri, eds. 1991. *Revolutions of the Late Twentieth Century*. Boulder, Colo.: Westview.
- Goodwin, Jeff, and Theda Skocpol. 1989. "Explaining Revolutions in the Contemporary Third World." *Politics and Society* 17:489–509.
- Gurr, Ted Robert. 1988. "War, Revolution, and the Growth of the Coercive State." *Comparative Political Studies* 21:45–65.
- . 1993. *Minorities at Risk: A Global View of Ethnopolitical Conflicts*. Washington, D.C.: U.S. Institute of Peace Press.
- Gurr, Ted Robert, Keith Jagers, and Will H. Moore. 1990. "The Transformation of the Western State: The Growth of Democracy, Autocracy, and State Power since 1800." *Studies in Comparative International Development* 25:73–108.
- Haas, Ernst. 1986. "What Is Nationalism and Why Should We Study It?" *International Organization* 40:707–44.
- Hall, John A., and G. John Ikenberry. 1989. *The State*. Minneapolis: University of Minnesota Press.
- Hechter, Michael. 1992. "The Dynamics of Secession." *Acta Sociologica* 35:267–83.
- Held, David. 1987. *Models of Democracy*. Stanford, Calif.: Stanford University Press.
- Hobsbawm, E. J. 1986. "Revolution." Pp. 5–46 in *Revolution in History*, edited by Roy Porter and Mikulas Teich. Cambridge: Cambridge University Press.
- . 1990. *Nations and Nationalism since 1789: Programme, Myth, Reality*. Cambridge: Cambridge University Press.
- Kearney, Michael. 1991. "Borders and Boundaries of State and Self at the End of Empire." *Journal of Historical Sociology* 4:52–72.
- Keddie, Nikki R. 1992. "Can Revolutions Be Predicted: Can Their Causes Be Understood?" *Contention* 1:159–82.
- Kimmel, Michael S. 1990. *Revolution: A Sociological Interpretation*. Philadelphia: Temple University Press.
- Kirby, Andrew, and Michael D. Ward. 1991. "Modernity and the Process of State Formation: An Examination of 20th Century Africa." *International Interactions* 17:113–26.
- Knight, Alan. 1992. "Revisionism and Revolution: Mexico Compared to England and France." *Past and Present* 134:159–99.
- Koopmans, Ruud. 1993. "The Dynamics of Protest Waves: West Germany, 1965 to 1989." *American Sociological Review* 58:637–58.
- Krasner, Steven. 1984. "Approaches to the State: Alternative Conceptions and Historical Dynamics." *Comparative Politics* 16:223–46.
- Kriesi, Hanspeter. 1993. *Political Mobilization and Social Change: The Dutch Case in Comparative Perspective*. Aldershot: Avebury.
- Laitin, David D. 1991. "The National Uprisings in the Soviet Union." *World Politics* 44:139–77.
- Lee, Su-Hoon. 1988. *State-Building in the Contemporary Third World*. Boulder, Colo.: Westview.
- Lerner, Adam J, ed. 1991. "Reimagining the Nation." *Millennium* 20:351–525.
- Levy, Jack S. 1989. "The Causes of War: A Review of Theories and Evidence." Pp. 209–333 in *Behavior, Society, and Nuclear War*, vol. 1. Edited by Philip Tetlock et al. New York: Oxford University Press.

- Licklider, Roy, ed. 1993. *Stopping the Killing: How Civil Wars End*. New York: New York University Press.
- Lowi, Theodore J. 1992. "The State in Political Science: How We Became What We Study." *American Political Science Review* 86:1-7.
- Löwy, Michael. 1989. "Internationalisme, Nationalisme et Anti-impérialisme." *Critique Communiste* 87:31-42.
- Lustick, Ian S. 1993. *Unsettled States, Disputed Lands: Britain and Ireland, France and Algeria, Israel and the West Bank-Gaza*. Ithaca, N. Y.: Cornell University Press.
- Mann, Michael, ed. 1990. *The Rise and Decline of the Nation-State*. Oxford: Blackwell.
- Mayer, Margit. 1991. "Social Movement Research and Social Movement Practice: The U.S. Pattern." Pp. 47-120 in *Research on Social Movements: The State of the Art in Western Europe and the USA*, edited by Dieter Rucht. Frankfurt and Boulder, Colo.: Campus/Westview.
- Melucci, Alberto. 1985. "The Symbolic Challenge of Contemporary Movements." *Social Research* 52:789-816.
- . 1989. *Nomads of the Present: Social Movements and Individual Need in Contemporary Society*. Philadelphia: Temple University Press.
- Mitchell, Timothy. 1991. "The Limits of the State: Beyond Statist Approaches and Their Critics." *American Political Science Review* 85:77-96.
- Most, Benjamin A., and Harvey Starr. 1983. "Conceptualizing 'War': Consequences for Theory and Research." *Journal of Conflict Resolution* 27:137-59.
- O'Donnell, Guillermo, and Philippe C. Schmitter. 1986. *Transitions from Authoritarian Rule: Tentative Conclusions about Uncertain Democracies*. Baltimore, Md.: Johns Hopkins University Press.
- Outram, Dorinda. 1992. "Revolution and Repression." *Comparative Studies in Society and History* 34:58-67.
- Poggi, Gianfranco. 1990. *The State: Its Nature, Development and Prospects*. Stanford, Calif.: Stanford University Press.
- Rice, E. E., ed. 1990. *Revolution and Counter-Revolution*. Oxford: Blackwell.
- Rueschmeyer, Dietrich, Evelyn Huber Stephens, and John D. Stephens. 1992. *Capitalist Development and Democracy*. Chicago: University of Chicago Press.
- Rule, James B. 1988. *Theories of Civil Violence*. Berkeley and Los Angeles: University of California Press.
- Schmitter, Philippe C., and Terry Lynn Karl. 1991. "What Democracy Is . . . and Is Not." *Journal of Democracy* 2:77-88.
- Schutz, Barry M., and Robert O. Slater, eds. 1990. *Revolution and Political Change in the Third World*. Boulder, Colo.: Lynne Rienner.
- Segal, Daniel A. 1988. "Nationalism, Comparatively Speaking." *Journal of Historical Sociology* 1:301-21.
- Sewell, William H., Jr. 1992. "A Theory of Structure: Duality, Agency, and Transformation." *American Journal of Sociology* 98:1-29.
- Somers, Margaret R. 1992. "Narrativity, Narrative Identity, and Social Action: Rethinking English Working-Class Formation." *Social Science History* 16:591-630.
- Starr, Harvey. 1994. "Revolution and War: Rethinking the Linkage between Internal and External Conflict." *Political Research Quarterly* 47:481-507.
- Stinchcombe, Arthur L. 1978. *Theoretical Methods in Social History*. New York: Academic Press.
- Strang, David. 1990. "From Dependency to Sovereignty: An Event History Analysis of Decolonization, 1870-1987." *American Sociological Review* 55:846-60.
- . 1991. "Global Patterns of Decolonization, 1500-1987." *International Studies Quarterly* 35:429-54.
- Tarrow, Sidney. 1993. "Modular Collective Action and the Rise of the Social Movement: Why the French Revolution Was Not Enough." *Politics and Society* 21: 69-90.

American Journal of Sociology

- Taylor, Stan. 1984. *Social Science and Revolutions*. New York: St. Martin's.
- Tilly, Charles. 1993. *European Revolutions, 1492–1992*. Oxford: Blackwell.
- White, Harrison. 1992. *Identity and Control: A Structural Theory of Social Action*. Princeton, N.J.: Princeton University Press.
- Wickham-Crowley, Timothy. 1991. *Exploring Revolution: Essays on Latin American Insurgency and Revolutionary Theory*. Armonk, N.Y.: M. E. Sharpe.
- Williams, Brackette F. 1989. "A Class Act: Anthropology and the Race to Nation across Ethnic Terrain." *Annual Review of Anthropology* 18:401–44.
- Zimmerman, Ekkart. 1983. *Political Violence, Crises and Revolutions*. Cambridge, Mass.: Schenkman.