

## HISTORY AND SOCIOLOGICAL IMAGINING

Charles TILLY

Sociology without history resembles a Hollywood set: great scenes, sometimes brilliantly painted, with nothing and nobody behind them. Seen only as the science of the present or, worse yet, of the timeless, sociology misses its vocation to fix causation in time. It thereby vitiates its vital influence on historical thinking, its influence as the study of social mechanisms operating continuously in specific times and places. Although after years of living in the borderland of the two disciplines little lectures on the complementarity of sociology and history burst from me as easily as bubbles escape from champagne, that pleasant cohabitation will not be my subject here. Instead, I want to advocate theoretically-informed historical inquiry as a solution to a major difficulty which social scientists, especially sociologists, frequently create for themselves.

What difficulty? Let us call it monadism. Monadism involves the adoption of three closely related assumptions: *first*, that the elementary units of social life are self-contained, self-directing monads, especially human individuals but also aggregates of individuals up to the level of something vaguely called a "society"; *second*, that regularities in the social world consist of structures, sequences, and directional processes of those monads that repeat themselves in essentially the same way time after time; *third*, that the central task of social science is therefore to create invariant models, one per structure, sequence, or directional process, match them to as many relevant cases as possible, then perfect each model in accordance with observed discrepancies from careful observation of the relevant cases.

advocating historical particularism or epistemological nihilism. Nor am I arguing that social life has no coherent recurrences or that it is fundamentally unknowable. I am advocating clearer reflection about ontology, about the character of the phenomena we purport to describe and explain.

History is not immune to monadism. Such reasoning appears as often in history as elsewhere in the social sciences, if only because singular models of structures, sequences, and processes infest the folk sociology on which historians so regularly draw without being self-conscious about their reasoning. Throughout the labyrinthine historical literature on political conflict in which I spend much of my own time wandering, for example, a limited number of competing models, each singular, recur in explanations of mass collective action. In caricature, we might call them misery models, madness models, and mobilization models. I confess to having contributed one or two of the latter to the literature myself; my most general representation of mobilizing actors postulates precisely that every person or group that acts collectively is responding in a similar way to an array of interests and opportunities (e.g. Tilly 1978, chapters 3 and 4). The maker of such a model characteristically presumes that every time large numbers of ordinary people band together and challenge authorities the same basic process of mobilization, collective action, and demobilization unfolds. Historical knowledge does not automatically eliminate that presumption.

Nevertheless, history provides its own antidotes to overdoses of singularity. For real history, carefully observed, does not fall into neat, recurrent chunks; it winds and snarls like a proliferating vine. What is more, in real history time and place make a difference to the way that ostensibly universal processes such as industrialization and secularization unfold; just as the flows of rivers, for all their common properties, depend intimately on the terrain through which they pass, and those terrains result in important part from previous flows of the selfsame rivers, the power of history means that social processes follow strong regularities yet do not repeat themselves; the regularities lie in causal mechanisms, not in recurrent structures or sequences.

Among analysts who are self-conscious about such matters, singular model reasoning has its own distinctive Method, recently codified by Charles Ragin as *The Comparative Method*. Ragin reports that he developed *The Method* out of his dissatisfaction with the application of multivariate statistical techniques to a number of problems that interested

Thus sociologists create models of social mobility in which characteristics of fathers cause characteristics of sons, with coefficients varying from one setting to another, all within the same basic structure of causation by human capital. In the same mode, urban sociologists formulate singular models of urban growth and decay within presumably self-contained cities based on the average experience of older capitalist cities, then cope with vastly contrary patterns in Third World cities by postulating a second urban species to which a quite separate model is supposed to apply. Likewise, we find specifications of the necessary and sufficient conditions for democracy, with little allowance for variation in time and space.

Following the same design, sociologists of revolution create unitary models of true revolutions as incidents in the lives of "societies", line up multiple cases of revolution, tug and haul to make those models fit each and every relevant case... then, not incidentally, spend much of their polemical time demonstrating that theorist X's model doesn't apply properly to revolution beta or gamma, an exercise that leads immediately to minor revisions of X's model. Similar models purport to explain crime, war, divorce, secularization, employment discrimination, racial conflict, suicide, homelessness, and dozens of other lugubrious phenomena sociologists have cheerily made their own. The models vary enormously in structure and scope, but have in common the presumption that at bottom the subject concerns a unitary phenomenon having a relatively invariant structure, sequence, or process, a phenomenon happening to some self-contained social unit or aggregate of them.

By no means all models in the social sciences conform to the singular plan. Some explicitly undertake explicitly to account for variation, as in Arthur Stinchcombe's brilliant old discussion of the influence on policing of property's spatial distribution (Stinchcombe 1963). Others represent recurrent and coherent causal mechanisms having wildly variant outcomes, as in Harrison White's extraordinary account of identity-formation (White 1992). Still others, and many of them, create representations of a single non-recurrent social structure or process, as in Immanuel Wallerstein's portrayal of the capitalist world-system. My polemic concerns only one manner of modeling in the social sciences, but a common one: postulating an essentially invariant structure or process in a self-contained social unit. I aim to make you wary of that common procedure, wary because in actual social life invariant structures and processes are rare or nonexistent. By no means, as you will see, am I

him (Ragin 1987: vii). As a distinctive procedure, he says, comparative analysis in general uses combinatorial logic to explain the characteristics of whole cases. The cases are most often "societies". In Ragin's variant, Boolean logic applied rigorously over a number of cases singles out those differences that can actually make a difference. The argument rests ultimately on John Stuart Mill's paired procedures: the method of agreement, the method of differences (Mill 1892: 221-234; Little 1991: 35-37). (Mill actually distinguished four experimental methods — agreement, difference, residues, and concomitant variation, but somehow his provocative treatments of the latter two have disappeared from social-scientific discussions of comparison.)

There is, alas, a catch. Mill himself pointed it out: For the pair of methods to be foolproof, the analyst must be able to specify, observe, and even manipulate all possible causes, a circumstance that lies beyond the reach of non-experimental social sciences. Hoping nevertheless to identify all the obvious candidates for causes of a given outcome, however, adepts of singular-model arguments commonly forge ahead with comparisons of cases that display the outcome with others that do not, searching for those other conditions that occur uniquely with the outcome. They habitually practice closest-case comparison. Their procedure follows the example of epidemiology in searching for the necessary conditions of a distinctive disease. Thus they conduct comparisons of whole countries to discern the special conditions that distinguish those experiencing major declines in fertility or great gains in per capita income from all others.

What's wrong with this standard *modus operandi*? Nothing much would be wrong with it if the social world did, indeed, consist of self-contained units, if it did, indeed, fall nicely into recurrent structures, sequences, and directional processes. John Stuart Mill, as a matter of fact, thought it did. He ended his discussion of Historical Method with this bright promise:

If the endeavours now making in all the more cultivated nations, and beginning to be made even in England (generally the last to adopt whatever does not originate with herself) for the construction of a Philosophy of History, shall be directed and controlled by those views of the nature of sociological evidence which I have (very briefly and imperfectly) attempted to characterize; they cannot fail to give birth to a sociological system widely removed from the vague and conjectural character of all former attempts, and worthy to take

its place, at last, among established sciences. When this time shall come, no important branch of human affairs will be any longer abandoned to empiricism and unscientific surmise: the circle of human knowledge will be complete, and it can only thereafter receive further enlargement by perpetual expansion from within (Mill 1892: 565).

(Francophiles and sociological chauvinists will be happy to know that Mill's chief example of Historical Method's proper application was Auguste Comte, coiner of the word "sociology".) If we should have learned anything from the fifteen decades of systematic social science that separates us from John Stuart Mill, however, it is that social life doesn't work that way: Boundaries of social units are porous, structures keep changing, sequences never quite repeat themselves, ostensibly directional processes stop, reverse, or split, what has happened before affects the character of the next structure, sequence, or process.

Taken as entire events, neither wars, occupational careers, spurts of urban growth, racial conflicts nor any of the other social phenomena to which sociologists have commonly applied the suspect *modus operandi* display enough invariance to make such models useful. The repetitions within them, furthermore, are superficial, at the level of the proximate causes that (given extensive separation of home from work, employment in large workplaces under time-discipline, and reliance on mechanical transportation) produce rush-hour transport peaks twice a day (but not on Sundays or holidays); the knowledge involved is not trivial, especially for transportation planners and traffic cops, but it is superficial and highly vulnerable to changes in boundary conditions. Try applying standard American models of daily traffic flow to today's Sarajevo or Mogadishu!

In such circumstances, the search for necessary and sufficient conditions becomes a wild goose chase. Discovering these difficulties, optimists who persist in holding to singular models conclude that the models need more refinement, pessimists that the world is too complicated for the location of regularities, skeptics that social life is unknowable in any reliable, systematic sense, pragmatists that for the time being we need more than one model, yet far fewer than one model per observation.

We have, however, a more hopeful alternative. We could begin to see that the elementary units of social life are neither individuals nor "societies" nor groups but interactions among social locations. We could

Karl Marx, but the rules remain the same: Explicate the model; single out one or two elements for criticism; correct those elements; glue the updated model back together; congratulate yourself; publish the result.) But I wanted to connect the analysis of revolution to those of state formation and collective action in which I had been dabbling for some years.

At length I realized that I was yoking a lion and a hippopotamus together for plowing; I was starting mayhem rather than the neat cultivation of a field. Why? Because my favored models of state formation and collective action concerned continuous variation rather than recurrent singular phenomena. Although I had perpetrated singular models of both earlier in my career – we remain creatures of our educations so long! – through protracted struggles with historical material I had first rejected one-track models, then begun to formulate accounts of variable trajectories by searching for deep causal mechanisms. Meanwhile, the available models of revolution, at least in their most general forms, all purported to specify the necessary and sufficient conditions for revolution, conceived of as a relatively invariant bundle of structures and processes. The misfit soon became obvious.

Attention: I don't claim to have found the Deep Causes of all changes in the character of states or of all variations in collective action. I only claim to have recognized that the regularities lie in the generating mechanisms rather than in the recurrence of whole structures, the repetition of whole sequences, the reappearance of the same unilinear processes. Such a recognition does not preclude typologizing states or collective actions, mapping sequences of conflict, or even tracing long processes of transformation, but it does entail recognizing that those operations do not yield explanations. They simply specify what is to be explained.

In the case of revolution, then, I found that I had to rethink the phenomenon as one zone in a much larger field of variation including many political interactions no one would label revolutionary, then search for clues as to why some peoples, places, and eras spent a lot of their time in that zone while others barely approached it. My first crude device for doing so consisted of separating analytically the conditions for revolutionary situations from those for revolutionary outcomes – revolutionary situations consisting of open splits within politics, revolutionary outcomes consisting of substantial transfers of power over

recognize that great social regularities do not occur at the level of whole structures, full sequences, or total processes but in the detailed social mechanisms that generate structures, sequences, and processes. Whole networks do not resemble each other in lawful ways, but the principles by which networks form and change do. Wars do not follow standard sequences or burst out in only one set of circumstances, but they conform to very strong principles of logistics, organization, and strategy. We could, in short, rediscover history, rediscover the interplay among causal mechanisms, idiosyncratic events, and powerful contingencies. In this sense, we could become historicists.

To rediscover history, however, is not to fit singular models, huge or modest, to great slabs of time and space. Arthur Stinchcombe ends his *Theoretical Methods in Social History* with a ringing declaration which, regrettably, readers frequently misunderstand: that "it is the details that theories in history have to grasp if they are to be any good" (Stinchcombe 1978: 124). Contrary to a superficial reading, Stinchcombe is not recommending barefoot empiricism, but examination by analogy with other historical situations of the actual mechanisms that generate social structures, sequences, and processes. Properly conducted, historical research has the great virtue of requiring the investigator to locate social actions in time and space, to specify their interrelations, to search for their causes in concrete circumstances. It also leads, as we shall see, to recognizing the tight interdependence and instant mutual modification of culture (conceived as shared understandings and their objectifications) and social structure (conceived of as durable relations among persons).

Three recent inquiries of my own made me aware of the difficulty, which had bothered me for years without my being able to articulate it well. The topics differed greatly: European revolutions from 1492 to the present, American immigration during the 19th and 20th centuries, British popular politics between the 1750s and the 1830s.

The first inquiry was a book on European revolutions from 1492 to 1992. On agreeing to write the book, I had implicitly assumed that it would be easy, almost a potboiler, an exercise in locating the best model of revolution around – perhaps Skocpol's, Kimmel's, or Goldstone's – polishing it up a bit for my own purposes, then fitting it to a number of European revolutions: the old game of Improving Karl Marx that self-appointed theorists among us all play so confidently. (These days the thinker Improved is more likely to be Max Weber, Anthony Giddens, Jürgen Habermas or, heaven help us, even Talcott Parsons rather than

The whole story of that transition would take too long to tell, and would require too many allowances for variations among, say, Iberia, the Balkans, and the British Isles. But one cluster of factors nicely illustrates my general point: the tight interdependence in early modern European states among the organization of great families, the existence of huge patron-client chains attaching officials, servitors, and tenants to those great families, the embedding of military force in those patron-client chains, and the adoption by great families of outmarriage strategies accomplishing three purposes: first, giving their heirs claims on aristocratic and royal successions elsewhere, providing local members of the family (including emperors or kings) with some call on military assistance from grandees or rulers outside their own countries, and arranging another place to survive comfortably if life became too dangerous at home.

Together, these circumstances meant that almost every royal succession constituted an opportunity, or at least a hope, for some rival to the most obvious heir, often a foreigner in whom another royal family also had an interest; where the inheritance was unclear or the heir incompetent, the opportunity became a strong incentive to employ autonomous military force, and enlist aggrieved popular support, for a dynastic coup. When protestant lords invited fellow-protestant William of Orange to England in 1688 to displace catholic king James II, they did not simply call on an experienced statesman from a distinguished family; they called on a grandson of Charles I and son-in-law of James himself. An explanation of the Glorious Revolution requires much more than knowledge of William's family background. Nevertheless, no one will understand it and other revolutions of the time without exploring the mechanisms by which great families attached themselves to each other and to regimes. Such an exploration is deeply historicist. In it, structure and culture interact.

Let me underline what this means, and what it does *not* mean. Considered as wholes, neither lineages nor revolutions had recurrent structures besides those they shared by definition. Singular models of lineages and revolutions would serve us badly. On the contrary, the regularities lie in the ways that kinship ties affected the formation of alliances, the probability of war, and the claims to succession to supreme positions in dynastic states, which in turn affected the probability and character of revolution. These are not invariant structures or processes, but wide-ranging causal mechanisms whose combinations produced the actual unique histories we observe.

states. I argue that the two sets of conditions vary and change in partial independence of each other.

My second crude device was to treat each of those conditions as a continuum, for example from no split whatsoever in a polity to a split putting every political actor on one side or another. My third was to treat major changes in the organization of states, state systems, and armed force as determinants of the positions of different states and polities on those continua. My answers surely contain defects, but they illustrate another way of thinking about revolution than as a one-track phenomenon. They represent a historicizing of the problem.

Any vendor of standard models of revolution, for example, will have trouble selling them to specialists in early modern European history who go beyond fitting their appliances to the English revolution of 1640-1660, the Fronde, and the revolt of Catalonia to asking why so many more forcible attempts to seize state power - revolutionary situations, roughly speaking - occurred, and why so many of them actually succeeded. Close study of the circumstances of those centuries' revolutionary situations does not produce a new General Model of Early Modern Revolutions. It does, however, reveal the grounding of revolutionary situations in prevailing conditions of indirect rule, military expansion, and dynastic competition.

Take the factor of dynastic competition. In Muscovy, then in the Russian empire that grew with Muscovy as its kernel, for three centuries after 1492 every time a tsar died without an adult, militarily competent son or brother to succeed him a serious struggle for the throne ensued, often with wide popular support for one faction or another. During the 17th and 18th centuries the serious claimants at different times even included thirty or forty men pretending to be tsars or heirs whom everyone else had believed dead - often murdered at the behest of the late ruler. Cossack Emelian Pugachev, who led the great peasant-Cossack rebellion of 1773-1775, claimed to be the deposed and dead tsar Peter III. A number of the claimants actually made it to the throne: Boris Gudonov, his successor the false Dmitry, Ivan V, his brother Peter the Great himself all became tsar irregularly, outside the standard inheritance rules, through the use of force. Russia was no more extreme in this regard than Poland, Hungary, and a good many other early modern states. Yet by the 19th century militarily contested successions had become rare in European monarchies.

The second inquiry concerns inequality and American immigration. As Ewa Morawska (1990) has well documented, recent work on immigration has challenged the two dominant models of earlier generations: human capital and assimilation. Human capital models escape my strictures somewhat by deliberately accounting for differential success as a function of variable resources, broadly defined; they deserve suspicion, nevertheless, for their reliance on a singular model of market-mediated success. Assimilation models clearly qualify as singular in so far as they posit only one invariant path into American life, the chief variation being the speed at which different groups travel that path. As Morawska says, an anti-singular historicist view helps make sense of the connections between migration and durable forms of inequality, including those forms people organize as ethnicity – as structured differences according to imputed national or racial origin. In thinking about American immigration as a whole, and about my current collaborative studies of nineteenth-century French silk workers in Paterson, New Jersey, and of twentieth-century Italian peasants in Mamaroneck, New York, I find it useful to ask how the social organization of migration constrained the subsequent opportunities of different groups of migrants and their descendants.

In the case of Paterson, Florence Baptiste and I are trying to find out how textile workers from Lyon and its vicinity entered the expanding silk industry of Paterson after 1860, as well as what impact those circumstances had on their experience, and that of their children, in the American labor force. As for Mamaroneck, Philippe Videlier and I are attempting to compare post-1900 migrations from a few villages in the Frosinone, near Rome, to the Lyon metropolitan area, to Mamaroneck and nearby towns, and perhaps eventually to Buenos Aires, São Paulo, and Toronto as well, in order to see how differently the survivors of those migrations turned out at their various destinations.

How well immigrants do in a new country, and whether they return to the old, depends mainly on five factors: the extent to which they integrate on arrival into networks that embrace a wide range of employment opportunities; the opportunities for individual income with which their networks give them contact, especially at the start; the opportunities for collective capital accumulation at the destination; the degree of obligation to support persons and enterprises in the place of origin; and the relative opportunities for reinvestment of accumulated capital at the origin and the destination. On the whole, the more the migrant group or its subdivisions

serves as an accumulator of capital, the greater the incentive to pass jobs through kin and paesani. Thus durable inequality among immigrant groups and their descendants depends on the initial organization of migration and its capacity for accumulation of collectively-available capital. While it sounds strange to put warm matters so coldly, immigrants and their descendants actually know these principles well; their stories of connections, favors, and ethnic differences reek of them.

Networks transformed or created by migration create and maintain inequality. Members of immigrant groups often exploited each other as they would not have dared to exploit the native-born. Every act of inclusion, furthermore, also excludes. North American immigration produced a remarkable specialization of work by origin, although the precise specializations varied from one locality and migrant stream to another. The characteristic story of Mamaroneck is the present domination of landscape gardening and related fields by Italian immigrants and their heirs, that of Paterson French, British, German, and Italian workers from well-defined industrial locations entering specific branches of Paterson's industry.

Any student of migration can tell similar tales of occupational specialization by regional or national origin. The actual tales refute grand stage schemes of immigration, illustrate the combination of bounded contingency with constraint in social life, and show us powerful causes working consistently as links among events. Generalized, that observation makes my case against monadic ontologies and singular models, for historicism concentrating on the discovery of mechanisms that generate social structures, sequences, and processes. Again, culture and structure interact.

A third area of research that made me think about these topics concerns changes in the forms of collective contention – for example, why and how sit-ins and similar deliberate occupations of contested spaces rise and fall. For the shared delusions of collective-behavior theorists, sociologists of the 1960s and 1970s generally substituted models of collective rational action: public choice, resource mobilization, political process, and so on. In so doing, however, they (perhaps I should say "we") stuck unwittingly to monadism, assuming that the main problems were a) to explain the behavior of one coherent actor (individual or collective) at a time, and b) to identify a single model of collective action that, with no more than nudges of a parameter or two, accounted in principle for all instances. In the study of social movements, for

example, this reformulation rejected earlier portrayals of prohibitionism or feminism as irrational reactions to the stress of social change, but retained the assumption that the social movement was a kind of self-contained group whose behavior could be explained by the group's social situation. Similar, Mancur Olson's injection of collective-goods models into the analysis of what sociologists previously called collective behavior (Olson 1965) sent sociologists scurrying for alternative singular models that would accommodate identity, loyalty, and self-satisfaction (see Cohen 1985, Gamson 1990).

Let me spare you a detailed critique of standard models for social movements and collective action. Suffice it to say that monadic analyses of contention ignored the strategic interaction among challengers, competitors, and sometime allies that pervades real episodes of contention. (As participants and benevolent observers of social movements, many formulators of monadic models had ample practical awareness of strategic interaction, but failed to draw the appropriate theoretical conclusions from their own experiences.) A combination of influences tipped the balance toward interaction: the infiltration of game-theoretic reasoning from economics and political science; the creation of large catalogs of events as alternatives to the treatment of one group, movement, or action at a time; above all, the historicization of polemology (as francophones call the systematic study of conflict).

In this setting, historicization meant installing time and place as major determinants of contention's character rather than as proxies for other more elusive variables such as modernization or level of grievance. To historicize the study of contention meant recognizing that collective claim-making entails the simultaneous use and recasting of relations, including shared understandings, among local actors. It meant seeing that each locality and each interacting set of claimants, both challengers and authorities, accumulates its own particular experience, memory, understanding, and practices, and accumulation that strongly constrains current contention. My own formulation of these insights adopts the theatrical language of *repertoires*; contentious actors perform in dramas in which they already know their approximate parts, during which they nevertheless improvise constantly, and of which the exact outcomes remain uncertain.

In this formulation, potential actors choose strategically among available performances, engage other actors, including objects of their claims, in those performances, and improvise their way to some

conclusion. The conception is at once deeply interactive — that is, structural — and deeply cultural. It reeks of culture, as Arthur Stinchcombe has pointed out, in insisting that shared understanding and their objectifications constrain social interaction (Stinchcombe 1987).

My current research on the subject uses catalogs of British "contentious gatherings" between 1758 and 1834 to examine how claim-making changed during a period that brought Great Britain the demise of Rough Music, collective machine-breaking, invasions of enclosed fields, and many related forms of interaction, as well as the rise of public meetings, demonstrations, petition drives, popular associations, firm-by-firm strikes and more now-familiar forms of struggle.

A contentious gathering, for the purposes of this study, is an occasion on which ten or more people gathered in a publicly accessible place and visibly made claims which, if realized, would bear on the interests of at least one person outside their number. The main machine-readable catalog provides detailed descriptions of 8,088 contentious gatherings that occurred in Southeastern England during thirteen scattered years from 1758 to 1820 or anywhere in Great Britain during the seven years from 1828 through 1834. Among other things, my group is analyzing the events in that catalog and complementary evidence to determine whether a strong version of the repertoire model actually holds up to close scrutiny. We are unquestionably seeing profound changes in the texture of British contention, as seizures of grain, invasions of fields, mocking ceremonies, and related forms give way to processions, demonstrations, petition drives, and their kin. The changes pivot on the years of war with revolutionary and Napoleonic France, and bear plausible relationships to the transformations of the state and economy during the war years. That much verifies at least a weak version of the metaphor.

For stronger versions, we must look at innovation and variation within and among contentious gatherings. We think we are finding evidence, for example, of parliament's increased salience as an object of contentious claims and of the role played by public meetings, local assemblies, and popular associations in that shift. We think we can trace the influence of innovators such as John Wilkes, Lord George Gordon, Francis Place, and Daniel O'Connell on cumulative shifts in contentious repertoires. We have some grounds for claiming that collective actors constantly innovate in small ways, and do so at a faster pace when political opportunities are changing rapidly, but that innovations in the

forms of contention only stick when associated with visible success for one actor or another. But many questions remain open.

I won't bore you with other results, technical details, and historical problems. I am trying here to illustrate how historical thinking, properly conducted, combats monadism and helps reveal the tight interdependence of culture and social structure. For in the analysis of British contentious repertoires, as in the study of revolutions and of immigrant itineraries, we find the cumulative intersection of history, social ties, and shared understandings.

What, then, are these elusive causal mechanisms I have identified as the true locus of regularities in social life? In the case of revolutions, they consist of rapid and visible diminutions of state power, splits in control over the major means of coercion, formation of anti-regime coalitions, and other political shifts that singly neither guarantee revolution nor constitute parts of its definition. In the case of immigration, the crucial causal mechanisms consist of the transmission of information about opportunities within existing ties of kinship or neighborhood, the pooling of capital or credit, the hoarding of access to remunerative work, housing, and social life, the remittance of money and other resources to the place of origin, and other collective actions that shape the structure of opportunities, rights, and obligations; all of these operate outside of immigration, indeed quite outside of residential mobility of any kind. In the case of changes in contentious repertoires, we must look for causes in the transformation of political opportunities by innovations associated with successful claim-making, in alterations — incremental or sudden — of various political institutions' capacity to deliver rewards or punishments, in the creation or rupture of links among potential collective actors, and in similar mutations of shared incentives and organizational resources. If revolutions, immigration, and changing repertoires defy singular models, that is not because they know no regularities. It is because their regularities do not lie in recurrent structures or sequences but in powerful causal mechanisms that in different combinations produce both those phenomena and a host of others.

## NOTE

A few passages in this paper come from "Cities and Immigration in North America," Working Paper 88, Center for Studies of Social Change, New School for Social Research, September 1989, which contains much more extensive discussions both of historicism and of migration.

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*Observation réciproque*