

Marco Giugni. *Social Protest and Policy Change: Ecology, Antinuclear, and Peace Movements in Comparative Historical Perspective*. Lanham, MD: Rowman and Littlefield, 2004. \$75.00 (cloth), \$32.95 (paper).

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Social movements exist not mainly to mobilize resources or change their participants, but to effect larger social change, often through state policy. For this reason, scholars have been increasingly examining whether social movements are influential and, when they are, what accounts for their influence.

Marco Giugni, of the Political Science Department of the University of Geneva, has been in the forefront of scholars addressing these questions. He is the author of a much-discussed 1998 article in the *Annual Review of Sociology* that has helped to shape this literature. *Social Protest and Policy Change* builds on his previous work and attempts to do three things. One is to describe the ecology, antinuclear, and peace movements in three countries—the United States, Switzerland, and Italy—as well as changes in policy for these areas. The second goal is to advance a set of theoretical arguments concerning the impact of social movements on public policy. Finally, the book attempts to demonstrate the value of these arguments by running them through a series of empirical tests.

On its own terms, the book is largely successful. Its early chapters provide focused descriptions of these movements and overall changes in ecology, nuclear, and military policy in the period from 1975 to 1995. These discussions are anchored by an analysis of protest data collected from the major dailies of each country and available data on their state expenditures in each of the policy areas. These chapters also indicate that there is at least a surface connection between movement collective action and ecological and nuclear policy.

More important, Giugni also provides a crisply argued explanation for the impact of social movements on state policy. He sensibly does

not try to explain all the possible consequences of social movements, under the premise that many micro and cultural consequences are likely subject to different determinants than those affecting state policy. Specifically, he posits that social movement mobilization and activity will influence policy under specific circumstances. One is that the movement must be acting in a domestic policy area. On matters of national security and war, states are much less vulnerable to social movements. Thus, he is skeptical that peace movements will be influential. He also builds on political mediation ideas by arguing that in domestic areas a social movement's actions will gain greater results when there is a favorable regime in power. Finally, he takes public opinion into account by arguing that favorable views on an issue will also amplify the influence of a movement's collective action.

As for demonstrating the argument, *Social Protest and Policy Change* has several advantages over standard social movement research, which is usually based on the case study. Giugni's approach is multiply comparative, addressing the impact of three movements across three countries. More than that, he gathers time series data pertaining to his arguments and state expenditures for each of the three policy areas. Giugni deploys the protest event and expenditure data in time series regression analyses, testing his theoretical arguments with a variety of statistical interactions. Although the statistical analyses can be only tentative, given the few number of cases, missing data, and the inability to control for alternatives, the author's arguments receive key empirical support.

The book is not without its problems. It takes a narrow view of policy, focusing only on spending, which has been shown in studies of the welfare state to be insufficient to capture state activity. Moreover, although the author's point is well taken that movements have an uphill battle to influence national security policy, I wanted to hear the author's ideas about the conditions under which this could occur. Also, the book might have taken more advantage of its comparative and historical framework. The author notes that he is engaged in a "parallel demonstration of theory," which means he deploys the cases as

simply additional data, and his conclusions rely almost wholly on the time series analyses. There is no discussion about how macro conditions across cases might influence the causal relationships within the cases, and the author also misses the opportunity to make process-based theoretical arguments and address possible path dependencies in movement influence. Finally, I wish the author had updated his useful review of the literature, instead of reprinting it, as there has been a lot of work in this area over the last six years.

But these are minor points. *Social Protest and Policy Change* is an impressive study that addresses an important subject, thinks hard about it, takes an innovative methodological approach, and provides systematic research. All scholars concerned with the policy impacts of social movements, and new social movements generally, will want to read this book. It has been standard to say, as the author does, that our theoretical and empirical knowledge is relatively lacking regarding the impact of social movements. But with studies like this one, and others published in recent years, that claim is now outdated. Those of us working in the area will have to develop a new hook for our work.

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Jeff Goodwin and James M. Jasper, (eds.). *Rethinking Social Movements: Structure, Meaning, and Emotion*. Lanham, Boulder, New York, Toronto, and Oxford: Rowman & Littlefield Publishers, 2004. \$75.00 (cloth), \$ 32.95 (paper).

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Jeff Goodwin and James M. Jasper's book, *Rethinking Social Movements*, is a collection of debates and reflections on the future directions for the theories of social movements and collective action that were played out in different publications. By putting together variegated culturalist renderings of the political opportunities approach, together with some defenses by its proponents, the editors hope that "the debates and reflections in this volume raise more questions than they answer, bringing theoretical reflection back to a field that has had too little of it in recent decades" (p. x).

The book was born out of the culturalist critique of the political opportunities approach that the editors originally published in *Sociological*

*Forum* (March 1999). Although not the first to point out the limitations of the political opportunities approach, theirs was the most sweeping in scope and dismissive of its analytic merit. According to them, the structuralist and invariant layouts, among others, of the political opportunities processes render the theoretical program tautological and analytically inadequate for better understanding human agency and cross-case complexity. Their critique provoked fierce responses from prominent political opportunity theorists such as Charles Tilly, Sidney Tarrow, and David S. Meyer. Their counter critiques and Goodwin and Jasper's reply, together with Ruud Koopmans' rational choice theoretic formulation of political opportunity models, were all printed in the same issue of the *Sociological Forum*.

The debate continued in other publications. Influenced by the editors' critique, Doug McAdam, a founding father of the political opportunities approach, attempted to reformulate his famous political process model in the second edition of *Political Process and the Development of Black Insurgency* (1999). This time, he placed the interpretative processes at the center of the dynamic, sequential loop from political opportunities to mobilizing structures to mobilization and back to political opportunities. In another publication (*Contemporary Sociology*, May 2000), Aldon Morris picked up the debate and criticized the political opportunity centered temporal sequence—initiated by the opening of political opportunities and followed by the activation of challengers' mobilizing efforts. Morris posits a reciprocal relationship between political opportunities and mobilizing structures with a greater analytic role assigned to the latter. In the same issue, Myra Marx Ferree, although not explicitly concerned with this debate, proffered the feminist "lessons" that emotions and value commitment are the crucibles for addressing the neglected issue of human motivations in the cognitively biased framing analysis.

In further response, a number of scholars further joined the debate. Those sympathetic with the editors' culturalist rendering of the political opportunities approach pushed the debate toward establishing a culture centered theory of social movements. Some, such as Richard Flacks, Marshall Ganz, and Debora B. Gould, picked up the specifics left unaddressed in the first round and offered research propositions that they believed could remedy the structuralist, agent-less, and invariance bias in the political opportunities approach. Others attempted to synthesize the struc-

turalist and the culturalist orientations: While calling for overcoming the dichotomous conception of structure versus culture, both Francesca Polletta and Marc Steinberg recognized the cultural dimension of structure and the structural dimension of culture simultaneously.

By bringing together these debates and reflections by scholars of different theoretical orientation, the book provides, as the editors hoped, a rare moment of reflection on the status of contemporary social movement theories. In the process, it pushes the readers to ponder how to theorize across seemingly disjointed incidents of social movements and collective action. In a field dominated by empirical case studies, the book is an overdue yet welcome challenge for developing parsimonious yet realistic theories of social movements and collective action. Nevertheless the book disappoints because the search for a Mertonian middle-range theory of the political opportunities and culturalist framing approaches is lost in the process of sided debates. What constitutes the key causal mechanisms that makes an account of contentious politics distinctively political opportunities theoretic? Does there exist only one or multiple mechanisms that with varying salience link social movements and political environments, thus giving rise to seemingly contradictory associational patterns between state reactions and contentious politics? If so, what is the underlying model of human action and interaction that gives conceptual coherence across seemingly disjointed causal mechanisms? With the exception of Koopmans, the proponents of political opportunities largely fail to provide an articulation of middle-range theory that would guide efforts to incorporate culturalist insights into the political opportunities formulation.

A similar failure is found in the culturalist alternatives. In her elegant recapitulation of the emergence of ACT-UP, for example, Deborah Gould contends that the paradoxical invigoration of movements by state repression could be resolved if emotions are viewed as motivational forces for activism. Thus, repression can induce changes in the emotional landscape of an affected group, of which some militant activist groups can take advantage. Convincing as it may sound, one cannot help but wonder why many gay men and lesbians opted not to join the ACT-UP militancy? Were they less angry at the repressive state action? Or, were they just as angry but had a differing strategic understanding of the situation and thus "rationally" pursued alternate forms of activism? A balanced understanding of the emo-

tional dynamics that might mediate between the state and the challengers calls for its own middle-range theory of human action and interaction. The common failure by the proponents of these two alternative perspectives ironically further emphasizes the theoretical impoverishment the editors intended to address. In the absence of the middle range theoretic specifications of mechanisms and hypotheses, abstract metatheoretical debates over the structuralist limitations of, and culturalist remedies for, political opportunities theories lead us nowhere.

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Carol J.C. Maxwell. *Pro-Life Activists in America: Meaning, Motivation and Direct Action*. New York: Cambridge University Press, 2002. \$65.00 (hardcover), \$23.99 (paperback).

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Social movement scholars have a habit of studying the movements they love or fear, and their research often points to conclusions that serve either to support the strategic calculations of the movements or the forces of social control arrayed against them. Against this backdrop, Maxwell's psychological-anthropological inquiry into what brought some people to obstructing abortion via direct action (sitting in, picketing, vandalizing clinics, and confronting their clients) is surprising. It is sympathetic to the protestors without being supportive of them or their tactics, and it offers little by way of strategic insight for either helping or controlling future protests.

Instead, Maxwell tries to understand the way that these protestors see the world and how the particular processes of meaning construction in which they engage and personal needs that they have, make participation in such protests seem sensible to them. She excludes the violent protestors who so often symbolize all protest action against abortion, but does focus on those who are engaging in direct action at the clinics (risking arrest via civil disobedience), rather than just holding an attitude against abortion. The study is based on her own intensive interviewing of a snowball sample of 80 activists in St. Louis, analyses of comparison samples from a larger group of members of a Missouri antiabortion organization and Wichita protestors, and observations of protests in St. Louis, Wichita, and

Washington D.C. Her approach is based on a cognitive theory of motivation and a biographical method of interviewing.

Her analysis, however, is not purely individual. It is greatly enriched by how she looks at change over time as important both for the movement as a whole and for the individual activist. Thus, Maxwell offers an excellent account of how Operation Rescue (OR) emerged from a pre-existing but quite different network of activists—people who were more Catholic than evangelical, often leaning more to the political Left than to the Right, and for whom civil disobedience was a form of personal witness rather than a media strategy. She traces the change in the protest movement against abortion, both locally and nationally, through three phases. In the transition from the first low-key phase to the mass protests of OR, she notes that what changed was who was active, not the feelings of the relative few whose activism spanned these periods. For the period of demobilization after OR, she emphasizes the increased cost of protest as deterrent, especially for the Catholics who were urged to engage in other forms of activism than protest.

Although her distinction between the changes at the individual level and at the organizational one is well done, sociologists and political scientists will be disappointed to see how little of their research on activist organizations informs her work. Maxwell's use of the distinctive personal features of the actors' lives as explanations of their political choices would, I think, make her sympathetic to my own assessment of this surprising lack of organizational theory—or for that matter, of references to most of the sociological literature on movements—as a reflection of her having done the dissertation research for the book at Washington University in St. Louis, an institution notable for having closed its sociology department in the 1970s. This loss, along with her own disciplinary affiliation in anthropology, has limited her ability to draw on studies that would have helped to fill in the mesolevel gaps in her analysis.

There is, nonetheless, much for sociologists to learn from Maxwell. She offers a richly nuanced picture of how activists move in and out of participation in risky protest activities. For example, she indicates that for some women activists, strong negative feelings about their own abortions may not only contribute to a relatively short period of intense activism as they make sense of their own experience, but can also con-

tribute to other activists' perceptions that women are "harmed" by their abortions. Since the activists tend to see not only the women with the most regrets but also interact with them precisely in the period before these women resolve the issue emotionally, they see support for their beliefs that women who have had abortions are guilty and in pain.

Maxwell also puts moving in and out of protest into a wider social psychological profile in which the expressive motives (witnessing to "evil" and worshipping God) vie with more instrumental ones (effectively stopping abortion). This leads to interesting and counterintuitive findings in which expressive motives can be more easily satisfied and lead to only transient action, while longer term participation in protest is related to seeing it as "the only way" to stop abortions in practice.

Maxwell's thoughtful analysis of how the meaning making of men and women protestors differs also avoids invoking biological essentialism or assuming that women are the misguided dupes of men. Lacking C.W. Mills's concept of a vocabulary of motives, she nonetheless shows it is the difference in vocabularies between women and men that is most striking, with the women turning to a language of grief, care, and loss and the men to warfare images as they explain their motivations to protest. But she also stresses gender similarities, as in the way that experiences of being an unwanted or unloved child (or feeling oneself to have been) promote identification with the fetus for women and men. She points out how the nature of the prolife movement itself, as well as women's lives in general, have changed in the more than two decades since Kristen Luker's influential book, and suggests that prolife activist women today are not different in social class or labor force experience than those who are activists on the other side. Overall, though unusual in many ways, this is a book that offers much that students of movements in all disciplines will find useful.

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Malcolm J. Todd and Gary Taylor, (eds.). *Democracy and Participation: Popular Protest and New Social Movements*. London: Merlin Press, 2003. \$29.95 (paperback).

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The introductory chapter of *Democracy and Participation*, written by Todd and Taylor, asserts that representative democracy is in crisis. Since the mid-twentieth century, voter participation has steadily declined in Western societies. The low turnout in contemporary democracies goes along with a weak party identification, a lack of trust in political representatives, and a deep crisis of political identities and ideologies. The collapse of the communist block, weakened traditional political markers in European politics, the political vacuum left by the Left, the adoption by politicians of a technocratic and managerial language, and many other changes have widened the gap between voters and politicians and led citizens to withdraw from politics. This observation brings the editors of this volume to raise the core question: Is “old politics,” as they call it, exhausted and replaced by a new form of politics. They speculate as to whether Western societies, Britain in particular, bring new forms of politics or whether they are becoming antipolitical societies. The purpose of the book is to provide a discussion on the current state of representative democracy in Britain. Specifically, its aim is to grasp the diverse forms of political expressions of British citizens. In reality, most of the contributions focus on protest politics. Todd and Taylor bring together a large number of scholars who discuss the mobilization of the so-called “new social movements,” antiglobalization, feminist, antiracist, peace, protest, as well as, anarchism, unionism, and voluntary associations in contemporary British society. The volume also addresses youthful political participation in Britain. Young citizens are less positively inclined towards the political process, but are they as alienated as we thought? *Democracy and Participation* thus examines various political and protest fields where citizens are active and are renewing democracy.

And what do we learn from this book? It is difficult to wrap up all the contributions in few sentences, as they are rather dispersed and lack a unifying thread. All the chapters discuss the issue of participation and democracy, so they clearly are on the same intellectual voyage, but they are so disparate in both their comprehension of democracy and the link between both concepts, that it is unclear whether the authors are addressing the same phenomenon. Nevertheless, the authors do grapple with various interesting ideas on the state of protest and political participation in contemporary Britain.

First of all, it seems obvious that there is no political withdrawal. British citizens and sections

of the British society are very much involved in politics. Many sectors of the society are organized and politically engaged. Protest not only mobilizes citizens, but also defends and promotes rights and social equality. Binding and protected consultation to enlarge citizenship rights and social equality seem to exist. According to McAdam, Tarrow, and Tilly’s view (*Dynamics of Contention*, 2001), these conditions should strengthen democracy.

Second, diverse social spheres are politicized. In line with a Foucauldian understanding of power, politics colonizes new spheres of society. Politicization emerges from different social spheres and contenders struggle with sections of social power for promoting rights and equality. Feminist protest, which politicizes the private sphere, and the mobilization of mental health service users converge with Melucci’s (*Nomads of the Present* 1989) idea of a democratization of everyday life.

A third observation that comes from the reading of this collection of chapters is that “old politics,” defined by the authors also as mobilization based on old class cleavages, faces important difficulties for mobilizing citizens. In Britain, unionism is in decline. Labor’s struggle against unemployment has encountered many difficulties and was largely unsuccessful in 1980s in challenging Thatcher’s politics when compared to the early twentieth century. In addition, the defense of people at work seems to follow independent tracks, as shown by anti-privatization protests, which organized outside the traditional forms of political expression (unions and parties). Linked to the apparent decline of traditional forms of political expression, a fourth conclusion can be drawn from the edited volume and reveals a certain paradox of representative democracy. While unionism as well as partisanship are in decline, and the young generation is increasingly critical of politics and tends to withdraw from its formal expression, youth are paradoxically still committed to the idea of election and representative democracy. This is a positive omen for the future of democracy.

Fifth, alternative forms and ideals of democracy are conveyed from contemporary protest activities. The model of participatory democracy and anarchist ideas of democracy contribute to a critical rethinking of the dominant model of representative democracy. As the editors put it “a common theme that recurs in these chapters is the belief that representative democracy is funda-

mentally limited in its ability to reflect the needs and desires of the citizens in contemporary Western society.” Finally, by reading all these contributions, we see that social movements at the present time belong to “normal politics.” They have become a stable element in contemporary Britain, they represent a wide and plural range of claims, and most of them are institutionalized, becoming an instrument of conventional politics. In other words, the British are living in what Meyer and Tarrow call, a “social movement society” (*The Social Movement Society*, 1998). Protest enlarges politics in various social spheres and strengthens democracy by mobilizing citizens, promoting rights and equality, and creating dense networks among citizens which is in Tocqueville’s words “a fabric of the community life.” In sum, citizens are not merely spectators who vote, as Walzer (*The Civil Society Argument*, 1992) said, but active contributors to democracy.

While bringing interesting ideas to the fore, the book also has serious problems. One problem is confusion in the authors’ understanding of democracy. As we all know, the concept is complex, subtle and polysemous. To assess the link between participation and democracy, we cannot avoid a serious theoretical discussion of the model(s) of democracy to which we refer. If we are not doing that, we remain on a rather vague and superficial level about the link of democracy and participation. Moreover, we always flirt with a normative discussion without serious theoretical and philosophical anchorage. When we make precise what we mean by democracy and focus on its specific features (increase rights, promote liberty, facilitate political access to win binding and protected consultation, etc.), we can really grasp the links between participation and democracy. This specification allows scholars not only to grasp the mechanisms at stake and assess the role of social movements in strengthening democracy, but also to underscore the conditions when participation actually promotes democracy.

In line with the previous criticism, we find in the book a common assumption that protest is synonymous with democracy. Contentious processes can strengthen democracy not only because people participate in politics, but because they make claims in its name. However, there are a number of cases where protest movements make antidemocratic claims, promoting severe rights and equality restrictions: the struggles of the Spanish Republic leading to Francoism, the pro-

test of German National Socialism, and more recently the mobilization of skinheads groups and racist associations demanding drastic rights restrictions for migrants and ethnic minorities. In an other vein, what of the “White Marches” struggling against pedophilia taking place all over Europe that demand severe civil restriction for perpetrators and death penalty restoration—are they promoting democracy? Despite the issue at stake and the urgency of protecting children, we may wonder if their participation in politics and their claim making are synonymous with democracy.

Analyzing the link between democracy and participation we need also consider who participates in politics. As we all know, participation in elections is unequal. Certain social categories are systematically excluded from democratic consultations, specifically the less socially, economically, and culturally equipped. Is it the same with protest politics? We cannot avoid such a discussion once we analyze the link between participation and democracy. Nevertheless, *Democracy and Participation* opens many stimulating research perspectives for social scientists. It establishes the agenda to which students of collective action will have to work in order to better evaluate the complex links between participation and democracy.

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Rick Fantasia and Kim Voss. *Hard Work: Remaking the American Labor Movement*. Berkeley, Los Angeles and London: University of California Press, 2004. \$50.00 (cloth), \$19.95 (paper).

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*Hard Work* is the expanded, English language edition of a work published in 2003 in France as part of Bourdieu’s *Raisons d’Agir* series. The series, according to the authors’ preface, is recognized as providing “politically provocative and analytically rigorous intellectual interventions” into the public debate over the social costs of neoliberalism. *Hard Work* does not present new data or analysis, in the narrow, social scientific sense. Rather, Fantasia and Voss have written a book intended to be accessible to the nonspecialist about “what we think about [U.S.] labor and what we think others ought to

know about it" (p. xiv).

The authors make three central claims. First, in opposition to current American triumphalism they argue that the U.S. does not exhibit superior living standards or economic performance and that most western European workers and their families are, in fact, better off than their American counterparts. Secondly, they claim this is "in large part because a historical weakness and narrowness of vision have prevented American labor from effectively challenging the power of U.S. capital" (pp. 2-3). Third, they allege that the U.S. labor movement is undergoing "a fitful reinvention" and "is now positioning itself . . . to become what it never has been before—a genuine counterweight to the power of U.S. capital" (p. 3). In my view, they succeed in establishing the first claim entirely, the second claim partially, and the third claim not at all.

Fantasia and Voss cover an enormous amount of territory in four chapters and a brief conclusion. They present a thorough overview of American economic performance and living standards in comparative context and a history of American industrial relations from the late nineteenth century to the present in the first two chapters. Chapter 4 provides a nice summary of the two contemporary labor struggles—the Justice for Janitors campaign in Los Angeles and the efforts to organize hotel and casino workers in Las Vegas—that are the basis for the authors' cautious optimism. (The "hard work" in the title refers to the effort and militancy required to make their optimistic scenario a reality.) Moreover, to their credit, the authors' attempt to integrate institutional, social movement, and cultural perspectives, rightly noting the critical role of "solidarity" in constructing a labor movement out of individual unions and wage workers.

That said, I found two glaring weaknesses with the analysis. The authors trace the apparent absence of a militant movement culture within U.S. organized labor to the late nineteenth century—specifically, the destruction of the Knights of Labor in the late 1880s by organized employers with the assistance of the state. They argue, "The downfall of the Knights . . . underscored the exceptional social power of employers and served to solidify the position of a much more conservative 'business unionism' as a rival to the Knights' egalitarian social unionism. . . . The simple lesson . . . was unmistakable: broad-based organizing and radical politics would be soundly repressed" (pp. 37-8). Even the CIO, which the authors claim "remade" the American labor

movement in the 1930s, "never escaped the imprint of the forces that shaped its formative years and that continue to mold it today" (p. 35). When they thereafter imply equal culpability for the absence of "social movement unionism" in the U.S. to a "narrowness of vision" among labor leaders emerging out of such a context—notwithstanding numerous exceptions to the contrary that go unmentioned and their own graphic evidence (see p. 71) that shows a substantial willingness by U.S. unions to confront capital lasting through the mid-1970s—this seems like misplaced causation. And then they argue, without seriously addressing contemporary contextual constraints every bit as hostile to organized labor as earlier ones (employer power, state indifference, globalization, and the absence of a compelling left ideology to name just four), that the reemergence of a real American labor movement can be attained if only American labor leaders work harder and adopt a movement culture. I was not persuaded.

Second, the analysis is unnecessarily elitist. The only actors given serious attention are the new leadership of the AFL-CIO and the handful of university schooled union organizers and researchers that have, in recent years, assumed central positions in some service sector unions. As for the rank and file, Fantasia and Voss declare, "members of most unions in the United States have never gone through any sort of process of preparation to predispose them to experiencing unionism as a social movement, or indeed to experience it as anything other than an occasional series of necessary bureaucratic procedures performed by a designated staff of trade union functionaries" (p. 167). This sounds much more like the 1970s than today, when virtually all the remaining private sector union members have a deep understanding of the battlefield and are eager to employ any and every strategy with a chance of success. Unlike Fantasia and Voss, however, they have a much clearer sense of the odds against them, and even so are willing in many cases to risk their livelihoods to preserve their dignity.

I agree with Fantasia and Voss's contention that creating a revitalized American labor movement will require a healthy dose of militant radicalism. But they have not made a case for how it could happen. Militant radicalism in an unwinnable context is nothing more than suicide, as any number of labor struggles over the last three decades can attest. Further, claiming as their central objective "to clearly—and without

euphemism—uncover the social logic of American labor” (p. xv) is misleading and unhelpful, as such unitary logic is not evident. What continues to be most distinctive about organized labor in the U.S. is its decentralization, variation, fragmentation and, ultimately, isolation.

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Robert M. Fishman. *Democracy's Voices: Social Ties and the Quality of Public Life in Spain*. Ithaca and London: Cornell University Press, 2004. \$29.95 (cloth).

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The debate on the foundations of democracy has decidedly returned to the forefront of the agenda of sociologists and political scientists. Much influenced by the debates on social capital, many sociologists have returned to questions about the social bases and character of lived democracy. Some of the most exciting work is taking place among those who import ideas about social movements and collective action, such as the importance of ties, structures of opportunity, and of collective identities, into a literature that too often brackets “citizenship” and “social movements” as if they were discrete phenomena. Taking as his case working-class communities in Spain, Robert Fishman has written a beautiful book that makes a simple, but worthwhile, point: the quality of democracy is improved by boundary crossing social ties. Fishman draws on a survey of working-class community leaders in medium sized municipalities throughout Spain and complements it with interviews and a focus on two such communities. The more boundary crossing ties that these leaders have with intellectuals, the more likely they are to engage in “globalizing rhetoric”—a kind of democratic talk that is broad-based and engaging, rather than parochial and disengaged.

Perhaps in the spirit of democratic discussion itself, the book is presented almost as a conversation rather than a traditional case study. After introducing the book’s main intellectual interlocutors, Gramsci and de Toqueville, and presenting us with some context on Spain’s transition to democracy, Fishman succinctly makes his causal argument early in the book. Workers in mining towns have had their interests under siege and actively defend them, but do so in either

“defensive localist” ways or through a more inclusive discourse. Where there are extensive ties between intellectuals and workers, political discourse is more likely to be expansive, globalizing, and attentive to a variety of local and national themes; where those ties are thin, it is more likely to be narrow and parochial. In the mining valley of Caudal, where such ties are thin, the effort to keep a particular mine open had a particular political strategy that was focused on the particular mines, and prioritized “the local, the singular.” In contrast, in the neighboring mining valley of Nalón, the effort was one for a sustainable reindustrialization, and keeping mines open was connected with a variety of issues, from the environment to women’s role in the mining industry.

The following chapters are devoted to answering questions readers might have. Is this basically a social capital argument? (No!) What might be alternative explanations for these patterns in the data? How can one generalize from this case? And, what are its lessons for other democracies? One of the book’s strengths and principal contributions is precisely its carefully argued theoretical case. When considering what it is that improves the quality of democracy, Fishman is attentive to culture, context, and agency. In fact, this *is* a book about the culture of democracy if one considers patterned practices as cultural and the way in which particular contexts are more or less conducive to democratic dialogue. Among post-communists in Spain, boundary crossing ties improve the quality of democratic dialogue because these ties are not instrumental in nature, whereas among those linked to the Socialist party, those ties do not improve discourse in the same way because they serve as brokerage functions to the party’s governmental apparatus. Thus the book avoids both a romanticization of democratic discussion or the reductive institutionalism that dominates some of the literature, while also offering something much more nuanced about the nature of ties than the win-win proposition of social capital theories.

The book also raises a number of provocative theoretical questions that the continuing discussion on the nature of democracy, perhaps a discussion that is not nearly globalizing enough, can address. One concerns the impact on democratic discussion of settings—relational contexts that bring together different sorts actors embedded within social networks. The literature certainly evokes many different settings for discussion in concepts of free spaces, forums, public

sphere, publics etc. Is it possible to imagine types of settings that are more conducive to globalizing discussion, and would it be possible to make similar types of causal arguments that Fishman brings up? Do actors that are more or less predisposed to expansive discussion present themselves or engage differently in different settings? Does what counts as proper discussion within such settings include globalizing or parochial discourse? Another set of questions concerns distinctive political cultures. Do different political cultures value globalizing or parochial discourse differently? As the concept travels, does the dichotomy hold or are there other, hybrid forms within distinct political cultures?

Of course, these are questions for scholars who take up Fishman's challenge to continue to search for the elusive sources of quality of public life. This powerfully argued and elegant book certainly takes us some steps closer to finding them.

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Vincent J. Roscigno and William F. Danaher. *The Voice of Southern Labor: Radio, Music, and Textile Strikes, 1929-1934*. Minneapolis and London: University of Minnesota Press, 2004. \$59.95 (hardcover), \$19.95 (paper).

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Despite conventional wisdom that southern mill workers were historically passive and conservative, they mobilized in impressive numbers and walked off their jobs in Marion and Gastonia, North Carolina in 1929, and then again throughout the South in 1934. *The Voice of Southern Labor* richly describes and convincingly explains these events. As Roscigno and Danaher discuss, mill workers' oppositional consciousness grew out of their preexisting cultural tradition of mountain folk tunes and gospel songs. Music was an integral part of mill-town life. People listened to and performed songs at work and during leisure time activities. Listening to and performing songs were emotional rituals that helped workers appreciate their roots and reestablish feelings of solidarity and group membership. Even though most of the songs were not mill-specific, they still facilitated collective action and provided strength during struggles because of those functions. However, mill-specific

songs became increasingly popular during the late 1920s and early 1930s, and the authors identify three important themes in their lyrical content that fueled insurgency among mill workers. First, mill-specific songs typically communicated a sense of collective experience and suffering. Second, they frequently specified that the greedy and malicious mill owners and managers were the causes of their misery. Last, the majority of mill-specific songs emphasized that there was a collective political solution—strikes and union mobilization—to their suffering.

In all three strikes, the authors show that indigenous music and song, rather than external union mobilization, radicalized mill workers and spurred collective action. Union involvement tended to be modest in these strikes, and generally occurred only after mill workers actively sought the support of unions. Because Marion and Gastonia were hubs of music and musicians, mill-related songs were especially pervasive in these mill towns. Roscigno and Danaher document how these songs solidified and heightened the already existing native radicalism and oppositional consciousness in Marion and Gastonia. Additionally, they describe how mill workers garnered strength and solidarity from music during strikes, singing and dancing at nightly meetings, during parades, in jails, and at funerals. Yet the decline of significant resources, court injunctions against the striking workers, and violence from mobs, militia, local deputies, and National Guardsmen proved to be too much for the workers to overcome, and the 1929 strikes in Marion and Gastonia ended in defeat.

In 1934, mill workers again mobilized and carried out the largest strike in the history of the South. Four hundred thousand mill workers went on strike throughout the region in hopes of ending their oppressive working and living conditions. Roscigno and Danaher demonstrate how the proliferation of southern radio stations between 1929 and 1934 played a central role in this broad based movement. With the founding of more and more southern radio stations during this era, "hillbilly music," by then all the rage, traveled beyond particular mill villages to mill villages across the entire region. The authors note that the growth of radio stations also increased occupational demands for musicians, many of whom were ex-mill workers. Responding to these demands, musicians formed networks, played on live radio broadcasts, signed recording contracts, and traveled from mill town to mill town performing and entertaining. Roscigno and Danaher

document that this dissemination of mill related music widely intensified workers' oppositional consciousness and gave them a larger sense of purpose, community, and action. They also specify how the growth of radio during this era helped alter mill workers' perceptions of political opportunity. Politicians increasingly utilized the radio to reach their constituents in this period. President Franklin Delano Roosevelt was no exception. During the president's broadcasted Sunday "fireside chats," he identified, sympathized, and sided with southern mill workers. Because of these broadcasts, mill workers now believed, as they expressed in thousands of letters to Roosevelt, that the federal government would support them, and that they had a legitimate and legal right to organize collectively when exploited.

The authors also discuss how the mill workers' indigenous culture of music and song led to two important tactical innovations during the strikes of 1934. The first innovation was the "dancing picket." As a way of blocking mill entrances, fostering solidarity, reducing fear and boredom, and showing opponents that they were nonviolent, mill workers, often led by musicians, sang and danced as they picketed. The second innovation was the "flying squadron." In hopes of facilitating greater mobilization, intimidating mill owners, and conveying an intercommunity spirit, cars or trucks filled with singing and shouting groups of mill workers traveled to villages where mills were still operating and either encouraged fellow workers to walk off the job or ran into the mills and shut off machinery. National Guardsmen and local police tried to stop the picketing and flying squadrons by threatening, fighting, imprisoning, injuring, and even killing mill workers. Once this occurred, union leaders, though it is not clear exactly why, ordered the flying squadrons to stop. Roscigno and Danaher describe how this demoralized the striking workers and broke their broad based unity. Roosevelt soon proposed the Winant Settlement and the union leaders accepted, officially ending the strikes. Importantly, the authors point out that, although perceived favorable political opportunities helped galvanize workers to organize and to strike, objective, favorable political opportunities in the form of federal protections were crucial in winning the battle.

To conclude, *The Voice of Southern Labor* is an excellent book that significantly contributes to various literatures, most notably social movements and collective behavior, labor studies, me-

dia and communication, and southern history. Roscigno and Danaher write in clear and lively prose throughout, and their inclusion of quotations, lyrics, and pictures at various points in the book brings human faces and voices to their description and explanation.

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Chris Rhomberg. *The There There: Race, Class, and Political Community in Oakland*. Berkeley: University of California Press, 2004. \$39.95 (hardcover).

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Odds are that Chris Rhomberg did not set out to write a social movement book; and odds are that even when he finished he did not think he had written one. But it is a book that all social movement scholars should read and maybe even emulate, because it is an exemplar of what we ought to be doing.

The virtue here is that Rhomberg has definitively broken down the conceptual and disciplinary barriers that have relegated social movements into an intellectual ghetto. Precisely because he has not focused on just the social movements that have populated Oakland's political and economic history, Rhomberg can see the role they play in larger social dynamics—both as responses to social, political, and economic exclusion and as the social agency for social, political, and economic reform. And because of this integrated vision, he allows us to discern the forces that determine the temporary and permanent impacts of social movements on social structure.

The basic structure of the book is simple. Rhomberg divides the history of Oakland into four periods based on the nature of the political regime that dominated local life. He then focuses his attention on the three transitions between these regimes, analyzing the underlying structural dynamics, the strengths and weaknesses of the dominant political alliance, and—most important for social movement scholars—the application of pressure by excluded groups for access to the inner sanctums of leadership and for concrete new programs. The overarching portrait is one of apparently stable structures weakened by the maturation of their own contradictions and challenged by the rise of powerful social movements representing outsiders; this leads to a pe-

riod of political, economic, and ideological disarray that ends only when a new regime is constructed from an unpredictable mixture of the combusted ingredients.

Tracing this pattern through three whole cycles yields many insights—some quite dazzling—that offer a fresh understanding of the origins, dynamics, and impacts of social movements—because Rhomberg scrutinizes all the systems at once. He never allows his attention to narrow onto the pushing and pulling within the political system (even in the early period when politicians truly ran the city), or onto the apparently inexorable economic forces (even during the depression when economic primacy is assumed by most authors), or onto the social movements that disrupt normal life (even during the Oakland general strike after World War II). As a result of this intellectual balance, Rhomberg and his readers are rewarded with detailed insights and general propositions that can be applied to the broadest range of intellectual projects—and especially the study of social movements.

One example will suffice to demonstrate the virtue of his method and the excellence of his scholarship. During the 1920s, the national revival of the Ku Klux Klan swept through Oakland; and the Klan became a central player in the destruction of the regime based on political bossism. Viewed through Rhomberg's analytic lens we discover some remarkable things about the Klan. First, it was based in the rising middle class and not in the threatened working class. Second, its main appeal was its determination to remove corrupt scoundrels from city hall—the ferocious racism was an important adjunct, but not the foundation of its popular strength. In fact, the Klan collapsed precisely because its leaders, once elected to city office, succumbed to the seduction of corruption. Third, the most visible legacy of the Klan in Oakland was not middle class domination of local politics. Instead, the destruction of the political machine, combined with the corruption scandals, left the movement exhausted. What emerged from this collapse was the ascendancy of downtown business, with the middle class a junior partner in a new urban coalition.

But this is not just a fascinating piece of nonintuitive history; it also has broader implications for our understanding of social movements. In particular, we are alerted to two fascinating larger propositions. First, regarding social movement dynamics: the high visibility banners of social movements (the racism of the Klan)

may not be the key to their rise (prevalent corruption in local politics), or even the main thrust of their programs (middle class inclusion in local politics). Second, regarding the impact of social movements: the most highly visible consequences of social movements (the collapse of the political machine) may be far less significant than their less visible and even unintended consequences (the creation of a new regime dominated by downtown business).

And this is only one part of the first of three transitions. Rhomberg has equally lucid things to say about the post-World War II transition animated by a huge labor movement, where we get very different perspective on the origins and impact of “business unionism.” And he offers the first treatment of the Oakland Black Panthers as an integral part of the forces that dismantled the military commercial complex captained by the Knowland family, replete with a unique interpretation of the impact of Bobby Seale's mayoralty campaign.

It may be an expression of the usefulness of Rhomberg's intellectual architecture that it fails to conceal—or perhaps calls attention to—a variety of analytic problems, missed opportunities, and suspicious conclusions. An illustration of these is found in his treatment of the racism of the Ku Klux Klan. On the one hand, he shows that it is a decidedly secondary aspect of the Klan's entry into the larger system, but then he swings too far in the other direction—neglecting to offer a serious analysis of how the Klan's racism articulated into the new system that emerged. We know, from the work of Massey and Denton and others, that the 1920s was the seminal moment for the ghettoization of Blacks in the North. Rhomberg missed an opportunity here: he could have explored the processes by which the Klan in Oakland, while focused on capturing and reforming city government, actually provided the popular base and perhaps created the institutional foundation for constructing the edifice of housing segregation that even today is fused into the foundation of urban life in Oakland and elsewhere. And by failing to do this, he leaves the reader with an incoherent understanding of the role of racism in social movements and in social change.

In each of the other transitions, we are faced with similar, high visibility, loose ends. Rhomberg's discussion of unions is immeasurably enriched by his consistent placement of them within local sociopolitical contexts. But he seems to suffer a strange lapse when he treats the split

between the AFL and the CIO as though its origins were strictly national, neglecting to analyze the ethnic and political differences that animated the split on the ground in Oakland. Or, during the 1960s and 1970s, when Black professionals finally carved a niche within the urban regime, Rhomberg's portrayal of the Black Panthers as an integral part of the dynamics of the era falters when they suddenly disappear from his narrative during the critical negotiations over the Model Cities program.

To my eye, these lapses are the inevitable consequence of pioneering a new perspective. Rhomberg is probably covering too much ground—he could have written a whole book about each of his transitions without exhausting his supply of important insights. By covering this much larger ground, he gives us a better sense of the richness of the method and the usefulness of the perspective, even if he misses some crucial points along the way.

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Jeffrey Cormier. *The Canadianization Movement: Emergence, Survival, and Success*. Toronto: University of Toronto Press, 2004. \$65.00 (cloth).

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This carefully researched book makes a useful contribution to both the sociology of social movements and the interdisciplinary field of Canadian Studies. Its focal point—the cultural politics of “Canadianization”—comprised an important point of distinction between the 1960s protest wave in Canada and its counterpart in the United States. Canadianization was a movement for cultural sovereignty formed in opposition to the colonizing tendencies of the American-based culture industries and academy. For sociologists familiar with the discipline on both sides of the border, Cormier's analysis helps account for the more idiographic, methodologically eclectic, and socially critical style of sociology that in the 1970s took root north of the forty-ninth parallel.

Indeed, much of Cormier's analysis hones in on the “bureaucratic insurgency” that occurred within the Canadian Sociology and Anthropology Association (CSAA) in the 1970s, after Canadianization fell into abeyance early in the decade. The first stirrings of the movement were largely,

in Cormier's account, the work of Robyn Mathews who, with his colleague James Steele, played the indispensable role of movement entrepreneur—developing in the late 1960s a resonant collective action frame that defined as a social problem the growing presence in Canadian universities of foreign professors and non-Canadian curricular content. However, despite successes in building informal networks and raising consciousness among academics and the general public, the initial phase of white-hot mobilization did not equip the movement with a viable organizational infrastructure.

At this juncture, around 1972, the story takes an interesting turn. Cormier recounts how a group of young scholars radically transformed the CSAA and in the process successfully lobbied university administrations, the federal government, and other relevant bodies to enact regulations promoting Canadianization. In effect, the movement “migrated” from the humanities (Mathews and Steele being professors of English) to the social sciences, and particularly to sociology. Cormier shows how the CSAA's hierarchical organization at the time made bureaucratic insurgency a viable strategy, as several Canadianizers took up key positions from which they could reshape the organization's agenda in dialogue with a sympathetic membership. By the early 1980s, the movement had become thoroughly institutionalized within the CSAA, blending with feminism (also by then institutionalized) and shifting its frame from cultural nationalism to a concern for “the underrepresentation of certain historically disadvantaged groups”—including Canadian female academics (p. 184). However, contrary to analysts such as McCarthy and McPhail, who associate institutionalization with decreased effectiveness of protest, Cormier argues that Canadianization achieved its greatest success—a federally mandated Canadians first university recruitment policy—with institutionalization.

Throughout, the analysis is steeped in the resource mobilization perspective, emphasizing the action frames, mobilizing structures, and political opportunities that constituted and shaped Canadianization. The book is largely a historical case study, assembled on the basis of select archives and interviews, with theoretical concepts introduced as sensitizing, interpretative tools. This approach is consistent with Cormier's objective which, following McAdam, Tarrow, and Tilly, “focuses our attention less on establishing certain static universal laws of contentious action

and more on the dynamic mechanisms involved in pushing forward social change” (p. 192).

Overall, this study illustrates the strengths of resource mobilization theory (RMT). Cormier presents a concrete analysis of the successive conjunctures and phases of Canadianization, invoking a raft of middle-range concepts to illuminate specific aspects of mobilization. But this is not to say that there are no weaknesses. At points, the analysis becomes formulaic, with well worn RMT propositions introduced as major premises before support for them is marshaled by resort to the historical record (e.g., the analysis of components of successful collective action frames, in the first chapter). This didacticism may be helpful to students, and the book’s highly accessible style will make it useful in the classroom, but we are left to wonder how much retrospective interpretation is in play, as each instance seems to confirm its corresponding theoretical formulation.

The book also suffers somewhat from its narrow focus on one organization, when as Cormier acknowledges in his conclusion, Canadianization was a much broader phenomenon. A narrative that charts a linear course from the late sixties activism of Mathews and Steele to the transformation of the CSAA in the 1970s risks misrepresentation, as major collective agents that constituted the political field for Canadianization—the new left and student movement, the left-nationalist, activist wing of the New Democratic Party (“the Waffle”), the Committee for an Independent Canada, and among academics, major groups such as the Political Economy Network and the Society for Socialist Studies—are omitted or marginalized. Arguably, the Committee for an Independent Canada, a broad based citizen’s group, was the social movement organization that inherited the Canadianization mantle after 1971, not the CSAA, a comparatively small professional association. In this sense, research-design issues, and particularly the decision to highlight the role of the CSAA, seem to have skewed the empirical analysis.

On the other hand, it is fascinating to see social movement theory applied to an academic association, whose repertoire of action consists mainly in discursive practice. Yet surely it is the case that social activism is often embedded within institutions and formal organizations, blurring the conventional distinctions among interest groups, parties and movements, and extending the relevance of many theoretical formulations beyond the so-called social movement sector. But

on this issue we need to ask which formulations might be most instructive in a given context. In the case of Canadianization—a project of moral and intellectual reform—I cannot help wondering what more we might have learned had Cormier resorted to some of the more hermeneutic and normative formulations that are often grouped under the rubric of new social movement theory. Cormier has constructed a highly plausible account of why the movement mobilized as it did and achieved its main objectives. It will be left to others to probe the ways in which the cognitive praxis of Canadianization may have presented a symbolic challenge to continentalist elites, may have helped transform the public sphere, and may have become articulated with other popular democratic currents such as feminism, multiculturalism, and most recently, antiglobalization.

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Scott Frickel. *Chemical Consequences: Environmental Mutagens, Scientist Activism, and the Rise of Genetic Toxicology*. New Brunswick, New Jersey: Rutgers University Press, 2004. \$62.00 (cloth), \$22.95 (paper).

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*Chemical Consequences* is a fascinating look at the rise of genetic toxicology as an interdisciplinary scientist movement. Drawing on documentary and interview data, Frickel describes the politics of knowledge involved in genetic science, particularly how activist scientists transformed knowledge, practices, and institutions to reflect pressing concerns about the relationships between environmental chemicals and genetic mutation which may otherwise have been relegated to the margins of genetics research. We are all beneficiaries of this activism as it was critical in producing much of what we now know about the genetic consequences of synthetic chemicals—for example, links between environmental pollution and cancers and the potential hazards of mutagens in food and consumer products. This movement brought that knowledge out of the laboratory, onto government agendas, and into popular awareness.

Frickel’s interest is in “the institutional politics of knowledge [which] views [scientific] discipline building as a political process...” (p.5). He argues against explanations that claim the rise

of genetic toxicology was an imminent result of fragmented research agendas that fortuitously converged around 1970, precisely at the moment when environmentalism exploded and government interest “in a new form of environmental knowledge became most acute” (p. 11). This oversimplification fails to acknowledge the real political, organizational, economic, and cultural obstacles overcome by a small group of activist scientists who “elaborated a social critique of the disciplinary organization of science [to] create a new way of ordering environmental knowledge” (p.12) around the study of genetic toxicology. The interdisciplinary character of this new science made it all the more difficult to overcome these obstacles. Those promoting genetic toxicology faced not only the challenge of convincing colleagues in one disciplinary branch (radiation biology or cytology) with a single institutional anchor (university departments), but across several branches, in diverse institutional contexts (research institutes, centers, and the federal government), with varying political and economic pressures shaping their activities.

The book begins with the struggle for prominence in the field of genetic mutation research between radiation mutagenesis (the study of radiation’s effects on gene mutation) and chemical mutagenesis (the study of chemical effects on gene mutation). Frickel explains that as the first of the two branches of gene mutation research to be developed (1927), radiation mutagenesis powerfully staked a claim on the field’s research agenda. By the time interest in chemical mutation emerged in 1940, the prestige and institutional support for radiation research acted as a brake on chemical mutation research development. Many molecular biologists acknowledged the great theoretical promise that chemical mutagenesis held for understanding big questions in the field about, for instance, evolutionary processes of mutation, but “theory was not some abstract motor that drove the new field of chemical mutagenesis” (p. 29). It remained subordinated to research driven by radiation genetics. To change this, interdisciplinary concerns had to be rerouted toward chemical mutation research as a new and productive approach. This meant convincing scientists and others that chemical mutagens constituted a credible environmental problem with important social and political implications beyond basic science in the lab.

After setting up the context for this disciplinary struggle, Frickel devotes his attention to

explaining how, in the end, the study of chemical mutagenesis was pulled from the margins of genetics research to its center. His explanation is guided by social movement theory, specifically political opportunity and framing approaches. Using insights from these standpoints, he picks apart a complex history to attribute the rapid institutionalization of genetic toxicology between 1968 and 1976 to the advocacy work of a few individual scientists that ultimately coalesced into a scientist’s movement. Frickel’s faithfulness to a social movement framework is clear as he describes scientists’ efforts as “contentious politics” (p. 68). But, he explains, “it was a subtle form of contention, primarily implicit in scientist-activists’ conventional institution-building activities. The movement challenged the disciplinary organization of professional science, federal environmental policy, the basic/applied dichotomy distinguishing biology and public health, and the social responsibility of geneticists and toxicologists” (p. 68).

During the critical years of genetic toxicology’s ascendancy, there was a sharp increase in research productivity and publication on chemical mutagenesis from a core group of geneticists. These scientists also gave public lectures, wrote politically challenging commentaries, and organized professional societies and conferences aimed at educating the public on the risks of mutagenic substances. Frickel uses framing concepts to explain “how the activist scientists made the rhetorical case for genetic toxicology” (p. 85). Scientists framed chemical mutagens as an environmental health issue, which invested the research program with a measure of moral and ethical obligation. Framing also explains how the core group of activist researchers enlisted others from across many disciplines, which expanded the institutional points of access and opportunities leading to support for genetic toxicology’s concerns. Using a strategy that Frickel calls “frame translation,” advocates made strong claims about the application of mutation research to problems in a wide range of other fields. The claims resonated, myriad others joined the new research agenda, and the interdisciplinary field of genetic toxicology was institutionalized.

*Chemical Consequences* gives important insights into the politics of science. In doing so, it offers a very nice balance to historical science studies, which often attribute the development of scientific knowledge to a simple ripening of ideas whose time has come. Using familiar social movement concepts applied to an unfamiliar

case, Frickel explains the cultural and material struggles faced by scientists and how collective efforts were organized to overcome institutional barriers and constitute genetic toxicology as a new and important research area. The book's strength is in the explanation of scientific transformation. There are no path-breaking theoretical advances here for the social movements field, although the concept of "frame translation" is a useful addition to the study of framing. What Frickel's research demonstrates best is just how indispensable social movements concepts are for making sense of complex sociological cases. In this instance, it improves our understanding of how scientific change occurs.

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Quintan Wiktorowicz. *Islamic Activism: A Social Movement Theory Approach*. Bloomington: Indiana University Press, 2004. \$24.95 (paperback).

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The study of Islamic politics has long been dominated by the Islamicist perspective, which relied heavily on the analysis of Islamic text in explaining Muslim political behavior under varying historical and social conditions. This text-based, reductive, and often formalistic approach had little appeal to the small number of social scientists interested in explaining cultural change in Islamic countries in the sixties and seventies. During the heyday of modernization theory that tended to draw its credibility from a period of fairly rapid economic development guided by the secular state in these countries, Islam was treated as a part of the traditional culture whose social influence was expected to wither away as industrialization, social differentiation, and value generalization created a new cultural pattern. The upsurge of Islamic fundamentalism from the late sixties on proved this prophecy to be premature. The remarkable persistence of this phenomenon generated considerable interest in the social-scientific communities and, as a result, a wide variety of theories that focused on crisis, cultural duality, state, collective action, Muslim exceptionalism, institutional transformation, and the fragmentation of cultural authorities came into the lime light.

This new edited volume is a welcome addi-

tion to this broad social-scientific movement and contributes to the accumulation of knowledge about one of the most perplexing problems of contemporary Islamic countries, which after the horrific event of September 11th, has turned into a global issue. With the aid of the collective action perspective in the area of social movements, the contributors analyze the problem of violence, networks, and culture in such diverse Islamic countries as Algeria, Egypt, Bahrain, Iran, Palestine, Turkey, Yemen, and Saudi Arabia. The first four chapters on violence (Hafez on GIA in Algeria, Hafez and Wiktorowicz on Gama'a Islamiyya in Egypt, Lawson on forms of contention in Bahrain, and Robinson on the social aspects of Hamas's activities in Palestine) focus on state repression and exclusionary policies, trends in local economy, and the interaction between state actions and Islamic groups. The following four chapters discuss the role of networks and alliances in the persistence of Islamic activism under state repression. Singerman focuses on informal networks in various context, Clark on the role of networks and informal institutions in mobilizing women Muslim activists in Yemen, Smith problematizes the bazaar-Mosque alliance in the Iranian revolution by arguing that the bazaars participated in the revolution for varied reasons and through different organizations, and Schwedler focuses on the ramifications of shifting coalition for Islamic activism in Yemen. The last three chapters all address the role of culture and framing in Islamic mobilization. Wickham makes reference to moral-obligation frames to explain the success of Egyptian Islamic groups in recruiting prosperous university graduates in the 1980s and 1990s, Okruhlik to the indispensability of Islam as a legitimizing political language for oppositional politics under the conditions of authoritarianism in Saudi Arabia, and Yavuz to the differential impacts of economic liberalization on state- and society-centered Islamic organizations and groups.

This collection as a whole contains considerable empirical materials and useful information on political Islam. It is useful for senior level and graduate courses on social movements and Islamic activism. The volume, however, falls short of transcending the current state of theory and research in the area of the sociology of the Islamic movement, or social movement research in general. There are several empirical and theoretical problems. First, about violence in Algeria, many French and Algerian researchers have uncovered that many of the GIA's violent activities

have in fact been conducted and perpetrated by the military regime. Second, comparative studies of violence have pointed to a curvilinear relationship between regime repressiveness and political conflict; a high level of political conflict is more likely to occur under a medium rather than a weakly or highly repressive regime. In fact, in the cases of such countries as Algeria, Egypt, and prerevolutionary Iran, the Islamic movements gained considerable momentum after the state decided to partially relax its repressive policies. Thus, the reference to repression as a causal factor may require a historical analysis of the relationship between variations in regime repressiveness and Islamic activism. Third, while networks are the sine qua non of social movements, the authors failed to document the specific networks underpinning the rise of Islamic activism. To simply mention that networks were important and that such networks were informal and connected to religious or traditional institutions is empirically inadequate. Fourth, although all the chapters are quite informative and expertly written, none achieves scientific explanation. It is difficult to verify or falsify arguments advanced by some of the authors.

Fifth, the introduction and conclusion, while quite effective in a didactic sense (save for an occasional slippage, for example, of placing in the same categories such diverse individuals as Romantic William Jones and Eurocentric rationalist Ernest Renan), fail to critically evaluate the collective action perspective or make an attempt at theoretical integration. We should note that this perspective was first developed to emphasize rationality, collectively shared values, and organized efforts to mobilize resources in order to realize the movement's objectives within the historical context of the struggle of African-Americans, other minorities, women, and peace activists in the U.S. for political inclusion, economic benefits, and cultural empowerment—the kind of objectives that were consistent with the values and promises of a democratizing society. Within this context, the humanitarian harbingers of social movement theories found the notion of marginality and disoriented individuals proposed in the mass society perspective to be biased and inadequate. There are certainly strong grounds to propose that the predominantly middle-class activists who joined the diverse Islamic movement ranging from Egyptian Muslim Brothers in thirties through the forties and Algerian FIS in the nineties were also demanding political inclusion and empowerment. Yet, one cannot but feel un-

easy using the same model to explain GIA or Hamas, which gained notoriety through their mass killing of innocent lives.

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Michael J. Pfeifer. *Rough Justice: Lynching and American Society, 1874-1947*. Urbana and Chicago: University of Illinois Press, 2004. \$35.00 (cloth).

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Thanks to the recent work of many historians and sociologists, America's shameful record of mob violence has now been thoroughly documented. The number of victims is sobering. The brutality of the mobs is numbing. The contradiction with our national fantasy of a history of "freedom and justice for all" is enlightening. Yet, social scientists still struggle to explain why mobs of average Americans felt justified in taking the law into their own hands to summarily execute fellow citizens without the benefit of a trial or conviction. In *Rough Justice*, historian Michael J. Pfeifer offers his explanation. Put simply, Pfeifer describes a competition between rough justice (i.e., lynching) and due process (i.e., legal execution). When and where the state failed to punish serious criminals swiftly and severely, citizens resorted to lynching in order to achieve retribution and to broadcast a message of deterrence to other potential offenders. Pfeifer's book is an important contribution to the growing literature on lynching in America. It is the first serious scholarship on the topic that encompasses such a wide geographic scope, and it offers the most articulate and well-reasoned argument for a causal link between the rise of legal executions and the demise of lynching.

Pfeifer's case is built upon the careful analysis of mounds of primary evidence, including coroners' inquests, newspaper articles, and other archival information related to lynchings, prevented lynchings, and legal executions that occurred in California, Iowa, Louisiana, New York, Washington, Wisconsin, and Wyoming. These states were chosen to represent regional variation in the frequency and functions of mob violence, but also to lend universal support to Pfeifer's thesis. According to Pfeifer, local citizens had a strong, almost visceral, desire to see that serious offenders were punished for their

crimes. They had little tolerance for legal maneuvering or leniency as the criminal justice system processed criminal cases. Changes of venue, appeals, reduced sentences, and other obstacles on the defendants' road to the gallows frustrated the populace and convinced them that the state was not a reliable partner in the fight against serious crime (or in their effort to intimidate and subjugate racial and ethnic minorities). Fueled by that frustration with due process and a resulting sense of social obligation, mobs of ordinary citizens resorted to rough justice. They lynched thousands of accused criminals, guilty and innocent alike, often in a way that delivered a clear message of warning to the legal authorities and to the general public. The demise of rough justice occurred only after the state demonstrated that it was serious in fulfilling its responsibility to punish serious criminals, and to do it in a way that satisfied the potential proponents of mob violence. To accomplish this, and to satisfy a cross-section of public sentiment, the use of the states' gallows needed to become dependable, routinized, "scientific," and private—much as it had been in the Northeast since the mid-nineteenth century. But, and this is very important to Pfeifer's argument, the imposition of the death penalty needed to maintain at least some of the racialized nature, and terroristic function, of mob violence. Even now, according to Pfeifer, "the arbitrary, racialized, and performative characteristics of today's death penalty carry on what was most important to the advocates of rough justice: that the guilt, innocence, or humanity of an executed person matter less than the collective vengeance satisfied by the ritualized taking of their life (p. 153)."

Now, what of the evidence that Pfeifer marshals in support of his main thesis? This is where *Rough Justice* will convince some, but possibly not all, readers. The supporting hard evidence consists primarily of three types: (1) examples of the temporal correspondence between increasing executions and declining mob violence within specific locales, (2) examples of area variation in the reliance on legal executions and lynching, and (3) testimonials by judges, editors, and others of an inverse relationship between the use of the death penalty and the likelihood of mob violence.

I venture a guess that one's evaluation of these will depend upon the rules of evidence to which he/she subscribes. In general, historians and non-quantitative sociologists will be more persuaded than will quantitative, hypothesis-testing, social scientists. Many of the latter will be somewhat troubled by the lack of a "p-value" that describes the probability that Pfeifer is wrong in drawing his conclusion.

I, myself, am a "quantitative, hypothesis-testing" sociologist. So, what is my take? Pfeifer makes a very powerful case. He is clearly a good and logical thinker. He has amassed an impressive amount of evidence to support his argument. Even if his style of research does not yield a "p-value," he has given us ample reason to consider seriously his explanation for why lynch mobs sprang into action, and why they disappeared from the nation's landscape. It is inconvenient for Pfeifer's argument that previous statistical evidence has failed to yield support for an inverse relationship between lynchings and legal executions, either in their covariation over time or across counties, at least within the South. In commenting on this previous evidence, he correctly notes that the dynamics of the relationship are complex, and that they play out at the local level. Still, a more direct attempt to reconcile this prior evidence with the evidence in *Rough Justice* would have been useful. It is also not entirely clear to me how the substitution of due process and legal executions for lynchings in cases of homicide can explain the willingness of southerners to abandon mob violence against those who committed only minor transgressions such as being disrespectful to a white man or insulting a white woman.

Social scientists in all disciplines will learn a great deal from *Rough Justice*. It presents the interesting and important results of painstaking scholarship by a very talented historian. Therefore, it has definitely earned a place among the very best work on the topic of lynching in America. That I have lingering questions about the exact nature of the link between legal executions and mob violence only means that there is more work to be done. We should be grateful that fine scholars like Michael Pfeifer are engaged in that work.