

PART I

Introduction

CHAPTER 1

METHOD AND EXPLANATION

Back in the Dark Ages more than half a century ago, I was a graduate student at Harvard in the composite field called Social Relations. In those distant days, Method meant statistical analysis, and Explanation meant one of three things: 1) location of a phenomenon within some large social structure (at the limit of a society or civilization), 2) discovery of strong correlations between two variables, or (if you were lucky) 3) identification of necessary and sufficient conditions for some important phenomenon. I still retain a vivid image of how the empirical wizard Samuel Stouffer taught us youngsters method and explanation. Imagine a dingy basement room containing a counter-sorter: a machine with an input tray for Hollerith cards (aka IBM cards) at one end, and a dozen trays among which the cards would sort when the operator pushed a button. Stouffer stood before the counter-sorter with a pack of cards in his hand, ash dangling precariously from the eternal cigarette between his lips, and muttered to us, “OK, now let’s try breaking on religion.”

Here was the idea: By means of holes punched in designated columns, each card encoded the responses of a single individual to a national survey on civil liberties. If the punches in the column for religion caused the cards to sort very unevenly on another column recording attitudes toward civil liberties, the uneven distribution told us that we were on our way to discovering that a respondent’s religion caused, at least in part, his or her attitudes toward civil liberties. Of course we would have to run the cards through the machine again to control for such possibly confounding variables as gender, class, and region. Still, a David Hume, exhumed, might have smiled as we followed a simple method in a search for constant conjunction, and called it explanation. Later I learned to follow the same logic on a wired tabulator (I even learned to wire it) and then on a mainframe computer (I even learned to program it, if not very expertly), but the logic long remained the same.

Yet, while still a graduate student I also encountered historical analysis and realized that the search for constant conjunction and correlation had two serious defects: it ignored transformative processes and it promoted premature simplification. My own research concerned responses to the Revolution of 1789–1794 in rural France (Tilly 1964). Once I entered French archives and left textbook schemes behind, I had no

choice but to analyze transformative processes and to keep a grip on complexity at least until I could see what features of those processes required explanation.

Explaining variable rural responses to the Revolution, it turned out, required close attention to dynamic processes: extraction, mobilization, repression, and polarization. The conventional simplification—traditional peasants versus modernizing urbanites—completely obscured the changing alignments that occurred between 1789 and 1794. It fostered a premature search for the presumably unchanging fundamental motives of the major parties in the rural revolution (Tilly 1961, 1963). I never forgot the lesson.

Although by no means an intellectual autobiography, this book displays the lifelong impact of that early encounter with complex historical processes. In reviewing my own work as a background for producing the book, I was surprised to discover that about a quarter of the roughly 700 scholarly items I have ever published deal primarily with method and explanation. That happened, I now see, because my peculiar situation at the edge of history and social science meant that people on one side or the other of the boundary kept asking me to interpret the mysterious ways of researchers across the boundary from them (see, e.g., Landes and Tilly 1971, Tilly 1981, 1985, 1997a).

History matters to social science because history matters to social processes: when and where a social process unfolds affects how it unfolds. All social processes incorporate locally available cultural materials such as language, social categories, and locally shared beliefs. Processes therefore vary as a function of historically determined local cultural accumulations. Even if urbanization has universal properties, for example, how cities grow and gain importance in any particular setting depends significantly on earlier urban experience in the same setting. Again, once a process (e.g., immigration) has occurred and acquired a name, both the name and one or more representations of the process become available as signals, models, or threats for later actors. National authorities, for instance, often try to stave off what they see as the evil consequences of previous migration waves by restricting the current wave (Zolberg 2006). In these and other ways, what has happened in the past shapes what happens now.

This collection of papers presents tentative conclusions from a lifetime effort to develop methods and explanations suitable for complex social processes and to place them in appropriate historical perspectives. Suitable methods and explanations need not be complex themselves, but they must somehow capture the ebb and flow of dynamic social interactions. As chapter 3 (“Observations of Social Processes and their Formal Representations”) explains in detail, methods and explanations usually work better if they involve formal representation of the elements within the processes under study—not just convincing narratives, but explicit matching of concepts and observations with the portions of the processes that require explanation. Many of the book’s chapters undertake just that sort of matching.

The book inevitably draws disproportionately on the social processes I have studied most closely: revolution, contentious politics, state formation, migration, urbanization, generation of inequality, democratization, capitalist transformation,

and population change. As a result, it has more to say about the large scale than the small scale, more about collective than individual experience, more about Europe and America than elsewhere, more about the last few hundred years of human history than before. Nevertheless, I am confident that its general approaches to method and explanation apply well outside its empirical range.

As for methods, I have occasionally done sample surveys, intensive interviews, street-corner observations, and analyses of statistics collected by public agencies. But the bulk of my systematic empirical work has consisted of constructing uniform event catalogs from published or archival sources, then analyzing connections, sequences, and settings over substantial numbers of events. (This book's chapter 4—"Event Catalogs as Theories"—lays out the logic of such a method.) Construction of event catalogs involves high-risk adventure; catalogs absorb a great deal of preliminary effort, and only when the catalog becomes extensive does an analyst know whether it reveals any patterns worth the investment of energy it entails. Still it offers important compensations: the collection of data event by event absorbs an investigator in the richness of the phenomenon under investigation from the start. When the inquiry goes well, furthermore, it provides indications of cause and effect that even participants in the events could hardly grasp.

In what follows, however, you will not plod through a manual for construction of event catalogs. Nor will you learn much about specific techniques for collection, measurement, or analysis of evidence concerning social processes. Instead, the book deals much more generally with the acquisition of reliable knowledge concerning social processes. The acquisition of reliable knowledge requires choices in three fundamental areas: epistemology, ontology, and logics of explanation. Let us look at each of them in turn.

Epistemology

Smart analysts avoid two opposite extremes: naïve inductivism and radical subjectivism. At one extreme, debris of meaningless observations, however glittering. At the other, no means of observing the world, much less of comparing objects in it. At either extreme, no reliable systematic knowledge and therefore no possible social science. At the inductive extreme, we face the call for nothing but the facts. Yet, as a generation of philosophers has established, the situation of discovery always shapes what observers and analysts recognize as facts, not to mention the significance of one fact or another.

At the subjective extreme, no one can acquire reliable knowledge of anything but her own consciousness—regardless of whether real phenomena actually exist outside the range of her senses. Before sinking into the subjectivist swamp, however, sentient humans always have the option of forming hypotheses about what lies outside them, then checking those hypotheses by such means as kicking walls to see if it hurts or insulting other people to see whether they kick back.

In an age of postmodern skepticism any such assertion readily gives rise to the riposte "But it's all a social construction." Race, class, gender, religion, and similar

categories, goes the argument, are all illusions resulting from mutual persuasion (Jung 2006). At the subjectivist extreme, the position again denies the possibility of reliable knowledge on one of two grounds: 1) the already familiar claim that no one can leap the boundaries of individual awareness or 2) the less familiar, but ultimately more threatening, claim that all knowledge resides within particular cultures and languages and is therefore at best comparable within an epistemic community. If either position holds, a transcultural social science becomes impossible.

To the extreme positions, this book replies, "Let's see whether we can identify and verify unexpected regularities that cross the boundaries of individuals and cultures." To the more general argument of social construction, it responds instead, "Yes, it's social construction all the way down. Social interaction generates what people experience as race, class, gender, or religion. The relevant conventions vary across periods, places, and population. So let's see how social construction works, then build systematic knowledge of social construction into superior analyses of social processes" (Tilly 2005a).

Fortunately, many intermediate epistemological positions exist. In an argument that should appeal to social scientists, philosopher Louise Antony has argued for truth as relative correspondence between a social environment and the competing theories adopted by different groups of analysts:

So let it be supposed that scientists' commitment to their theories is accounted for, in causal/historical terms, by a variety of factors, including non-rational, or even irrational factors like loyalty to colleagues or desire for fame and fortune. And let it be supposed, furthermore, that some biasing factors are ubiquitous and ineliminable. If [Thomas] Kuhn and [John Stuart] Mill are right, the hope that theories that result from these unholy mixes of motivations will approximate truth, lies in the constitution of the social environment. Objectivity, in other words, is not secured by the scrupulousness of individual scientists, but rather by the effects of competition among the ideas of contending groups of theorists. (Antony 2006: 69)

No social scientist can declare flatly, "I saw it, so what I say is true." But it is perfectly feasible to say that a theory conforms more closely to what we can jointly observe of the social environment than the next best available approximation. In such a view, relative truth is possible, but always remains subject to revision as a better approximation comes along. That position informs the essays in this volume. "Linkers, Diggers, and Glossers in Social Analysis" (chapter 10) deals most fully with epistemological problems.

Ontology

In this volume's essays, ontology occupies a far larger place than epistemology. The greater emphasis on ontology results from my concern that social analysts frequently arrive at false conclusions by assuming the existence of fundamental entities such

as social systems without doing the work required to establish the presence of those entities. (The critique of the great historical sociologist S.N. Eisenstadt's work on civilizations and societies in chapter 5, "Iron City Blues," takes Eisenstadt to task for just such assumptions.)

Within social science, major ontological choices concern the sorts of social entities whose coherent existence analysts can reasonably assume. Major alternatives include methodological individualism, phenomenological individualism, holism, and relational realism. *Methodological individualism* insists on decision-making human individuals as the basic or unique social reality. It not only focuses on persons, one at a time, but imputes to each person a set of intentions that cause the person's behavior. In economists' versions of methodological individualism, the person in question contains a utility schedule and a set of assets, which interact to generate choices within well-defined constraints. In every such analysis, to be sure, figures a market-like allocative structure that operates externally to the choice-making individual—but it is astonishing how rarely methodological individualists examine by what means those allocative structures actually do their work.

The less familiar term *phenomenological individualism* refers to the doctrine that individual consciousness is the primary or exclusive site of social life. Unlike methodological individualism, it makes no assumption that individuals are rational decision makers, much less that something like a market adjudicates their decisions. Phenomenological individualism veers into solipsism when its adherents adopt the epistemologically extreme position that adjacent minds have no access to each other's contents, therefore no observer can escape the prison of her own awareness. Even short of that analytically self-destructive position, phenomenological individualists tend to regard states of body and mind—impulses, reflexes, desires, ideas, or programs—as the chief motors of social action. In principle, they therefore have two ways to account for large-scale political structures and processes: 1) as summed individual responses to similar situations; 2) as distributions and/or connections among individual actions.

In the first case, social scientists and historians sometimes constitute collective actors consisting of all the individuals within a category such as peasant or woman. In the second case, they take a leaf from those social scientists who see national political life as a meeting place, synthesis, and outcome of that shifting distribution of attitudes we call public opinion or from the social psychologists who see individual X's action as providing a stimulus for individual Y's action. Even there, they hold to the conception of human consciousness as the basic site of social life.

Holism is the doctrine that social structures have their own self-sustaining logics. In its extreme form—once quite common in social science but now unfashionable—a whole civilization, society, or culture undergoes a life of its own. Less extreme versions attribute self-reproducing powers to major institutions; treat certain segments of society as subordinating the rest to their interests; represent dominant mentalities, traditions, values, or cultural forms as regulators of social life; or assign inherent self-reproducing logics to industrialism, capitalism, feudalism, and other distinguishable varieties of social organization.

Relational realism, the doctrine that transactions, interactions, social ties, and conversations constitute the central stuff of social life, once predominated in social science. Classical economists Karl Marx, Max Weber, and Georg Simmel all emphasized social relations, regarding both individuals and complex social structures as products of regularities in social relations. During the twentieth century, however, relational realism lost much of its ground to individualism and holism. Only in American pragmatism, various versions of network analysis, and some corners of organizational or labor economics did it prevail continuously.

With the retreat of structural Marxism and the advance of various institutionalisms, however, relational reasoning has once again started to prosper. Relational realism concentrates on connections among people and social sites—for example, households, neighborhoods, associations, firms, or organized occupations. It sees those connections as concatenating, aggregating, and disaggregating readily, forming organizational structures at the same time as they shape individual behavior. Relational analysts follow flows of communication, patron–client chains, employment networks, conversational connections, and power relations from the small scale to the large and back. For example, interactions between interpersonal networks of trust and national political institutions turn out to play crucial roles in democratization and de-democratization (Tilly 2005b, 2007).

Intellectual synthesizers can, of course, create combinations of the four basic ontologies. A standard combination of phenomenological individualism and holism portrays a person in confrontation with society, each of the elements and their very confrontation having its own laws. Methodological individualists usually assume the presence of a self-regulating market or some other allocative institution. Individualists vary in how much they allow for emergents—structures that result from individual actions but once in existence exert independent effects on individual actions, much as students enter a lecture hall one by one, only to see the audience’s distribution through the hall affect both the lecturer’s performance and their own reactions to it. Relational analysts commonly allow for partly autonomous individual processes as well as strong effects on social interaction by such collectively created structures as social categories and centralized organizations. Nevertheless, the four ontologies lead to rather different accounts of social processes.

They also suggest distinctive starting points for analysis. Methodological individualists can treat social ties as products of individual calculation, but above all they must specify relevant individual actors before launching their analyses. Phenomenological individualists likewise give priority to individuals, with the double qualifications that a) their individuals are sites of consciousness rather than of calculating intentions, and b) they frequently move rapidly to shared states of awareness, at the limit attributing shared orientations to all members of a population. A holist may eventually work her way to the individuals that live within a given system or the social relations that connect individuals with the system, but her starting point is likely to be some observation of the system as a whole.

Relational realists may begin with existing social ties, but to be consistent and effective they should actually start with transactions among social sites, then watch when and how some transactions bundle into more durable, substantial, and/or consequential relations among sites. In the following chapters, you will encounter a thoroughgoing relational realism. The essay called “*Terror, Terrorism, Terrorists*” (chapter 9), for example, counters the frequent portrayal of the terrorist as a particular type of person with an analysis of terror as a strategic relation among social sites.

Logics of Explanation

Epistemologies and ontologies provide the (often invisible) philosophical grounding of social analysis. Epistemologies and ontologies limit what sorts of explanations are logically possible—a holist can’t appeal to individual motivations as her ultimate causes. But they do not dictate logics of explanation by themselves. Social scientists and historians have experimented with a number of different competing logics for the explanation of social processes. They include:

- a) proposal of covering laws for complex structures and processes; explanation here consists of subjecting robust empirical generalizations to higher and higher level generalizations, the most general of all standing as laws (see chapters 6 and 7)
- b) specification of necessary and sufficient conditions for concrete instances of the same complex structures and processes
- c) variable analyses in which statistical analysis shows the extent to which one or more predictor variables (often called “independent variables”) account statistically for variation in an outcome variable (often called the “dependent variable”)
- d) location of structures and processes within larger systems they supposedly serve or express, for example through the claim that element X serves function Y within system Z
- e) stage models in which placement within an invariant sequence accounts for the episode at hand, for example the stages of revolution or of economic growth
- f) identification of individual or group dispositions just before the point of action as causes of that action—propensity or disposition accounts
- g) reduction of complex episodes, or certain features of those episodes, to their component mechanisms and processes

In the interest of exhortation, chapter 2—“*Systems, Dispositions, and Transactions in Social Analysis*”—reduces modes of explanation to the three of its title, which means essentially d), f), and g) on this list. Chapter 12 (“*Historical Analysis of Political Processes*”), in its turn, enumerates four classes of explanations: covering law-, propensity-, system-, and mechanism-based. But a full accounting of the last half century’s social science and history includes all seven logics on the list.

Most of these explanatory modes, as it happens, exclude history as a significant shaper of social processes. History *can*, of course, figure in any of these explanatory conceptions. In a covering-law account, for example, one can incorporate history as a scope condition (e.g., prior to the Chinese invention of gunpowder, war conformed to generalization X); or as an abstract variable (e.g., time elapsed or distance covered since the beginning of an episode; see Roehner and Syme 2002). Nevertheless, covering-law, necessary-sufficient condition, and system accounts generally resist history as they deny the influence of particular times and places. Stage models do incorporate time, but they usually run roughshod over the actual complexities of historical social processes. Propensity accounts respond to history ambivalently, since in the version represented by rational choice they depend on transhistorical rules of decision making, while in the versions represented by cultural and phenomenological reductionism they treat history as infinitely particular.

Mechanism-process accounts, in contrast, positively welcome history, because their explanatory program couples a search for mechanisms of very general scope with arguments that initial conditions, sequences, and combinations of mechanisms concatenate into processes having explicable but variable overall outcomes. Mechanism-process accounts reject covering-law regularities for large structures such as international systems and for vast sequences such as democratization. Instead, they lend themselves to “local theory,” in which the explanatory mechanisms and processes operate quite broadly but combine locally as a function of initial conditions and adjacent processes to produce distinctive trajectories and outcomes (McAdam, Tarrow, and Tilly 2001, Tilly 2001).

Mechanisms compound into processes: combinations and sequences of mechanisms that produce some specified outcome at a larger scale than any single mechanism. Within contentious politics, analysts commonly invoke such processes as escalation, framing, identity shift, and scale shift (Tilly and Tarrow 2006). But they rarely identify the component mechanisms, much less their combinations and sequences. Nevertheless, in social science as a whole, a substantial intellectual movement has formed to adopt mechanism- and process-based explanations as complements to variable-based explanations, or even as substitutes for them.

As is always the case in new movements, competing definitions and practical proposals for the analysis of mechanisms and processes have proliferated wildly (Brady and Collier 2004, Bunge 1997, 1998, 2004, Campbell 2005, Cherkaoui 2005, Elster 1999, George and Bennett 2005, Goodin and Tilly 2006, Hedström 2005, Hedström and Swedberg 1998, Little 1991, 1998, McAdam 2003, Norkus 2005, Pickel 2006, Stern, Dietz, Dolšák, Ostrom and Stonich 2002, Stinchcombe 2005, Tilly 2000, 2001, 2004). No conceptual, theoretical, or methodological consensus has so far emerged. This book will not restore unity to the field. Instead, it concentrates on repairing another defect of the existing literature: a severe shortage of concrete demonstrations identifying coherent mechanisms and processes, with special emphasis on converting observations into measurements (see again chapter 3, “Observations of Social Processes and their Formal Representations”).

Event Catalogs in Contentious Politics

Over a long career, two theoretically charged questions have motivated more of my research than any others. First, how do repertoires of contention form and change? Translated, the question refers to how people's forms of public, collective claim making—think street demonstrations, peasant uprisings, and military seizures of power—come into being, cluster, and evolve? Second, in Western countries since 1600 or so, how did the mutations of capitalism and national states shape and reshape those repertoires? My largest inquiries into the two questions have concerned France and Great Britain (Tilly 1986, 1995). Let's take up the British study and its background as a case in point. It displays a pragmatic epistemology, a relational realist ontology, and a mechanism-process explanatory logic at work.

Methodologically speaking, the British inquiry grows out of a strong tradition of event analysis among students of contentious politics. European and American governments began collecting official reports on work stoppages during the later nineteenth century. From that point on, statistically minded analysts began conducting quantitative analyses of industrial conflict based on government data (Franzosi 1989, 1995, Haimson and Tilly 1989, Korpi and Shalev 1979, 1980, Shorter and Tilly 1974). Not until after World War II, however, did analysts dealing with other forms of struggle start constructing parallel data sets for revolutions, coups d'état, international wars, civil wars, and domestic collective violence (Cioffi-Revilla 1990; Rucht, Koopmans, and Neidhardt 1999, Rule and Tilly 1965, Sarkees, Wayman, and Singer 2003, Tillema 1991, Tilly 1969). For many years, investigators sought to do one of two things with those collections: either to explain place-to-place variation in the intensity of conflict or to analyze fluctuations over time. For those purposes, simple counts of whole events served reasonably well. They served well, that is, so long as investigators could agree on what counted as an individual event (Olzak 1989).

By and large, analysts who did simple counts worried little about repertoires. To be sure, students of strikes distinguished strikes from lockouts, wildcats from formally registered walkouts, and successful from unsuccessful stoppages. Similarly, studies of collective violence typically employed classifications of intensity (how many killed and wounded, how much property damage) and form (street fights, violent demonstrations, uprisings, and more). They analyzed *classified event counts*. For them, cross-tabulations and correlations provided information on the nature and characteristic settings of different sorts of claims.

Austrian social historian Gerhard Botz, for example, prepared a chronology of strikes and "violent political events" for Austria from 1918 to 1938. The violent events came mainly from his reading of three Viennese newspapers—the *Reichspost*, *Arbeiter-Zeitung*, and *Neue Freie Presse*—over the entire period. Botz then added strike data from 1946 to 1976 (Botz 1983, 1987). He combined two methods: 1) analytic narratives placing the selected events in Austria's political history and 2) regression analyses relating fluctuations in violent events (1918–1938) and strikes (1918–1938, 1946–1976) to economic growth, unemployment, and trade union membership. Like many other studies in this

vein, the quantitative analyses show mainly a broad tendency for strike activity to rise and fall with employment, union membership, and prosperity.

About the same time that Botz was working in Austria, Swiss sociologist-historian Hanspeter Kriesi and his colleagues were cataloging what they called “political activation events” in Switzerland from 1945 to 1978. They combed newspapers, political yearbooks, historical works, archives, strike statistics, and leftist literature collections for occasions on which ordinary citizens initiated collective, public claims over specific political issues (Kriesi, Levy, Ganguillet, and Zwicky 1981: 16–33). They also examined the public responses to those 3,553 events. Their extensive quantitative analyses of the data showed that the Swiss system encouraged plenty of citizen participation (see Frey and Stutzer 2002, Trechsel 2000), but also gave a very cold shoulder to marginal groups and stridently anti-government activists (Kriesi, Levy, Ganguillet, and Zwicky 1981: 596–598).

With these results as a background, Kriesi recruited another group of collaborators for a large-scale international comparison of “protest events” in France, Germany, the Netherlands, and Switzerland. They read the Monday issues of four national newspapers from 1975 through 1989, spotting “politically motivated unconventional actions” (Kriesi, Koopmans, Duyvendak, and Giugni 1995: 263; see also Kriesi 1993). They found 7,116 of them, about 120 per country per year. They meant to determine whether the form of the political opportunity structure—for example, very fragmented in Switzerland, highly centralized in France—affected the character and intensity of social movements. Their answer, backed by extensive data: yes, it does. Switzerland provided many more niches for small, differentiated protests while France gave the advantage to nationally coordinated political activity. The Kriesi et al. study represents a sophisticated use of the classified event count, which in recent decades has become the standard method for making descriptions of contentious episodes available for quantitative analysis.

For all their other virtues, none of these massive investigations offered much opportunity to look inside contentious performances and discern their dynamics. Some investigators, however, have come closer. Driven by a general interest in how collective behavior works, Clark McPhail took the first steps toward a general account, not just of contentious events, but of all occasions on which people assemble, act together, and disperse. In 1983, McPhail personally observed 46 political demonstrations in Washington, D.C. He broke them down into specific types of gathering, more than one of which sometimes occurred in the same demonstration. The distribution of the 75 gatherings he saw looked like this:

rally 34
march 19
vigil 10
picket 6
rally-picket-civil disobedience 3
rally-civil disobedience 1

picket-civil disobedience 1
 civil disobedience 1 (McPhail 1991: 183)

McPhail's observed repertoire thus consisted of five distinct performances: rally, march, vigil, picket, and civil disobedience. Mostly the performances occurred separately, but sometimes they combined.

McPhail proposed to group individual gatherings of these sorts into larger sets: events like the demonstrations, campaigns involving multiple events, waves including both individual events and campaigns, and trends. He nevertheless attached particular importance to the fine structure of gatherings:

If comparatively few sociologists have given attention to what people do collectively within gatherings, an increasing number have given attention to larger units of analysis, at more macro levels of analysis, e.g., gatherings, events, campaigns, waves, and trends. The relationships between what people do collectively at micro and macro levels of analysis are too important to ignore. These must be considered in relation to rather than at odds with one another. (McPhail 1991: 186; see also McPhail and Miller 1973, McPhail and Wohlstein 1983, McPhail 2006)

Later, McPhail became more ambitious and fine-grained: He decomposed actions and interactions into four broad categories: facing, voicing, manipulating, and locomotion. Joint actions (e.g., simultaneously facing in the same direction) and interactions (e.g., joining hands) counted as collective action (McPhail, Schweingruber, and Ceobanu 2006). Next McPhail and his collaborators broke each of these categories down with finer and finer distinctions. Voicing, for example, first divided into verbalizing and vocalizing, with vocalizing further subdivided into cheering, booing, oohing-ohhing-ahhing, and whistling. A code sheet then permitted observers to record how many people in some assembly were performing each action or interaction at a given point in time and space (Schweingruber and McPhail 1999: 466).

Multiple observers and their code sheets thus aggregated into overall characterizations of action and interaction distributions for different episodes. They showed, for example, how much more frequently people cheered in a rally than in a march (Schweingruber and McPhail 1999: 480). The procedure centers attention on actions and especially interactions as the elementary particles of collective performances. McPhail's promising line of research has not so far yielded either a coherent theory of performances and repertoires or a feasible method for aggregating and disaggregating descriptions of contentious performances into the sorts of characteristics studied by Botz, Kriesi, and other users of classified event counts. But it heads in the right direction.

International relations specialists have come at the problem from a somewhat different angle: transcribing international actions such as diplomatic exchanges and military attacks uniformly and voluminously from standard news sources. Political scientist Philip Schrodtt and his collaborators have devised methods for making simple

transcriptions of newswire reports. Schrodt called the system KEDS, the Kansas Event Data System. As Schrodt describes it,

KEDS relies on *shallow parsing* of sentences—primarily identifying proper nouns (which may be compound), verbs and direct objects within a verb phrase—rather than using full syntactical analysis. As a consequence it makes errors on complex sentences or sentences using unusual grammatical constructions, but has proven to be quite robust in correctly coding the types of English sentences typically found in the lead sentences of newswire reports. On early-1990s hardware, the system coded about 70 events per second, which seemed at the time to be a huge improvement over human coding projects, which typically have a sustained output of five to ten events per coder per hour. (Schrodt 2006: 5)

A technical cousin of KEDS called the VRA (Virtual Research Associates) System likewise processes the leads or first sentences of online news reports, recording subject, verb, and object (Bond 2006). In principle, these related approaches could eventually produce a fast, sophisticated way to assemble detailed accounts of contentious performances and repertoires. For the moment, however, they have not come close to solving the problems of aggregation and disaggregation inherent in any such effort.

So far, Sidney Tarrow, Roberto Franzosi, and Takeshi Wada have come closest. Tarrow and I began our long collaboration (see, e.g., Tilly and Tarrow 2006) with my consultation on his largest empirical venture, a study of Italy in the 1960s and 1970s. Tarrow examined Italy's cycle of protest from 1965 to 1975, for which the national newspaper *Corriere della Sera* yielded 4,980 "protest events." "Since I was interested in actions that exceeded routine expectations and in which the participants revealed a collective goal," Tarrow tells us,

I collected information on 'protest events', a category which included strikes, demonstrations, petitions, delegations, and violence, but which excluded contentious behaviour which revealed no collective claims on other actors. I defined the protest event as a disruptive direct action on behalf of collective interests, in which claims were made against some other group, elites, or authorities. (Tarrow 1989: 359)

Like most of his predecessors, Tarrow produced a single machine-readable record for each event. But he enriched the enterprise in two important ways. First, he incorporated textual descriptions at a number of critical points—summaries of events, grievances, policy responses, and more. That made it possible to refine his classified counts without returning to the original newspaper sources. Second, within the record he placed checklists where two or more features could coexist. As a result, he was able to analyze, not only the overall distribution of events, but also the frequency of such features as different forms of violence—clashes with police, violent conflict, property damage, violent attacks, rampages, and random violence (Tarrow 1989: 78). Thus cross-classifications of broad event types with specific forms of action brought Tarrow closer to a systematic description of performances, if not of repertoires.

Tarrow consulted me before starting his collection of evidence on Italian contentious events. So did Roberto Franzosi. Franzosi once spent a year working closely with members of my research group. Although he eventually developed his own sophisticated system for recording events, he started his analysis of Italian conflicts since 1919 with a logic my own work has followed closely. The logic uses observed combinations of subject, verb, and object—which Franzosi calls “semantic triplets”—to identify interactions, then attaches further information to the triplets.

On 30 August 1920, workers at Milan’s Romeo metalworking plant responded to a management lockout by occupying the factory (Franzosi 2004a: 66). The occupation started a great wave of sit-down strikes—*occupazioni delle fabbriche*—that eventually became a model for sit-downs in France, the U.S., and elsewhere.

Franzosi shows that he can meaningfully reduce the complex story in the Genoese newspaper *Il Lavoro* to these phases:

firm announces lockout
 workers do not accept decision
 labor leaders decide factory occupation
 workers do not leave plant (Franzosi 2004a: 78)

This plus further information tagged to these spare elements makes it possible for Franzosi to produce rich analyses first of the single episode and then of many episodes: network representations of relations among the actors, classifications of participants’ actions and their sequences, time-series of different sorts of events, and much more. Packed into the general-purpose data storage and retrieval system Franzosi has developed (Franzosi 2004b), the information becomes available for a great variety of pairings.

Properly handled, as Franzosi says, even simple counts tell complex stories. For example, Franzosi’s frequency distribution of the most common actors from 1919 to 1922 identifies an astonishing shift: from heavy involvement of workers and trade unions during the revolutionary years of 1919 and 1920 to their rapid decline; from near-absence of political activists (including Fascists) to their utter prevalence; and no more than a weak presence of government officials as Mussolini’s Fascists began their ascent to power (Franzosi 2004a: 82–84). Those counts then send canny analyst Franzosi back to look more closely at how different actors within these categories interacted, and what claims they made.

Takeshi Wada’s work on Mexican politics between 1964 and 2000 displays many affinities with both Tarrow’s and Franzosi’s analyses of Italian contention (Wada 2003, 2004; Wada wrote his doctoral dissertation under my direction). Wada drew accounts of protest events from the daily newspapers *Excelsior*, *Unomásuno*, and *La Jornada* for 29-day periods spanning national elections over the 37 years, a total of 13 electoral periods. From the newspapers he identified 2,832 events, some linked together in campaigns, for a total of 1,797 campaigns. Wada’s subject-verb-object-claim transcriptions made it possible for him to employ sophisticated network models

of who made claims on whom. Overall, they reveal a sharp politicization of Mexico's collective claim making as the country's partial democratization proceeded. From claims on business, landowners, and universities, protesters moved to making increasingly strong claims on the government itself.

According to Wada's analysis, the weakening of network ties among the elite (especially as concentrated within the longtime ruling party PRI) provided an opportunity for claimants to divide their rulers. It thus advanced the partial democratization of the 1990s. Technically, Wada broke free of many restrictions imposed by classified event counts. That technical freedom opened the way to a sophisticated treatment of interaction in Mexican politics.

Contentious Events in Great Britain

Pinning down performances and repertoires by means of event catalogs requires a vigorous but vital technical effort. Over about ten years, research groups at the University of Michigan and the New School for Social Research worked with me to create a systematic body of evidence on repertoires, and their settings in Great Britain between 1758 and 1834. We invented a series of interactive routines that allowed our researchers to converse with a mainframe computer, store extensive summaries of hand-edited files in a relational data base, and retrieve information from and about contention in an almost infinite variety of ways (Schweitzer and Simmons 1981). We called our enterprise the Great Britain Study. It merited a large effort because the evidence we accumulated helped explain two very large processes: transformations of contentious repertoires and democratization.

The central data set we produced includes machine-readable descriptions for 8,088 contentious gatherings (CGs) that occurred in southeastern England (Kent, Middlesex, Surrey, or Sussex) during thirteen selected years from 1758 to 1820, or anywhere in Great Britain (England, Scotland, and Wales, but not Ireland) from 1828 to 1834. In this study, a CG is an occasion on which ten or more people gathered in a publicly accessible place and visibly made claims which, if realized, would affect the interests of at least one person outside their number. In principle, CGs include almost all events that authorities, observers, or historians of the time would have called "riots" or "disturbances" as well as even more that would fall under such headings as "public meeting," "procession," and "demonstration."

Our standardized descriptions of CGs come from periodicals: the *Annual Register*, *Gentleman's Magazine*, *London Chronicle*, *Morning Chronicle*, *Times*, *Hansard's Parliamentary Debates*, *Mirror of Parliament*, and *Votes and Proceedings of Parliament*; we read these periodicals exhaustively for the years in question plus January–June 1835. Although we frequently consulted both published historical work and archival sources such as the papers of the Home Office in interpreting our evidence, the machine-readable descriptions transcribed material from the periodicals alone. We did not try to find every event about which information was available or even a

representative sample of such events. Instead, we assembled a complete enumeration of those described in standard periodicals whose principles of selection we could examine, and sometimes even test.

Our group created a kind of assembly line: one researcher scanned periodicals for reports of likely qualifying events, another assembled those reports into dossiers of qualifying and nonqualifying CGs, a third hand-transcribed the reports onto preliminary code sheets, an editor reviewed the summary, the next person entered the material in a computer conversation, and so on, until we had a finished entry in the database. Obviously, we could not simply automate the assembly line as the newswire analysts described earlier almost have. On average, we had 2.6 accounts from our periodicals for each CG. That meant we often had to piece together incomplete stories and sometimes had to adjudicate disagreements over such aspects as how many people participated. We also spent plenty of time looking up obscure place names in gazetteers and personal names in histories or biographical dictionaries. Now and then, transcribers made mistakes. Editors had to catch them. In short, it took plenty of conscious, intelligent effort to produce faithful but reduced transcriptions of our sources.

The computer-stored records for CGs break into separate sections:

- a general description of each *event* (8,088 machine-readable records)
- a description of each *formation*—each person or set of persons who acted distinguishably during the event (27,184 records)
- supplementary information on the geographical or numerical *size* of any formation, when available (18,413 records)
- a summary of each distinguishable *action* by any formation, including the actor(s), the crucial verb, (where applicable) the object(s) of the action, and an excerpt of the text(s) from which we drew actor, verb, and object (50,875 records)
- excerpts from detailed *texts* from which we drew summary descriptions of actions (76,189 records)
- identification of each *source* of the account (21,030 records)
- identification of each *location* in which the action occurred, including county, town, parish, place, and position within a one-kilometer grid square map of Great Britain (11,054 records)
- a set of verbal *comments* on the event, or on difficulties in its transcription (5,450 records)
- special files listing all alternative *names* for formations and all *individuals* mentioned in any account (28,995 formation names, 26,318 individual names)

Except for straightforward items such as date, day of the week, and county names, the records do not contain codes in the usual sense of the term. On the whole, we transcribed words from the texts or (when that was not feasible) paraphrases of those words. Think of formation names: Instead of coding names given to formations in

broad categories, we transcribed the actual words used in our sources. For example, the transcription of each action includes the actor's name, a verb characterizing the action, and (in the roughly 52 percent of cases in which there was an object) the object's name.

Here is a simple case. In its issue of 24–26 January 1758, the *London Chronicle* reported that

some Persons assembled in a riotous Manner on Tower-Hill, and broke several Windows, Candles not being soon enough lighted in Honour of the King of Prussia's Birth Day. The same night the Mob committed great Violences in Surry-Street in the Strand, particularly at the Coach-Office, not a Window was left with a whole Pane of Glass.

In the course of the European conflicts historians eventually called the Seven Years War, Britain had recently allied with Prussia, which temporarily made the King of Prussia a popular hero in London. Lighting candles in windows then signaled the occupants' support for a public celebration. People often marked their disapproval of a building's occupants by breaking windows. We interpreted the "some persons" as insufficient evidence that ten or more people had gathered in the same place before the attack on the Coach Office, but took "mob" as indicating at least ten persons got together at the office. The machine-readable transcription of the actions in question therefore ran like this:

| <i>Transcription</i> | <i>subject</i> | <i>verb</i> | <i>object</i> |
|------------------------------------------------------------------------------------------------------------------------------------------------------------------|----------------|-------------|--------------------------|
| the same night the mob (gathered) | mob | #gather | none |
| the mob committed great violences in Surry-Street, in the Strand, particularly at the Coach Office, not a window was left with a whole pane of glass | mob | #break | owner of Coach Office |
| | mob | #end | none |

In this case, the #gather, #break, and #end indicate that we inferred the verb from the narrative rather than finding it directly in the text. Most of the time, however, the texts themselves supplied verbs.

We did take a few editorial liberties. We defined the event itself as starting when interaction began between the first pair of participating formations (including absent objects of claims, such as Parliament), and ending when interaction ended. That led to two further adaptations: a) dividing actions reported in our sources into three segments, before, during, and after the event, but recording all of them, and b) supplying the verb END at the event's termination when our sources failed to report how the participants ceased interacting and/or dispersed. That second maneuver placed 5,936 ENDS in the machine-readable action record—almost 12 percent of all verbs, and almost 75 percent of all events.

We also produced a number of other machine-readable files, including a transcription of Kent's directory of London trades for various years between 1758 and 1828, county census data, descriptions of London-area parishes, assemblies of ten or more people between 1758 and 1820 that did *not* qualify as CGs, a transcription of 1830's Swing events from *Captain Swing* by E.J. Hobsbawm and George Rudé, and a catalog of CGs drawn from a large number of published historical works. In addition, we accumulated massive files of microfilm, photocopy, and notes from British archives, notably including a complete set of county files from the Home Office for 1828–1834. All of these have played their parts in the analysis of changing claim-making.

Unlike investigations singling out events in advance for their importance (whatever the criterion of importance), this sort of inquiry leads almost inevitably to a sense of *déjà vu*, to a realization that the events in any particular time and place fall into a limited number of categories repeating themselves with only minor variations. The ideas of performance and repertoire almost force themselves on a reader of our event catalog.

Yet different settings and periods produce different arrays of events: collective seizures of grain, invasions of enclosed fields, and attacks on gamekeepers in one place and time; sacking of houses, satirical processions, and sending of delegations in another; demonstrations, strikes, and mass meetings in yet another. The prevailing forms of action likewise vary by the social class of the actors (burghers dealing with nobles act differently from peasants dealing with burghers), the contentious issues at hand (disciplining a fellow worker differs from seeking royal favors), and the immediate occasion for gathering (festival, election, meeting of legislative assembly, etc.). The arrays of actions obviously bear a coherent relationship to the social organization and routine politics of their settings.

Theories of Evidence, Theories of Process

The study of Great Britain builds in some risky epistemological and ontological wagers. Epistemologically, this line of investigation bets that periodicals (at least in Great Britain during the eighteenth and nineteenth centuries) report enough of political contention sufficiently well for systematic analysis of large-scale change and variation in contentious performances and repertoires. It goes farther, assuming that canny investigators can infer causes and effects of political contention from relationships between public performances and their settings and from relationships among performances as well. Such an approach rejects a common belief: that only by probing the consciousness of individual and collective actors can we develop reliable causal accounts of their actions. It gets at meanings, not by trying to reconstruct each actor's phenomenology, but by observing how people construct publicly available meanings through social interaction.

Ontologically, this sort of event-based study wagers on the likelihood that public political performances constitute causally coherent and distinguishable phenomena—

that, for example, we can arrive at valid histories of the strike, the forcible seizure of food, or the public meeting in Great Britain. On the other hand, it denies that public political performances defined in these terms form universal categories for which we might be able to formulate covering laws of the type "All strikes everywhere are X and result from Y." Indeed, I initiated the study of Great Britain after long immersion in French history precisely because it seemed to me that the two regimes had fostered different patterns of variation and change in contention (Tilly 1986). Eventually this and other comparisons led me to the conclusion that analysts of political phenomena should not be searching for broad transnational empirical generalizations but for the causal mechanisms and processes that in different combinations, sequences, and initial conditions produce political variation and change.

An important lesson emerges from the nitty-gritty detail of the Great Britain Study. Conscientious investigators can't get away with a theory of the phenomenon under study and a simple hope that their methods test the theory. They need two theories: one of the phenomenon, and another of the processes producing their evidence (chapter 4, "Event Catalogs as Theories," elaborates on the point). In this case, the theories overlap: the same big processes that transformed Britain's contentious repertoires between the 1750s and the 1830s also transformed the sorts of evidence available for studying those repertoires.

Here are some examples. The periodicals from which we drew our uniform catalogs of CGs were becoming abundant as the literate public for political reporting expanded. In the archives, we see how much more assiduous both central authorities and their regional correspondents became in reporting contentious events during the long domestic struggles of the French Revolution and Napoleonic Wars, when a repressive British state clamped down on what its leaders saw as the threat of revolutionary contamination.

In both published and archival sources, furthermore, we see enhanced sensitivity on the part of reporters once a new actor, organization, or form of action appears. The popular mass meetings and regional political unions that surged as the Napoleonic Wars ended and state repression declined attracted worried attention from authorities and reporters alike. In order to make sure that increased or newly biased reporting was not producing what we took to be genuine alterations in repertoires, we had to do a great deal of cross-checking between our national sources and regional or local reports of contentious events (Tilly 1995, appendices). Testing theories about our evidence took almost as much effort as testing theories about transformations of repertoires.

Nevertheless, my collaborators and I did draw some strong conclusions about repertoire transformation from our analyses. We discovered, for example, that the increasing centrality of Parliament to British national politics strongly affected how people made claims (Tilly 1995, 1997b, Tilly and Wood 2003). Parliamentarization had several significant effects: enhancing the importance of individual Members of Parliament as links between local populations and national centers of power, thereby displacing local officials, landlords, and priests as intermediaries, making the public

meeting with appeals to Parliament a central site of contention, and facilitating the growth of national social movements. Without the painstaking effort of cataloging individual contentious events between 1758 and 1834, we would never have been able to document processes of this sort.

What's to Come

Except for a couple of pages in chapter 11 (“History and Sociological Imagining”), you will find only passing references to the Great Britain Study in the following chapters. The chapters consist instead of broadly methodological papers I wrote from 1990 onward. The first cluster of papers (Concepts and Observations) stresses the necessity for informed self-consciousness in the formulation of questions about social processes. The second bunch (Explanations and Comparisons) goes much farther into the epistemology, ontology, and explanatory logic of comparative analysis. The third and final group of papers (Historical Social Analysis) continues the themes of the first two but deals more explicitly with the interplay of history, social science, and the time-bound phenomena they examine. A brief epilogue (chapter 16) draws lessons from the ensemble of essays.

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