



Review: Social Movements in Contentious Politics: A Review Article

Author(s): Sidney Tarrow

Source: *The American Political Science Review*, Vol. 90, No. 4 (Dec., 1996), pp. 874-883

Published by: [American Political Science Association](#)

Stable URL: <http://www.jstor.org/stable/2945851>

Accessed: 20/06/2011 12:32

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

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/action/showPublisher?publisherCode=apsa>.

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

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.



American Political Science Association is collaborating with JSTOR to digitize, preserve and extend access to *The American Political Science Review*.

<http://www.jstor.org>

Social Movements in Contentious Politics: A Review Article

SIDNEY TARROW *Cornell University*

Political Protest and Social Change: Analyzing Politics.

By Charles F. Andrain and David E. Apter. New York: New York University Press, 1996. 387p. \$50.00 cloth, \$19.95 paper.

The Politics of Social Protest: Comparative Perspectives on States and Social Movements.

By J. Craig Jenkins and Bert Klandermans, eds. Minneapolis: University of Minnesota Press, 1995. 381p. \$49.95 cloth, \$19.95 paper.

New Social Movements in Western Europe: A Comparative Analysis.

By Hanspeter Kriesi, Ruud Koopmans, Jan Willem Duyvendak, and Marco G. Giugni. Minneapolis: University of Minnesota Press, 1995. 310p. \$54.95 cloth, \$21.95 paper.

Shanghai on Strike: The Politics of Chinese Labor.

By Elizabeth J. Perry. Stanford, CA: Stanford University Press, 1993. 327p. \$42.50 cloth, \$16.95 paper.

Popular Contention in Great Britain, 1758–1834.

By Charles Tilly. Cambridge, MA, and London: Harvard University Press, 1995. 476p. \$49.95.

Ikki: Social Conflict and Political Protest in Early Modern Japan.

By James W. White. Ithaca, NY, and London: Cornell University Press, 1995. 348p. \$39.95.

Amid the many “turns” in the social sciences over the past few decades—most of them only briefly taken—there has been an extraordinary *re*-turn: the rebirth of systematic comparative and historical studies of contentious politics. I use the word *return* not because studies of social movements have been absent but because, since their heyday in the 1960s, most of these have been case studies of individual movements within a relatively short historical compass. In addition, many have left the complex and multifaceted relations between movements and political structures underspecified and badly operationalized. These new books, in contrast, all attempt to place social movements within a broader structure of contentious politics.

“Contentious politics” I define as collective activity on the part of claimants—or those who claim to represent them—relying at least in part on noninstitutional forms of interaction with elites, opponents, or the state.¹ “Social movements” I define, with Tilly, more narrowly as sustained challenges to powerholders in the name of a disadvantaged population living under the jurisdiction or influence of those powerholders (p. 369). Relating social movements to all forms of contentious politics should enable us to locate the former more effectively in relation to institutions, political alignments, and long-

The author is grateful to Miriam Golden, Doug Imig, Craig Jenkins, Fred Lawson, Mark Lichbach, Michael Hickey, Hanspeter Kriesi, Elizabeth Perry, Jonas Pontusson, Dieter Rucht, and two *APSR* reviewers for comments on a draft of this review article.

¹ For the justification for so broad a definition and for a statement of the ambition to compare social movements with collective action and revolutions within the same framework, see Tarrow 1994 and McAdam, Tarrow, and Tilly 1996a and b.

term political struggles; this is why, along with most of these authors, I call for a “political process” model of social movements. But as I shall argue later, such a move demands that we be more precise about the relations between movements and institutional politics—a conceptual task that remains to be completed.

With their pedigree stretching back to the work of Eisinger (1973), Lipsky (1970), Piven and Cloward (1977), Tilly’s earlier work (1978), and sociologists such as McAdam (1982, 1988) and Gamson (1990), these books and several other recent ones link the study of social movements explicitly to politics.² Central to all of these studies is the concept of “political opportunity structure.”³ This is a concept that still needs better specification and operationalization; but it is the theoretical pivot that has allowed recent students of social movements to connect their subject to institutional politics—a far cry from the older “collective behavior” tradition—and distinct from the more recent traditions of “resource mobilization” and “new” social movements.

The revival of social movement studies that we have been witnessing is the more striking because it does not depend, as was the case in the 1960s, on the outbreak of a major cycle of protest.⁴ If this has rendered this group of studies less inspirational than those of the 1960s, it has allowed them to be more systematic, more historical, and more comparative. In these six books, we can trace the halting, tentative, and sometimes problematic emergence of a political process model of social movements. It is a move that promises to bring the study of movements back to its original heartland—the political struggle—but it also poses some serious problems of conceptualization and comparison to which I will return below.⁵

The new approaches are characterized by three major elements. First, they often employ more refined methodologies designed specifically to analyze contentious interaction and take advantage of the advances in cheap and rapid computation over the past decade, what I will

² See the recent books by della Porta (1995), Rucht (1994), and Tarrow (1994) and the collective volumes edited by Dalton and Kuechler (1990), McAdam, McCarthy, and Zald (1996), and Traugott (1995).

³ The concept is more explicitly developed and employed in the books reviewed here by Andrain and Apter, Kriesi and others, Tilly, and Jenkins and Klandermans, and less explicitly in Perry’s and White’s books. For the origins and development of the concept of political opportunities, see Eisinger (1973), McAdam, McCarthy, and Zald (1996), and Tarrow (1994). For applications to particular movements and areas of the world, see Brockett (1995), Costain (1992), Katzenstein and Mueller (1987), Kitschelt (1986), and Tarrow (1989).

⁴ Some readers of a draft of this essay have objected that events in the Philippines (1986), Burma (1988), China, Eastern Europe (1989), and elsewhere constitute a cycle of protest. Be this as it may, most of these studies were begun, or at least conceived, before the latest wave of movements broke out or came from totally different areas of the world.

⁵ The term “political process model” was first suggested by McAdam (1982) in relation to the U.S. civil rights movement. Its relationship to the key concept of opportunity structure, and to structural approaches to movements in general, is discussed in McAdam, Tarrow, and Tilly (1996b) and in Tarrow (1988).

call contentious event analysis.⁶ Instead of pursuing a group and telling its story from the perspective of the movement or the observer, event history practitioners like Tilly, Kriesi and others, and White compile databases from published accounts in the contemporary press on those events that they consider worthy of notice. This provides them with a temporal map of incidents through which the movement's activities and interactions can be traced and through which movements can be related to relevant covariates and political contexts.

Second, after years of psychological, social-psychological, and resource mobilization approaches, these books bring sociologists and political scientists together in collaborative and replicative work. This is a departure for movement specialists from both disciplines, two groups who had been growing apart in recent years—political scientists in the direction of economics and sociologists toward organization theory.

Third, instead of the familiar case study approach that dominated the field in the past, these scholars all bring a broadly comparative and historical framework to the study of social movements.⁷ Let us begin with this most notable feature of these books, their embedding in comparative and historical frameworks of interpretation.

COMPARATIVE AND HISTORICAL PERSPECTIVES

If these six books turn out to be representative, then the tradition of the movement-career case study has been seriously challenged. In the case study tradition, scholars who are, or once were, associated with or strongly opposed to particular movements typically fasten on a movement or movement organization, trace its career from spontaneous beginnings to institutionalized ends, and identify the agents responsible for this dynamic.⁸ Three of the books under review (by Andrain and Apter, Jenkins and Klandermans, and Kriesi and others) are explicitly comparative; of the three that focus on one country alone, all are at least implicitly comparative and cover periods of time that go beyond the life of a single movement organization. Tilly's book on Britain is

⁶ Rather than borrow from electoral, organizational, and other fields of study, three of the studies under review employ this methodology. For reviews of the instruments and the kinds of data typically used in event history analyses, see Olzak 1989 and Tarrow 1996.

⁷ *Inter alia*, this has brought students of U.S. movements squarely into contact with the work of European scholars, helping to lead them out of an exceptionalist mode and producing major indirect as well as direct benefits of comparison. Costain (1992) and McAdam (1982) are two of the scholars whose American-based work is conceptually linked to comparative research on European movements. Direct transatlantic collaboration can be traced to the 1980s and work of scholars brought together by Dalton and Kuechler (1990), Katzenstein and Mueller (1987), and Klandermans and others (1988). Direct comparisons of U.S. and West European movements are those of Kitschelt (1986), McAdam and Rucht (1993), and Rucht (1994).

⁸ In the best work in this tradition, for example, Piven and Cloward's *Poor People's Movements* (1977), a model of institutionalization was applied comparatively to four movements in recent U.S. history. But in less skillful hands, the approach can degenerate into telling stories of the rise and fall of a movement organization, sometimes blaming the agents responsible for its institutionalization and sometimes celebrating its achievements.

packed with comparative material and is part of his long-term project on claims-making in Britain and France⁹; Perry's on China follows a heterogeneous social actor across a century of changing economic, social, and political systems and compares it in three very different industries—textiles, ports, and tobacco; and White's on Japan is structured as a moving multivariate relationship between peasants and the structural trends in their society over three centuries.

The importance of this comparative dimension for the study of contentious politics cannot be overstated. Social movements are historically evanescent and, as a result, scholarly interest in them waxes and wanes almost as rapidly as they do. This has produced a wealth of idiosyncratic case studies that cannot be compared systematically to those in other countries or periods of history and are therefore doomed to appeal to narrow audiences and to have short shelf lives. By focusing on movements comparatively, the books under review promise their authors longer futures and broader readerships.

With respect to history, there is something new too: several of the books employ rigorous statistical methods within historical perspectives. In the past, historical studies of social movements centered on the history of the particular movement, with general political history as the backdrop. By looking systematically at contentious events over time, authors such as Kriesi and his collaborators, Perry, Tilly, and White focus on the intersections between contending actors and their opponents in different political contexts. From the history of particular movement organizations with politics in the background, all six books shift our attention to the histories of the contentious interaction between movements and the polity. Let us summarize them briefly to examine how their authors do this.

TILLY'S TWO RHYTHMS: ENUMERATING TALES OF CONTENTION

Both in its comparative framework and in its historical framing, Charles Tilly's *Popular Contention in Great Britain* is exemplary. Focusing on more than 8,000 contentious gatherings of ten or more people observed from contemporary sources in the greater London region during thirteen sampled years from 1758 to 1820, and in Britain as a whole from 1828 to 1834, Tilly has elaborated a model of contentious event analysis over many years of experimentation and analysis (p. 63). The method, later adopted in broad outline by Kriesi and others, White, and others,¹⁰ is to enumerate and analyze the actors, their actions, and the targets of their conten-

⁹ See Tilly (1964, 1986) and Shorter and Tilly (1974) for his major works on French contention. Tilly has also provided the most historically rooted version of contentious event analysis and has directly influenced most of the other works under review.

¹⁰ Similar methods have been used by Olzak (1992), by her students Olivier (1989) and Soule (1996), and by Tarrow (1989), as well as in important work in progress by Beissinger (1993, 1996), Ekiert and Kubik (1995), and Rucht (1996). The latter two studies directly replicate important parts of the research protocol developed in Tarrow (1989) for Italy.

tion from serial sources in the context of the political struggles of their times and to relate the data to covariates drawn both from the same sources and from other serial and contextual data. Tilly, in whose work sociology always meets history, leans more extensively on contextual than on serial data; but in the hands of a scholar like White or Olzak (1992), the method allows sequences of contentious events to be related to structural time-series trends like food prices, immigration, urbanization, and the growth of capitalism.

Tilly's focus is more directly on the contentious data themselves. He shows that the secular trend of contention as Britain moved into the nineteenth century lay in a shift from the local, parochial, bifurcated, and particular—and often violent—gatherings of the early part of the period to the cosmopolitan, autonomous, modular—and usually peaceful—repertoire of its close (pp. 45–8). He focuses not only on the “long rhythm” of changes in Britain's contention but also on the shorter rhythms of a number of crucial cycles of protest, including the one that gave rise to Britain's modern politics, 1828–34 (chapter 7). This period emerges from Tilly's method not, as many have thought, as a single peak of contention centered on the Reform Act of 1832 but as a partly autonomous and partly linked series of hills and valleys, including the struggles over Catholic emancipation and Protestant reaction, the suffrage reform, and the Swing movement. In the process, argues Tilly, the social movement organization, soon to become the staple of modern contentious politics, was born.

But although Tilly makes a reasonable case that the changes in contention correlated roughly with the growing centralization of the national state and the capitalization of the British economy, the reader will look in vain for a statistical test of the association between changes in capitalism and the changes in the nature of contentious gatherings. Capitalism only hovers in the background of Tilly's account, in part because he has not included strikes in his enumerations, but in part through an explicit choice: “For thirty years,” he notes, “capitalism has dominated the discussion, and I want to redress the balance.”¹¹

As for changes in the state, these operate in two directions, internal parliamentarization and external warmaking, both of which show up dramatically in the changes Tilly finds in the mounting of collective action. This, on the one hand, is increasingly directed at Parliament and, on the other, is greatly affected by war and by the strains of war financing. But despite the wealth of data he collected, these correlations are only illustrated and are never demonstrated statistically.¹² A major reason for this is the small number of sample years studied (thirteen sampled years for the London region from 1758 to 1834 and seven consecutive years for the shorter national sample between 1828 and 1834). But

another is that, once having established the long rhythm of changes in the repertoire, Tilly's attention is captured by shorter rhythms, cycles of protest, and particular epochs of political history, like the reform period. The weakness of the book is in its failure to relate the “long rhythm” of his database systematically to relevant covariates; its strengths are in tracing the fundamental change in the repertoire as Britain modernized and in carving out a strategy for the contextualized study of events-in-history that can be replicated and compared to other countries, like China and Japan.

ASIAN ACTORS: PERRY AND WHITE

More locally based and written in a more narrative mode than Tilly's book, but equally embedded in particular structures of contention, is Elizabeth Perry's inspiring book, *Shanghai on Strike*. Though influenced by the work of such cultural historians as Hunt (1984), Perry actually has more to say about the effects of a structural factor emphasized by sociologists such as Gould (1995) and McAdam (1988): social networks. But rather than focus only on conventional movement organizations, Perry shows the key role that guilds, criminal gangs, and native-place associations played in the development of the Shanghai labor movement. And alone among these authors, she shows how gender intersected with skill hierarchies and native-place identities in this development. Although the theory in her book sometimes needs to be teased out of the narrative, Perry shows how these “natural” proclivities took on a changing political significance depending on the political context, most notably in the partisanship of first the KMT and then the CCP.

As in Tilly's book, events are the major data points in Perry's account—strikes in her case. But although she looks at contention for 110 years, her book has no equivalent for Tilly's “long rhythm,” for she contents herself with more or less discrete period analyses, as in Tilly's “short rhythms.” These studies of particular periods in Shanghai's labor history are richly detailed and deeply researched. If the book has an overall theme, it is the uneasy tension in the Shanghai labor movement between solidarity and fragmentation. Labor historians have often taken polar positions on the issue of class solidarity, from the impossible-ism of mainstream U.S. historians (e.g., the position that the fragmentation of the working class, which was due to ethnic and racial differences, was a fundamental bar to class solidarity) to the inevitable-ism of the Marxists (e.g., the conviction that the concentration of capital would produce an inevitable growth of solidarity). Perry engages these schools, but she explores the ways in which a fragmented class can act in a politically solidary fashion under various structures of political opportunity. But she does not see workers engaged in a linear developmental process in which they gain “consciousness” and revolutionary potential.¹³

¹¹ In a personal communication to the author, commenting on an earlier draft of this paper.

¹² Tilly writes that he decided to forswear the modeling and estimation of causal relationships in this book to disencumber the narrative, speak to historians of Britain, and give himself the discipline of laying out in words “what quantitative modeling will eventually have to represent, verify and falsify” (p. 73).

¹³ Perry's concluding discussion is worth quoting in detail: “The very awareness of substantial differences among workers often encourages labor activism. . . . Even in the important instances when workers at different skill and wage levels cooperate in joint struggles, the alliances

But does Shanghai's labor history reveal no more than a series of contingent coalitions among otherwise fragmented groups? Or did the act of participating in Perry's short rhythms create increasingly broader collective identities and action frames? What of the long rhythm of the city's industrial tradition? Did it induce no permanent political culture of rebellion? It is striking that a tradition of strikes survived through every phase of Shanghai's history, including some fairly repressive ones. Perry's narrative method penetrates discrete historical periods defined by their dominant political conflicts, but, like Tilly's "short rhythms," it leaves us in the dark about the cumulative growth of a tradition of rebellion.

Even more impressive for its historical and geographic breadth than Perry's book, and more statistically structured over the *longue durée*, is James White's analysis of Japanese rural revolts between 1590 and 1877. Declaring his debt to Tilly (p. xi), White bases his analysis of contentious events on a well-known Japanese archive, Aoki Koji's historical data set on rural rebellion, which in turn was compiled from a wide variety of local and national sources on legal, nonviolent, disorderly, and aggressive contention for almost three centuries (count them!). White roots his study in the tradition of early modern Japanese economic history, but more explicitly than Perry, he connects it to current social movement theories.

White's book shows how quantitative social science methods can be creatively married to in-depth historical knowledge of a particular country. Unlike either Perry or Tilly, he engages in the use of complex multivariate models using data aggregated at both the county and the provincial level. This provides White with a common metric for a long historical period, allowing him to compare the effects of rice shortages, inflation, unemployment, and low wages upon peasant unrest and rebellion. But it will have costs for this otherwise exceptional book. First, it will probably lose White the readership of historians who are more comfortable with narratives of discrete periods than with the statistical study of the *longue durée*. Second, in the absence of indicators of individual behavior, White improvises with a number of purpose-built ones drawn from census data whose relationship to individual-level variables (or its absence) can only be surmised. Third, there is no overarching theme to carry the reader along from one short rhythm to the next over a vast historical terrain.

Still, this path-breaking study marries a superb data set with the skills of a historically trained social scientist. With the exception of one now-dated and rather simple analysis of collective action events after World War II (Sugimoto 1981), it breaks new ground in the statistical analysis of contention in modern Japan. And its openness to rational choice perspectives (see below) will provide a benchmark for years to come for statistically trained Japanese social scientists to test the sometimes

facile generalizations about their country that come from the now dominant culturalist tradition in Japanese historiography.

NEW EUROPEAN MOVEMENTS

There was a period in the 1980s when European scholars like Offe (1985) and Melucci (1989) saw a new wave of social movements developing out of the changes in advanced capitalism—movements that had been liberated from class and ideology; that employed new and creative forms of action; and that focused on identity concerns rather than on strategy (Cohen 1985). The resulting "new" social movement paradigm had good fortune among scholars in Western Europe and even in parts of the world where its social structural preconditions (e.g., advanced liberal capitalist states) were absent. So goes it with new trends in research; but the following decade saw these movements either weaken considerably or take a "long route through the institutions" without, however, inducing their theorists to revise their sometimes apocalyptic vision to relate the new movements more systematically to routine politics.¹⁴

Now, for almost the first time, in the book by Hanspeter Kriesi and his collaborators,¹⁵ we have an empirical analysis of Europe's new social movements which relates them directly to institutional politics. More modest historically than the three studies previously discussed (the book covers a sample of protest events for fifteen years from the mid-1970s to the late 1980s) *New Social Movements in Western Europe* is the only study in this group based on a systematic comparative design. The authors have collected parallel data on protest events from newspaper files for France, Holland, Switzerland, and West Germany from 1975 to 1989 and analyzed them in relation to a battery of indicators of conventional politics. Using a single newspaper source and a sample of dates for each of their four countries, they work with a rather simple research protocol but relate their findings creatively to the characteristics of the political systems of their four states.

Not the least of this book's virtues is that its authors specify the much overused label "new social movements" more clearly than their European predecessors tended to do. They demonstrate the junctions between these movements and the not-so-new institutional politics of the countries they study. Far from being detached from the political process as the more ardent students of these new movements imagined, Kriesi and colleagues show how the rhythms of the contentious events that were mounted over a decade and a half corresponded closely to the different party alignments in each country—from France's Socialist government's successful co-opting of its new social movements in the early 1980s to the West German government's provision of opportuni-

do not necessarily reflect class consciousness. . . . The mobilization that led to massive struggles was usually based on pre-existing, smaller-scale groupings. Only when fictive kinship networks, native-place gangs, secret societies, and the like could be drawn into cooperation was a major upheaval possible" (p. 251).

¹⁴ See the partial revision of his theory in the guise of an updating by Offe (1990) and the critiques of the theory's ahistoricism by Calhoun (1995) and D'Anieri, Ernst, and Kier (1990).

¹⁵ They are Jan Willem Dyvendak, Marco G. Giugni, and Ruud Koopmans.

ties to similar movements through its support for the European missile system. This book is a model for the comparative study of social movements in relation to the processes of conventional politics.

At the heart of the study is the concept of political opportunity structure, which the authors employ in both a cross-national and a dynamic cross-time manner to relate their findings about protest to more conventional institutional and social structural indicators. Unfortunately, they hew more closely to the static, cross-national operationalization of the concept than to its time-series version, which makes it easier for them to compare national political systems' influence on movements than to analyze changes in movement strategy and structure. They show clearly how differences in the character of states and cleavage structures have an effect on movements in the four countries—the basic message being that France is different. Except for a chapter on changing alliances (chapter 3) and another on cycles of protest (chapter 5), however, the static, cross-sectional specification of opportunity structure prevents them from taking analytical advantages of how changes in political alignments stimulated movement rise and fall over time.¹⁶

Be that as it may, Kriesi and colleagues demonstrate how movements respond to, and occasionally create, political opportunities in relation to standard variables of political analysis. This not only provides a healthy antidote to the obsessive “new-ism” of the studies of new social movements that appeared in the 1980s but also demonstrates the powerful uses of the concept of political opportunities for comparative politics; and it will eventually help us understand whether the last few decades of Western history witnessed a progressive increase in contentious politics, as was recently argued by Dalton (1996, chapter 4), or rather a more familiar pattern of protest cycles.¹⁷

TWO SYNTHESSES

Standing apart from these four books both in style and content are the volume edited by Jenkins and Klandermans, *The Politics of Social Protest*, and Andrain and Apter's *Political Protest and Social Change*. The former delivers both less than it claims—to “move beyond existing social movement theories by examining the role of states in social movement development” (p. 7)—and more. While the editors' specification of the theoretical relations between states and social movements is too cursory and is not followed through consistently in the empirical chapters, Jenkins and Klandermans provide the reader with a broad sample what many of the new group of social movement researchers are writing, and they make a distinction that is often lost in this tradition—between the largely structural approaches used in

most comparative studies and the processual focus in time-series studies of individual countries. As indicated above with respect to Kriesi and others, the attempt to analyze the processes of change from a static structural standpoint is one of the ongoing problems of the comparative political process approach.

The Jenkins and Klandermans book is divided into sections on the political origins of social protest, the electoral contexts in which protest occurs, and (a few of) the outcomes that movements sometimes achieve. Particularly notable are the three comparative chapters (the contribution of Karl-Dieter Opp and his collaborators on Germany, Israel, and Peru; that of Michael Nollert on neocorporatism and protest in Western democracies; and the essay by Donatella della Porta and Dieter Rucht on the “left-libertarian” movements of Italy and Germany from the 1960s through the 1980s).

Some of the chapters in the book synthesize work reported at greater length elsewhere: Kriesi provides a succinct chapter summarizing the major findings of his co-authored volume; Ron Aminzade offers a deft analysis of the relations between movements and parties in mid-nineteenth-century France (see Aminzade 1993); and Russell Dalton summarizes the findings on strategies of partisan influence reported in his *The Green Rainbow* (1994). Diarmuid Maguire draws on his study of the 1980s European peace movements (1990) to employ the political opportunity structure concept to great advantage. Jenkins provides a crisp theoretical chapter on movements, representation, and the state that goes beyond the “here is what our authors will say” format (chapter 2). But Klandermans, who has done more to create the current synthesis of West European and U.S. research than anyone else, contents himself with co-authoring a brief introduction with Jenkins.

Political Protest and Social Change also delivers both more and less than its authors promise. Like Kriesi and others and Tilly, Andrain and Apter “formulate a theory of political opportunities,” one which assumes “that cultural values, socio-political structures, and individual behaviors shape the origins, activities, and outcomes of protests” (p. 3)—a tall order. But in contrast to Tilly, the book makes no attempt to apply the concept historically; and in contrast to Kriesi and others, opportunity structure is never well specified theoretically or even applied to concrete social movements. (In fact, between its first appearance in the Introduction and its apotheosis in the Epilogue, the language of opportunity structure appears only eleven times in 317 pages of text.) Given the centrality of the concept in the current social movement literature and to the authors' own theoretical position, one might have expected to learn much more about how opportunities shape movement mobilization, collective action, or outcomes.

The greatest strength of the Andrain and Apter book is the authors' incredible range of acquaintance with theory and research in comparative politics. They work out of three building-block concepts (structure, culture, and behavior). These they surround with syntheses from a plethora of theoretical approaches and issues, illustrating them with samplings from their own and others' empirical work from Africa, Asia, and Western Europe.

¹⁶ It also leads them to underplay the growing *transnational* influences on these movements that began after the 1960s, since their independent variables are almost all specified at the level of the national state. For evidence of transnational diffusion of the movements emanating from the 1960s, see McAdam and Rucht (1993).

¹⁷ For alternative “cyclical” approaches, see Brand (1990), Koopmans (1993), and Tarrow (1989, 1994).

There are some choice morsels here, such as the synthetic chapter on ideologies (chapter 2), which has echoes of Apter's classical piece from his edited volume, *Ideology and Discontent* (1960), and the one on nationalism (chapter 4), which recalls his earlier work on political religion (1963). But there are also long, sloggish essays that seem better suited to preparing graduate students for their general exams than for motivating and shaping empirical work on contentious politics. The notes and references alone would take most readers several years to cover.

In summary, these six books provide a rapid overview of the current state of theory and research on social movements in both Western Europe and the United States. Three of them (Tilly, White, and Kriesi and others) operationalize a familiar subject—social movements—through a systematic methodology—contentious event history. Two of them (Tilly and Perry) are deeply historical in a narrative sense. And three (Kriesi and others, Jenkins and Klandermans, and Andrain and Apter) are explicitly comparative. Together, they bring both history and comparison systematically into the study of collective action. But there are two major disappointments in these studies: their uncertain relationship to the rational choice paradigm that has been gaining grounding in our discipline, and the connections among social movements, contentious politics, and political institutions.

CONTENTIOUS POLITICS AND RATIONAL CHOICE: PARALLEL PROGRAMS OR COMPETING PARADIGMS?

In a definitive review article, Mark Lichbach writes that “in spite of numerous efforts by economists, political scientists and sociologists, most scholars now recognize that the marriage between the CA (collective action) research program and conflict studies has largely failed” (1994, 9). Why has this synthesis not occurred? One common element in all six studies under review is that they are far more contextualized, and less deductively streamlined, than most of the work that has come out of the choice-theoretical tradition. In these and other studies in the political process tradition, the guiding assumption is that movements arise, change, succeed, or fail as a function of changes in political opportunities. However operationalized (and this is a source of contention), a focus on opportunity structure produces a rich analysis of how political rules and institutions, strategic choices, and changes in the forms of contention affect social movements.

In the form in which rational choice theorists have approached contentious politics, which I will refer to with Lichbach as the “collective action” approach, this focus on the variable and changing interaction between movements and their interlocutors is radically foreshortened and comparatively impoverished. Apart from one or two works on peasants (Berejikian 1992, Popkin 1977), a reader on revolutions (Taylor 1988), a modest literature on strikes (Golden 1996), Chong's study of civil rights (1991), Opp's work based on survey materials (1989) and his study of the East German revolution

(Opp, Voss, and Gern 1995), and Tong's innovative reconstruction of Chinese rebellions (1991), most researchers on contention who access the rational choice tradition have limited themselves to enunciating general laws (DiNardo 1985). The reason seems to be that the version of rational choice that has been imported into the study of social movements has “focused almost exclusively on the initial problem of whether anyone who is rational will actually participate in protest and rebellion” (Lichbach 1994, 9). The result, concludes Lichbach, “is that almost no CA theorists have gone on to study the many substantive problems arising in revolts and protests” (p. 9)—the very problems that are central to the works reviewed here.

In fact, for most writers coming from the rational choice tradition, collective action remains a generic term. This has the virtue of allowing its users to subsume a variety of kinds of action under a general theoretical rubric, but its subcategories are seldom distinguished from one another or related to the differential tendencies of different actors to employ them (e.g., Tilly's findings about the historical changes in the structure of the repertoire would be unimaginable for a theorist who posited something called “collective action” and called it a day). For most writers in the collective action tradition, individuals calculate the costs, risks, and constraints on collective action based on the nature of the goods they want to maximize and of their incentives and constraints to seek them; but they seldom take into account the incentives and constraints offered by particular opportunities or traditions of collective action, like the strike, the *charivari*, or the protest demonstration.¹⁸

The dominant paradigm of collective action theory is and remains the market.¹⁹ This, in turn, leads to an inevitable caricaturization of CA on the part of scholars who find market models unpalatable,²⁰ a reaction that has not been helped by the tendency of some rational choice theorists to claim totality for their models and dismiss work coming from less deductively satisfying traditions as “atheoretical” (Kiser and Hechter 1991). Few scholars on either side of the divide have taken Lichbach's advice of trying to build bridges between the two research traditions (1994).

¹⁸ Some recent efforts have relaxed the microeconomic assumptions of the original CA model, focusing, for example, on the differential tendency to participate of people who are facing losses versus gains (Berejikian 1992); on the influence on willingness to participate of the likelihood that a campaign will succeed (Klandermans 1984); on the incentives to collective action that arise within communities (Taylor 1988); and differentiating the incentives to collective action in markets, communities, hierarchies, and contracts (Lichbach 1994, 1995).

¹⁹ Lichbach makes an exhaustive effort to specify hypotheses in the collective action tradition based on his typology of hierarchies, communities, and contracts, as well as markets. The dominance of market metaphors in this area is evident, however, in the much larger number of hypotheses he summarizes under markets ($N = 12$) than under any of the other three types (communities = 2; hierarchies = 5; contract = 3). See his 1994 article, pp. 11–9.

²⁰ For example, the adoption of the language of economics by McCarthy and Zald (1977) and Zald and others (1987), who used such terms as movement “entrepreneurs,” “movement industries,” and “movement sectors,” led some scholars who had been active in the movements of the 1960s to reject their research program in its entirety, partially out of distaste for what they mistakenly saw as a conservative bias.

This takes us back to the books under review and to a puzzle: Between the guiding assumptions of the collective action paradigm and those of the contentious politics school, there is an underlying homology, or at least a dovetailing. Both assume that potential actors mobilize not in response to raw grievances and discontents but as the result of the incentives and opportunities that surround them. While rational choice scholars find these incentives in individual calculations of cost and benefit, and political process scholars see the decision working through group processes and political opportunities, members of both schools explain variations in participation as the result of constraints and opportunities. Therefore, potentially creative intersections between the two approaches cry out to be explored.²¹

Where do these six studies take the dialogue between the choice-theoretic paradigm and political process approaches? Not very far. Tilly, who surveys a number of competing theoretical approaches to British contention, does not include rational choice among them. Andrain and Apter discuss the rational choice perspective but mainly in connection with elections (pp. 260ff) and appear to confound it with behaviorism. Jenkins and Klandermans do include in their reader the work of one group of rational choice researchers (Karl-Dieter Opp and his collaborators), but they barely mention the theory in their introduction. Elizabeth Perry mentions it, too (p. 37), but only to express approval of Michael Taylor's emphasis on community in his 1988 essay.

Of all our authors, only James White and Kriesi and others try to access the rational choice tradition empirically and theoretically. White uses rational choice perspectives to interpret his empirical findings but does so only inferentially. Lacking individual-level data, he operationalizes rationality in early modern Japan as interests, which he measures by proxy through urbanization, economic vulnerability, and productivity per capita (p. 233). These variables are lodged at several removes from the individual strategic decision making that most authors see as central to rational choice theory. Using these admittedly proxy measures, White finds "an extraordinary consistency between popular behavior and the assumption of popular rationality" (p. 234). Though these results are suggestive rather than definitive, White at least gives the rationality assumption a run for its money.²²

White's findings point to one of the weaknesses of the political process model: Although it is better rooted empirically than work in the CA tradition, few authors in this tradition attempt to assess the strength of political opportunity variables against more traditional economic ones (e.g., inequality, inflation, unemployment). For example, Kriesi and others deal with "cleavage structures" as one of their defining variables, but these are

deduced as national constants (chapter 1), which gives the authors no analytical leverage to relate changes in economic conditions to changes in political opportunity.²³

Kriesi and colleagues criticize rational choice theory for leaving vague the thesis that social movements are rational decision makers who make strategic choices (p. 37). Stated in such vague terms, they argue, the thesis "does not constitute much of a bridge between political structures and movement action, since it leaves us not much wiser with regard to the ways in which political opportunity structures translate into costs and benefits at the individual level" (pp. 37–8). They suggest instead returning to Tilly's (1978) distinction among facilitation, repression, reform, and threat—in other words, a set of concrete opportunities and constraints derived from political structure which directly affect the costs and perceived benefits of collective action.

Why have students of contentious politics shied away from testing the powerful deductive logic of rational choice? It may partially result from the fact that they mostly employ aggregate protest data and have a penchant for structural analysis, while rational choice theorists focus on individuals and are less sensitive to the permutations of the structural and historical contexts that surround them. It may also relate to the greater mathematical inclination of practitioners of rational choice, compared to the richer empirical and historical resources of students of contentious politics.²⁴ Given the homologies between the two approaches and their obvious complementarities, however, more scholars should take up Lichbach's challenge and attempt to confront them.

CONTENTIOUS POLITICS, SOCIAL MOVEMENTS, AND POLITICS

This takes us to my final observations. Focusing on contentious politics, rather than on individual social movements, will allow us to link contention to conventional politics, to political alliances, and to the changing seasons of the political struggle more effectively than older studies of social movements or collective behavior. But, by the same token, it creates problems in differentiating social movements within the general range of contentious politics, as well as that of distinguishing the latter from politics in general.

Look, first, at the concept of political opportunity structure: Often considered a variable, it is really an aggregate of separate variables. By breaking it down into a small number of finite dimensions (e.g., the presence or absence of influential allies, the opening of possibilities for legal collective action, splits within or between elites, realignments in the party system),²⁵ the impor-

²¹ For a preliminary attempt to do so, see McAdam, Tarrow, and Tilly 1996b.

²² White also tries to assess opportunities for protest, which he measures as variations in shogunal control and the size of the local samurai population. He finds both interests and opportunities increase the propensity to protest, with opportunities slightly outweighing interests at county levels and the reverse occurring at the provincial level (p. 237).

²³ For a provocative cross-national analysis that relates opportunity structure to inequality in producing collective action, see Schock 1996.

²⁴ But this is not true of all rational choice practitioners. See, for example, Chong (1991), Golden (1996), and Tsebelis (1990), who combine comparative and historical materials with powerful deductive models.

²⁵ See McAdam's defining essay on political opportunities in McAdam, McCarthy, and Zald (1996, chapter 1).

tance of opportunities in triggering movements can be operationalized and assessed and their changes related to conventional politics. But if opportunity structure is allowed to become a catch-all term for any interaction between a group and the state, or if the concept is specified post hoc, then we will end up with ad hoc analyses that border on descriptions. One of the priorities in the next wave of studies in this tradition must be to pin down opportunity structures in a more testable and widely accepted form.

Second, consider the place of social movements within the broader field of contentious politics. Tilly, who comes closest to formulating this relationship theoretically, regards movements as only one form of collective action, a point on a typology consisting of the scope of action and the orientation to powerholders (1983). Other points on this continuum, for Tilly, would be rick burnings, forced illuminations, the pulling down of houses, the *charivari*, the strike, and the protest demonstration. But if movements are no more than a point on a continuum of types of collective action, are they observable events, in the same sense as some of these other actions? Then, what of movement organizations that *employ* these other forms? Can an observable event employ another observable event? Or is a social movement more than an observable event? I prefer to think that a movement is an actor or coalition of actors whose presence can be traced by observing the combination of collective actions which typify its interaction with its antagonists, allies, and publics but one that is not reducible to or comparable to a particular form of action.

None of our other authors seriously takes up the theoretical issue of the relationship between social movements and contentious politics in general. Andrain and Apter do not discuss it; White and Perry focus on different forms of contentious politics without raising the issue of the role of social movements within that universe of cases. Jenkins and Klandermans study social movements without specifying a broader universe of contentious politics. And Kriesi and others simply assume that the newspaper record of contentious public events in the areas of concern of the new social movements during the period studied in itself constitutes the record of the dynamic of the "new" movements of that period.

But are all the individual actions enumerated by Kriesi and colleagues—or, for that matter, by any of our other authors—necessarily "movement events"? Are there no contentious collective actions carried out by nonmovements? And are there no movement activities that are not captured by the record of contentious collective action? Authors writing in the "new" social movement tradition appear to think so (Melucci 1988); students of the women's movement point out that much of its work is done in private, or at least in "unobtrusive" forms of mobilization (Katzenstein 1990). There is a good deal to be said for both positions. Especially given the tendency for the "normalization" of protest in Western societies since the 1960s—in which parties, unions, interest groups, and temporary coalitions of local actors regularly employed contentious forms of politics—this poses a major conceptual problem for the event-based study of

social movements and one that the next wave of studies will need to confront.

This leads to a final question, that of the relationship between contentious politics and politics in general.²⁶ As defined above, the study of contentious politics includes all situations in which actors make collective claims on other actors, claims which, if realized, would affect the actors' interests, when some government is somehow party to the claims. In these terms, wars, revolutions, rebellions, social movements, industrial conflict, feuds, riots, banditry, shaming ceremonies, and many more forms of collective struggle potentially qualify as contentious politics. Although, from time to time, a heroic synthesizer such as Kenneth Boulding (1962) has laid out a general theory of conflict, the study of contentious politics has not proceeded as a unified field. Instead, specialists in different kinds of political contention have created sui generis models of their subject matter, often ignoring powerful analogies or continuities with neighboring phenomena. As a result, each group of practitioners has emphasized a different set of concepts, theoretical issues, and comparisons.

These six studies attack the problem of synthesis empirically rather than theoretically. But the problem they raise is one of knowing what to *exclude* from the range of contentious politics. Are interest groups part of this population? Or only those interest groups which engage in noninstitutional actions? Or only interest groups *when* they engage in such actions? Conversely, do social movements engaging in legal actions, as so many do today, escape the boundaries of contentious politics? And what if, having completed a successful judicial challenge or failing to do so, they turn to more disruptive actions? Can we be content to retrieve actors in our net of political events only when they are anti-institutional and allow them to slip through it when they work within institutions?

Perhaps the solution is to focus on the strategic interactions between claims-makers and authorities whenever the claims made threaten some fundamental standing commitment of powerholders or other groups, regardless of the tactics used by claimants? But such an approach would cut against the grain of one of the main achievements of the new study of contentious politics: the methodological advance of focusing serially on contentious event histories. Studying movement actions regardless of whether they produce contentious clashes in public space would require a broader, less homogeneous, and immensely larger measurement strategy than the systematic enumeration and analysis of protest events. Practitioners of the new approach will have to decide which virtue they want to maximize, an inclusiveness that will combine serial data on protest with other kinds of information, or an exclusiveness that maximizes homogeneity and seriality at the risk of losing important information when movement actors work within institutions.

But these are problems for the next generation of studies of contentious politics. In these six books, the

²⁶ The following passage roughly follows the argumentation in McAdam, Tarrow, and Tilly 1996b.

study of social movements has moved squarely back to its origins—the political struggle—from social psychology, organizational sociology, and public choice; it has produced a form of instrumentation that is a major advance on the idiosyncracies and dispersiveness of the case study; and in the concept of political opportunity structure, it has developed a theoretical pivot which advances comparison and produces hypotheses to explain movement emergence, dynamics, and outcomes. From an archipelago of books whose subjects range from eighteenth-century London to twentieth-century Shanghai, it would be hard to ask for more.

REFERENCES

- Aminzade, Ronald. 1993. *Ballots and Barricades. Class Formation and Republican Politics in France, 1830–1871*. Princeton: Princeton University Press.
- Apter, David E. 1960. "Ideology and Discontent." in *Ideology and Discontent*, ed. Apter. New York: The Free Press.
- Apter, David E. 1963. "Political Religion in the New Nations." In *Old Societies and New States*, ed. Clifford Geertz. New York: The Free Press.
- Beissinger, Mark R. 1993. "Demise of an Empire-State: Identity, Legitimacy, and the Deconstruction of Soviet Politics." In *The Rising Tide of Cultural Pluralism*, ed. M. Crawford Young. Madison: University of Wisconsin Press.
- Beissinger, Mark R. 1996. "How Nationalisms Spread: Eastern Europe Adrift the Tides and Cycles of Nationalist Contention." *Social Research* 63 (Spring):1–50.
- Berejikian, Jeffrey. 1992. "Revolutionary Collective Action and the Agent-Structure Problem." *American Political Science Review* 86 (September):647–57.
- Brand, Karl-Werner. 1990. "Cyclical Aspects of New Social Movements." In *Challenging the Political Order*, ed. Russell J. Dalton and Manfred Kuechler. Cambridge, MA: Polity Press.
- Brockett, Charles. 1995. "A Protest-Cycle Resolution of the Repression/Popular-Protest Paradox." In *Repertoires and Cycles of Collective Action*, ed. Mark Traugott. Durham: Duke University Press.
- Calhoun, Craig. 1995. "New Social Movements of the Early Nineteenth Century." In *Repertoires and Cycles of Collective Action*, ed. Mark Traugott. Durham, NC: Duke University Press.
- Chong, Dennis. 1991. *Collective Action and the Civil Rights Movement*. Chicago: University of Chicago Press.
- Cohen, Jean. 1985. "Strategy or Identity: New Theoretical Paradigms and Contemporary Social Movements." *Social Research* 52 (Winter): 663–716.
- Costain, Anne. 1992. *Inviting Women's Rebellion. A Political Process Interpretation of the Women's Movement*. Baltimore and London: Johns Hopkins University Press.
- Dalton, Russell J. 1994. *The Green Rainbow: Environmental Groups in Western Europe*. New Haven: Yale University Press.
- Dalton, Russell J. 1996. *Citizen Politics in Western Democracies*. 2d ed. Chatham, NJ: Chatham House.
- Dalton, Russell, and Manfred Kuechler, eds. 1990. *Challenging the Political Order*. Cambridge, MA: Polity Press.
- D'Anieri, Paul, Claire Ernst, and Elizabeth Kier. 1990. "New Social Movements in Historical Perspective." *Comparative Politics* 22 (July):445–58.
- della Porta, Donatella. 1995. *Social Movements, Political Violence, and the State*. New York and Cambridge: Cambridge University Press.
- DiNardo, James. 1985. *Power in Numbers: The Political Strategy of Protest and Rebellion*. Princeton: Princeton University Press.
- Eisinger, Peter. 1973. "The Conditions of Protest Behavior in American Cities." *American Political Science Review* 67 (March):11–28.
- Ekiert, Grzegorz, and Jan Kubik. 1995. "Protest Event Analysis in the Study of Regime Change and Democratization." Protest Event Analysis: Methodology, Applications, Problems, International Workshop at the Social Science Center, Berlin, Germany, June 12–24.
- Gamson, William. 1990. *Strategy of Social Protest*. 2d ed. Belmont, CA: Wadsworth.
- Golden, Miriam. 1996. *Heroic Defeats*. New York and Cambridge: Cambridge University Press.
- Gould, Roger. 1995. *Insurgent Identities: Class, Community, and Protest in Paris from 1848 to the Commune*. Chicago: University of Chicago Press.
- Hunt, Lynn. 1984. *Politics, Culture and Class in the French Revolution*. Berkeley and Los Angeles: University of California Press.
- Katzenstein, Mary F. 1990. "Feminism within American Institutions; Unobtrusive Mobilization in the 1980s." *Signs* 16 (Autumn):27–54.
- Katzenstein, Mary, and Carole Mueller, eds. 1987. *The Women's Movement of the United States and Western Europe*. Philadelphia: Temple University Press.
- Kiser, Edward, and Michael Hechter. 1991. "The Role of General Theory in Comparative-historical Sociology." *American Journal of Sociology* 97 (July):1–30.
- Kitschelt, Herbert. 1986. "Political Opportunity Structures and Political Protest: AntiNuclear Movements in Four Democracies." *British Journal of Political Science* 16 (January):57–85.
- Klandermans, Bert. 1984. "Mobilization and Participation: Social-Psychological Expansions of Resource Mobilization Theory." *American Sociological Review* 49 (October):583–600.
- Klandermans, Bert, and others. 1988. *From Structure to Action: Comparing Social Movement Research across Cultures*. Greenwich, CT: JAI.
- Koopmans, Ruud. 1993. "The Dynamics of Protest Waves: Germany, 1965 to 1989." *American Sociological Review* 58 (October):637–58.
- Lichbach, Mark I. 1994. "Rethinking Rationality and Rebellion. Theories of Collective Action and Problems of Collective Dissent." *Rationality and Society* 6 (January):8–39.
- Lichbach, Mark I. 1995. *The Rebel's Dilemma*. Ann Arbor: University of Michigan Press.
- Lipsky, Michael. 1970. *Protest in City Politics: Rent Strikes, Housing and the Power of the Poor*. Chicago: Rand, McNally.
- Maguire, Diarmuid. 1990. "New Social Movements and Old Political Institutions. The Campaign for Nuclear Disarmament, 1979–1989." Ph.D. diss. Cornell University.
- McAdam, Doug. 1982. *Political Process and the Development of Black Insurgency, 1938–1978*. Chicago: University of Chicago Press.
- McAdam, Doug. 1988. *Freedom Summer*. Chicago: University of Chicago Press.
- McAdam, Doug, John McCarthy, and Mayer Zald, eds. 1996. *Comparative Perspectives on Social Movements*. New York and Cambridge: Cambridge University Press.
- McAdam, Doug, and Dieter Rucht. 1993. "The Cross-National Diffusion of Movement Ideas." *Annals of the American Academy of Political and Social Science* 528 (July):56–74.
- McAdam, Doug, Sidney Tarrow, and Charles Tilly. 1996a. "To Map Contentious Politics." *Mobilization* 1 (March):17–34.
- McAdam, Doug, Sidney Tarrow, and Charles Tilly. 1996b. "Towards a Comparative Synthesis of Social Movements and Revolutions." Presented to the Annual Meeting of the American Political Science Association, San Francisco.
- McCarthy, John, and Mayer Zald. 1977. "Resource Mobilization and Social Movements: A Partial Theory." *American Journal of Sociology* 82 (May):1212–41.
- Melucci, Alberto. 1988. "Getting Involved: Identity and Mobilization in Social Movements." In *From Structure to Action*, ed. Bert Klandermans and others. Greenwich, CT: JAI.
- Melucci, Alberto. 1989. *Nomads of the Present. Social Movements and Individual Needs in Contemporary Society*. Philadelphia: Temple University Press.
- Offe, Claus. 1985. "New Social Movements, Challenging the Boundaries of Institutional Politics." *Social Research* 52 (Winter):817–68.
- Offe, Claus. 1990. "Reflections on the Institutional Self-Transformation of Movement Politics." In *Challenging the Political Order*, ed. Russell Dalton and Manfred Kuechler. Oxford and New York: Oxford University Press.
- Olivier, Johan. 1989. "Dynamics of Ethnic Collective Action in South Africa, 1970–1984." Ph.D. diss. Cornell University.
- Olzak, Susan. 1989. "Analysis of Events in the Study of Collective Action." *Annual Review of Sociology* 15:119–86.
- Olzak, Susan. 1992. *The Dynamics of Ethnic Competition and Conflict*. Stanford: Stanford University Press.
- Opp, Karl-Dieter, with Peter and Petra Hartman. 1989. *The Rationality of Political Protest*. Boulder, CO: Westview.

- Opp, Karl-Dieter, Peter Voss, and Christian Gern. 1995. *Origins of a Spontaneous Revolution*. Ann Arbor: University of Michigan Press.
- Piven, Frances Fox, and Richard Cloward. 1977. *Poor People's Movements: Why They Succeed; How They Fail*. New York: Vintage.
- Popkin, Samuel. 1977. *The Rational Peasant: The Political Economy of Rural Society in Vietnam*. Berkeley and Los Angeles: University of California Press.
- Rucht, Dieter. 1994. *Modernisierung und neue soziale Bewegungen. Deutschland, Frankreich und USA in Vergleich*. Frankfurt and New York: Campus.
- Rucht, Dieter. 1996. "Forms of Protest in Germany 1950–1992: A Quantitative Overview." Paper prepared for the workshop on "Europe and the United States: Movement Societies or the Institutionalization of Protest." Cornell University.
- Schock, Kurt. 1996. "A Conjunctural Model of Political Conflict." *Journal of Conflict Resolution* 40 (March):98–133.
- Shorter, Edward, and Charles Tilly. 1974. *Strikes in France*. Cambridge, MA: Harvard University Press.
- Soule, Sarah. N.d. "The Student Divestment Movement in the US and Tactical Diffusion: The Shantytown Protests." *Social Forces*. Forthcoming.
- Sugimoto, Yoshio. 1981. *Popular Disturbances in Postwar Japan*. Hong Kong: Asian Research Service.
- Tarrow, Sidney. 1988. "National Politics and Collective Action: Recent Theory and Research." *Annual Review of Sociology* 14:421–40.
- Tarrow, Sidney. 1989. *Democracy and Disorder*. Oxford: Oxford University Press.
- Tarrow, Sidney. 1994. *Power in Movement, Social Movements, Collective Action and Politics*. New York and Cambridge: Cambridge University Press.
- Tarrow, Sidney. 1996. "Studying Contentious Politics." Unpublished paper, Ithaca, New York.
- Taylor, Michael, ed. 1988. *Rationality and Revolution*. Cambridge: Cambridge University Press.
- Tilly, Charles. 1964. *The Vendée*. Cambridge, MA: Harvard University Press.
- Tilly, Charles. 1978. *From Mobilization to Revolution*. Englewood Cliffs, NJ: Prentice-Hall.
- Tilly, Charles. 1983. "Speaking Your Mind without Elections, Surveys, or Social Movements." *Public Opinion Quarterly* 47:461–78.
- Tilly, Charles. 1986. *The Contentious French*. Cambridge, MA: Harvard University Press.
- Tong, James. 1991. *Disorder under Heaven. Collective Violence in the Ming Dynasty*. Stanford: Stanford University Press.
- Traugott, Marc, ed. 1995. *Repertoires and Cycles of Collective Action*. Durham, NC: Duke University Press.
- Tsebelis, George. 1990. *Nested Games: Rational Choice in Comparative Politics*. Berkeley and Los Angeles: University of California Press.
- Zald, Mayer and John McCarthy, eds. 1987. *Social Movements in an Organizational Society*. New Brunswick, NJ: Transaction Press.