POLITICAL THEORY

Deliberation Day. By Bruce Ackerman and James S. Fishkin. New Haven: Yale University Press, 2004. 288p. $30.00.

Deliberative Democracy in America: A Proposal for a Popular Branch of Government. By Ethan J. Leib. University Park: Pennsylvania State University Press, 2004. 156p. $27.50.

Democratic Autonomy: Public Reasoning about the Ends of Policy. By Henry S. Richardson. Oxford: Oxford University Press, 2002. 328p. $45.00 cloth, $19.95 paper.

— Emily Hauptmann, *Western Michigan University*

Compelling theories of politics invite us to see the world differently. But once we see political life in different terms, what will we be moved to do? Redesign our political institutions? Or revise our reasons for supporting those that currently exist? As the authors of the three books reviewed here illustrate, those who have taken up deliberative theories of democracy are moved to engage in profoundly different kinds of projects, marked either by redesign or revision. Bruce Ackerman, James Fishkin, and Ethan Leib believe their commitments to theories of deliberative democracy require them to focus on drafting extensive plans for institutional redesign. By contrast, Henry Richardson, while endorsing institutional reforms, ranging from changing electoral law to opening administrative rule making to greater citizen participation (pp. 200–202, 219–22), devotes the majority of his book to showing how the ideals of his theory of deliberative democracy can make better and more complete sense of political life as it is. The deep contrast between how these authors understand what one ought to do with a commitment to deliberative democracy prompts us to consider whether they are simply committed to different things or are striking out on different paths from substantially similar starting points instead.

Ackerman and Fishkin argue for opening elections to citizen deliberation on a grand scale. The proposal at the heart of *Deliberation Day,* a two-day national holiday several weeks before election day, differs in several important respects from the deliberative polls Fishkin advocated in *Deliberation and Democracy* (1991) and *The Voice of the People* (1997). In his earlier work, Fishkin argued for selecting a random, representative group of people from across the country to meet together for at least several days before the beginning of the presidential primary season—in effect, a national primary in microcosm. *Deliberation Day* substantially revises these earlier proposals. Gone is the case for constituting the public in microcosm, as is the ambition to begin the primary season on a national, deliberative note. Instead, Ackerman and Fishkin hope to open up deliberations about national elections to every citizen at a time when many are likely to be paying attention—just a few weeks before election day. Meeting at local public places with around 500 other citizens from the immediate area, each participant would receive a $150 honorarium for spending the day listening to the candidates debate, taking part in small group discussions, and posing questions to party representatives in plenary sessions.

A substantial portion of *Deliberation Day* is devoted to laying out the details of how the event would work and to allaying fears about its costs or vulnerability to the wiles of career politicians and interest groups (Part I, Chapters 1–6). In other words, this is largely a work of institutional design. To be sure, Ackerman and Fishkin engage several broader, more fundamental issues, such as the place of mass participation in a representative government (Chapter 7) and the political consequences of deep inequality (Chapter 9). But as the placement of these chapters in the book suggests, what the authors have to say on these matters reads more like an extension of their argument for Deliberation Day than an explication of what led them to devise it in the first place.

So what moves Ackerman and Fishkin to embark on this project of institutional design? In a book so clearly and accessibly written, this turns out to be a surprisingly difficult question to answer. Rhetorically, they present their picture of what ails the American polity as an obvious, uncontroversial one (pp. 5–13). But if we "grant" what Ackerman and Fishkin say we must—that "a majority of voters are woefully ignorant and readily manipulated" (p. 13)—a great deal of the groundwork for embracing Deliberation Day as the solution to these ills has already been laid. Add to this their claim that "[o]rdinary men and women *can* function successfully as citizens" given the right "institutional contexts" (p. 5), and the fundamental case for Deliberation Day is nearly made.

Ackerman and Fishkin's proposal, however uncontroversial its presentation, will fail to convince any deliberative democrat who believes "political poverty" is not principally an informational deficit (e.g., James Bohman, *Public Deliberation,* 1996, pp. 110–11). If, instead, what makes people politically poor are low wages, little free time, and rare opportunities to connect with others in similar circumstances, then the imperative that participants in Deliberation Day look for middle-of-the-road challenges to pose to candidates on which most people can agree (p. 28) will tend to muffle, rather than give voice to, what the politically poor need. This tendency is especially worrisome since Ackerman and Fishkin concede that if their Deliberation Day proposal were adopted, its operation could "enhance the legitimacy of the remaining *inequalities* haunting contemporary life" (p. 193). This concession highlights the difference between those problems that the authors believe the deliberative institutions they have designed are best suited to solve and those that they are likely to leave in place or even make worse.

Like Ackerman and Fishkin, Leib puts institutional design at the center of *Deliberative Democracy in America.*

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Chiding other deliberative democrats for being "notoriously short on proposals for practical institutional reform," Leib announces his intention to take what he deems the road less taken, calling his first chapter, "Getting Right Down to the Business of Institutional Design" (p. 9). His proposal is an ambitious one: He argues that only the creation of a new branch of government can make popular sovereignty a reality. The "popular branch" would be staffed principally by citizens selected at random from the jury pool; its charge would be to replace votes on referenda and initiatives with deliberative discussions leading to binding decisions on policy matters. Though binding, the popular branch's decisions could be overridden by other branches; other branches could also forward matters to the popular branch for consideration. Those selected to participate in the popular branch would be compensated for their time but would be required to serve (pp. 12–27).

Leib devotes much of his brief book to filling in the details of the proposal introduced in his first chapter, often by contrasting his aims and methods with those proposed by others. It is to Fishkin more than anyone else that Leib pays such attention (pp. 5, 23–27, 36–38, 93–95), albeit to the Fishkin of Deliberative Polling (1991, 1997), rather than the Fishkin of Deliberation Day (a proposal to which Leib makes no reference). Of course, Leib and Fishkin are of one mind on the importance of institutional design, but Leib wants popular deliberation to have a more settled place and more power in the political system than Fishkin does. For readers looking for a thorough exposition of differences between various proposals for institutional reform made by deliberative democrats and some of their predecessors, Leib's book, particularly in Chapters 2, 5, and 6, is a fine resource. This very quality, however, might make it forbidding to a reader relatively unacquainted with deliberative democratic theory; although he writes engagingly, Leib too often allows relatively small disagreements with other writers to structure his chapters (as in Chapter 6, in which five successive sections, from pages 93 to 103, are each devoted to explicating his relatively minor disagreements with five different theorists). What is more, he is so eager to introduce his proposal that he skimps a bit on making a case for its necessity. To say, as Leib does, that our political system suffers from "legitimacy deficits" (pp. 4, 34) is not terribly controversial. But how one ought to judge the scale or depth of this ill and whether his ambitious proposal is especially well suited to cure it are central questions he leaves unaddressed.

While Ackerman, Fishkin, and Leib think too few deliberative democrats have directed their energies toward full-fledged proposals for institutional reform, Richardson, in *Democratic Autonomy,* locates the shortcomings of deliberative democratic theory in its advocates' failure to explain how deliberation yields the goods ascribed to it (p. 74). If public deliberation can indeed be "open-ended, preference- and end-changing," Richardson argues, we must show how this can be so. This would mean giving an account of deliberation as a noninstrumental form of reasoning—reasoning that "can extend to the ends of policy and not just concern the selection of means" (p. 74). Constructing such an account does not entail a project of institutional design; rather, it requires only that one make explicit and defend "a mode of reasoning that our best public servants actually use" (p. 76). He offers both an extended critique of understanding public deliberation instrumentally (including in cost–benefit terms, pp. 119–29) and presents an exposition of noninstrumental public reasoning in the six chapters that make up Part II. On its own, this part of the book makes a major contribution to deliberative democratic theory.

Spelling out what it is about public deliberation that enables it to yield the goods we prize, however, is only one of several large tasks Richardson sets for himself. He also sets out to examine whether and how the administrative power that contemporary representative governments grant bureaucracies can be squared with the ideal of popular sovereignty. He makes a compelling case for the importance of this issue, one that rests in part on showing how infrequently contemporary democratic theorists have addressed it (pp. 8–16). But especially if one takes this criticism to heart, one is likely to feel a bit disappointed that the body of the book includes relatively few discussions of the workings of administrative power and how they might be made more compatible with popular self-rule. Chapter 16, "Democratic Rulemaking,"offers the most sustained discussion of this issue; here, Richardson argues both for making administrative rule making more open to popular participation (pp. 219–22) and for the ways in which administrative expertise can enhance rather than frustrate popular rule (pp. 224–30). But this chapter, even taken together with the discussions of bureaucratic domination and the importance of allowing administrative deliberation to redefine preferences and ends (pp. 28–36, 107–12), does not fully satisfy one's heightened expectations that reconciling bureaucratic power and popular sovereignty will figure centrally in the book.

To return to the question I posed above: Why, then, do these authors embark on such different projects, given that all say they are committed to the ideals of deliberative democracy? Although I cannot fully show this here, I do not think that Ackerman's, Fishkin's, and Leib's reasons for focusing on institutional redesign, as opposed to Richardson's for reimagining the ideals that govern existing institutions, can be traced back to some fundamental disagreement about what the ideals of deliberative democracy are. Rather, the disagreement centers on whether each thinks those ideals are themselves a fruitful topic for inquiry. For Ackerman, Fishkin, and Leib, little about the ideals of deliberative democracy needs to be spelled out or justified—not only are these our ideals already, but many of us also have a sense of the parts of our political system to which their more stringent application would be most salutary. The task of

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the theorist, then, is to put a complete institutional skin on already solid, intuited bones. Richardson, by contrast, focuses on unpacking the concept of public deliberation and the ideals it includes to show that how we understand them will affect the expectations to which we hold institutions, public officials, and citizens. On this approach, the task of the deliberative democrat is to refashion older, often conflicting ideals while working in a new theoretical medium—a task logically prior to any project of institutional redesign. The contrast between these approaches illustrates how big a tent deliberative democratic theory has become.

For the Sake of Argument: Practical Reasoning, Character, and the Ethics of Belief. By Eugene Garver. Chicago: The University of Chicago Press, 2004. 264p. $55.00 cloth, $22.50 paper.

— Thomas W. Smith, *Villanova University*

Does reason advance democratic values, or is it a manifestation of the will to power, used by elites to justify coercion? Eugene Garver seeks a middle way through this question by claiming that certain uses of reason oppress, but others do not. Practical reason can build democratic community, but only if it is carefully delineated from manipulative sophistry and an excessively theoretical reason that seeks logical rigor to the detriment of decent practices. Garver makes rhetoric a central case of practical reason to argue his point. This is understandable, given his previous work on Aristotelian rhetoric and the history of prudence.

The author begins by taking issue with the assumption that the paradigm of rationality is disinterested bargaining between strangers. Using the example of the South African Truth Commission, he argues that embedding practical reason in political forms of friendship allows it to be sensitive to context and to build an ethos for an ethical community. How do we know we are not being manipulated into agreement for the sake of "ethical community"? Garver argues for a distinction between rational and sophistic uses of rhetoric. In contrast to the sophistry that manipulates, rational rhetoric constitutes an ethical community by offering arguments. He then explores how *Brown v. Board of Education* is a paradigm of practical reasoning. For Garver, the Warren Court faced a kind of impasse: It could find neither historical nor constitutional justifications for overturning *Plessy.* So it enunciated an ethical principle instead. *Brown* was an instance of epideictic rhetoric, for it urged citizens to recognize equality and antidiscrimination as fundamental American values. It thus evoked a kind of political friendship. Further, its use of practical reason was "ampliative." That is, the Court's argument generated an ethos, but the implications of that ethos exceeded its argument. This ethos led not merely to the overturning of antimiscegenation laws and other forms of discrimination. Its ethical force also led Americans to see equality in terms of ending discrimination, profoundly transforming constitutional doctrine on sexuality, the disabled, and religious freedom (85).

Garver then argues that practical reasoning can be the subject of ethical judgment; we are not being illogical when we engage in ethical reasoning. Given his account of the proper uses of rhetoric, he must also be able to show how an authority in a democracy can legitimately employ arguments to construct an ethical ethos for the community. He distinguishes authority and force by revisiting *Brown.* In this case, the Court's authority derives from the force of its argument, not from precedent or history. In the kind of community to which *Brown* aspires, trust is central. Trust can be rational because it flows from the kind of ethical ethos that practical reason constructs. Garver next argues that his conception of practical reason can illuminate pluralism. Modern democratic communities are organized around "plural ultimate values" (p. 11). In turn, there are plural legitimate modes of interpreting the Constitution, and these pluralisms are responsible ways of thinking through ethical arguments.

In the conclusion, Garver glosses a host of problems: the relationship of philosophy to practical reason, Richard Rorty's critique of philosophy, the unity of practical reason, whether modern science is a model for democracy, human nature, the death of metaphysics, and Dewey's pragmatism. His most important point is that metaphysical arguments about human nature cannot provide a model for practical reasoning because human essence is not fixed. Human nature is open-ended because it is historically conditioned. This liberates practical reason, for it can be conceived as autonomous from philosophy and human nature. This allows it to deliberate about our ends and the plural sources of values that inform those ends. Needless to say, Garver's argument is dense, complex, and wide-ranging. My sketch does not do it justice.

I have two questions. First, what are the implications of Garver's account of rhetoric and practical reason? Aristotle argues that these are distinct, though related, phenomena: Practical reason is a virtue (*arête*) and rhetoric is a technique (*techne*). By contrast, Garver holds that rhetoric and practical reasoning are abilities or powers (pp. 163, 198) that can be used to construct a community's ethos. Perhaps he abandons Aristotle's account because he wants to make rhetoric a paradigmatic instance of practical reason. Yet this may make it more difficult to fulfill a central aim of the book—to argue that there is a difference between rational and sophistic rhetoric. The relationship among ability, virtue, and technique is far too large an issue to pursue here. But we could get a sense of the stakes involved by asking what would happen if we rejected the notion that, say, honesty is a virtue. What if we thought honesty were a kind of power that could be used in a plurality of legitimate ways to construct a community's ethos or an individual's open-ended identity? Would we be more or

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less likely to treat assertions of honesty as expressions of interest? In the end, Garver thinks of his account as a modification of pragmatism—as aiming at agreement rather than truth, and as pointing to plural sources of value. So he might not be discomfited to speak of a plurality of honesties or practical reasonings. But his claim that rhetoric and practical reasoning are powers or abilities may mean that his account lies closer to the spirit of some of the ancient sophists than he wants to admit. After all, many of them rejected the distinction between virtue and power.

Second, in a book defending reason from the accusation that it justifies power, it is important to ask whether Garver's account avoids that charge. In the conclusion of *For the Sake of Argument,* he says that metaphysical conceptions of human nature and freedom must be abandoned to allow an autonomous practical reason room to deliberate about our ends. This is ethical progress, because "forcing us to abandon the language of nature and choice and adopt the language of due process and equal protection forces us into an argumentative community" (p. 171). However, Garver never argues that human nature in an untenable concept; he simply asserts it (pp. 190–91). Perhaps he assumes that his academic audience will take for granted the death of metaphysics and of human nature. Yet these are claims, not arguments. How can forcing someone to abandon a deeply held conviction without giving any reasons either construct an argumentative community or be considered ethical progress? Further, the assertion that there is no human nature will determine which arguments are considered rational and which are not. Those basing their political policies on conceptions of human dignity, on philosophical conceptions of freedom, on religious affiliations, or on traditions of various kinds may find it hard to get a hearing in Garver's ethical community. It might be understandable if those whose deepest convictions are dismissed as irrational without philosophical argument interpret the account of practical reason that leads to this exclusion as an expression of power.

Imagining the American Polity: Political Science and the Discourse of Democracy. By John G. Gunnell. University Park: Pennsylvania State University Press, 2004. 289p. $40.00.

— David Schlosberg, *Northern Arizona University*

John Gunnell laments the ignorance of our own past in political science and is determined to get us to understand the richness of earlier generations of political scientists and theorists who focused on the American incarnation of democracy. The key task of this book is to examine the discipline's understanding of democracy, with a focus on the conversations surrounding conceptions of the state, liberalism, and, especially, pluralism. On this, Gunnell offers a comprehensive and enlightening history of some of the central defining discourses of political science and political theory, interwoven with discussions of how this history relates to the development of political science as a discipline.

The author starts with the conception of "the people" in the founding of the United States, in particular, the conceptions found in interpretations of *The Federalist.* In examining this period, he is less concerned with whether or not the United States was democratic than with whether it was *perceived* as such by commentators. This is tied to the development of the mission of American political science at the time, which, he argues, was less critical and analytical and more educational: "Its self-consciously defined vocation was to provide an image of the polity and to justify that image" (p. 55). This focus was criticized early on from within the discipline, and Gunnell obviously wants us to note the similarity of early and contemporary complaints. Walter Lippman bluntly observed in 1913 that "works on politics by American university professors are useless" (p. 113) as they had lost contact with the reality of political practice. Graham Wallace called for a "change in the conditions of political science" that would produce some real "political invention" (p. 114).

Gunnell, however, passes up the opportunity to focus on these long-lived critiques of the distance between the profession and its subject. Instead, he offers a thorough overview of the evolution of pluralist theory in the teens and twenties, with a focus on original material rarely seen. He explores a different tension in the discipline, between the growing interest in pluralism—from studies of groups to organic theories of the state—and the mainstream of political science in its emergence. As pluralist thought moved political theory away from "the state" and began to encompass sociology, psychology, and economics, it seemed to undermine the distinctive disciplines in the academy. He claims that the identity of political science was threatened; this helps explain why pluralism was, and still is, attacked by those uncomfortable with the move away from a focus on the state and on to the myriad groups within society.

Gunnell explores the move from a focus on pluralism to the prominence of liberalism in the 1930s and 1940s. This is not so much a discussion of the decline of pluralism as a discourse within political science as methodical coverage of the rise and evolution of the liberal discourse in this period. He describes where the discipline's central concern with liberal theory and institutions began before World War II, and offers a good discussion of the origins of the struggle around the conceptions of liberalism, totalitarianism, and conservatism in the years before and after the war. There is a very interesting, but unanswered, question here: Why did most pluralist theories disappear "from the consciousness, or at least the literature, of mid-century political science?" (p. 181). Pluralism as a discourse, of course, reappears in a new form after the war, and Gunnell notes the odd lack of acknowledgment of the earlier pluralists in the postwar generations.

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The strength of this book is in the comprehensive history of the pluralist discourse in the discipline—up until the recent past. Unfortunately, Gunnell eschews a substantive discussion of the contemporary pluralist literature, covering the last 20 years in just seven pages of the final chapter—listing no fewer than 25 authors in a single footnote (n. 69, pp. 246–47). There are crucial differences among these authors, though Gunnell seems to assume a singularity of vision that simply does not exist. For example, he argues that the new pluralism devolves authority and democratic identity to small groups in response to the classic democratic problem of equality in decision making (p. 251). This is simply not true, as many contemporary pluralists named (a few of whom might object to the title) focus on relations between groups and the state (John Dryzek, Will Kymlicka, and Iris M. Young, to name three from Gunnell's list). The same attention to the contemporary literature as Gunnell showed the prewar generation would have illuminated some imaginative new responses to the central issues raised in earlier generations—conceptions of the state, the people, the government, group identity, and the relationships among them.

Gunnell offers some explanations for the resurrection of pluralist discourse—including John Rawls's interest, the collapse of the Cold War, and the rise of postmodern, poststructuralist, and multicultural theories. But his final explanation for this recurrence is the most intriguing: "the grip of the discursive heritage of political science" (p. 249). This is a fascinating claim and one of the key arguments in the book—that pluralism is how political science, over the entire term of its efforts, has imagined the American polity. Even though the discipline is oblivious to the literature and figures of the past, Gunnell argues, we are trapped in a discourse of our own making.

Imagining the American Polity concludes with a plea for the discipline to imagine "a fundamentally different way to think about the democratic concept" (p. 252). But the author does not suggest any strategies for extending the debate within the discipline or, more importantly, exploring other democratic discourses in civil society or bringing the internal debate to bear more on political practice. He defines his task as a third-order examination of second-order reflections and articulations on first-order democratic practice. Again, this is admirable intellectual and disciplinary history, but as it remains inwardly focused, it gives us no guidance for crossing or breaking down that boundary between first-order politics and second-order reflection. Political science as a discipline may be rich in history and discourse on conceptions of the state and pluralism, but a current critique of the discipline, from both within and without, stems from its lack of engagement with political movements and processes. Political science is already too internally focused, and while Gunnell sheds much light on the discourses that we have constructed, we will need more than just that light to break out of our shell. We will need a bit more imagination.

Critical Resistance: From Poststructuralism to Post-Critique. By David Couzens Hoy. Cambridge, MA: MIT Press, 2004. 288p. $35.00.

— Lasse Thomassen, *University of Essex*

This book examines the value of different so-called poststructuralist philosophical approaches to normative justification and political resistance. The label "poststructuralist" is a contended one, and David Couzens Hoy provides a useful discussion of it. He includes discussions of Friedrich Nietzsche as the most important precursor to poststructuralism, of the poststructuralists Michel Foucault, Jacques Derrida, and Ernesto Laclau, and of Pierre Bourdieu, Emmanuel Levinas, and Slavoj Žižek, who, according to Hoy, are not poststructuralists. As such, the book discusses a variety of poststructuralist and related approaches and provides a good introduction to the potential problems with these approaches with regard to normative justification of resistance to domination.

Although divided on a number of issues, the poststructuralists are united around certain philosophical concerns. These can best be summarized in terms of Nietzsche's rejection of philosophy as analytical and aiming at truth. Nietzsche and the poststructuralists following in his footsteps argue that meaning comes before truth and that no conceptuality is free from a certain amount of rhetoric. As a consequence, one has to examine how meaning is constituted through relations of power, through its embodiment in a concrete context or body, and so on. For poststructuralism, there is something inherently opaque about meaning, and as a result, truth is always contestable, as are conceptual distinctions.

Hoy organizes his discussion around the theme of resistance. The book is not an attempt to draw out the legislative or institutional consequences of poststructuralism in any detail. Instead, he wants "to see whether the various theorists can explain *critical* resistance, and whether their accounts point toward the possibility of resistance that is not merely reactive" (p. 6). When Hoy speaks about critical resistance, he is referring to emancipatory resistance to domination: "Critique is what makes it possible to distinguish emancipatory resistance from resistance that has been co-opted by the oppressive forces" (p. 2). His definition of critical resistance—and in particular his insistence on the possibility of distinguishing critical, emancipatory resistance from resistance in appearance only—is key to understanding the questions that he is addressing to the poststructuralist authors.

Hoy's treatment is informed by a worry, shared by other commentators, that poststructuralism is potentially relativist. The worry is that poststructuralism cannot answer questions such as: Why resist? Why resist this particular instance

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of domination? The flipside of this is poststructuralism's apparent inability to clearly distinguish domination from emancipation. As a corrective, Hoy proposes what he calls "post-critique," by which he means a position that is antifoundationalist and self-reflexive, and can provide theoretical support for resistance to domination. Post-critique should be understood both as a historical description of the development of poststructuralism and as a normative program. In the first sense, postcritique comes after poststructuralism, and Hoy describes it as the way in which poststructuralism is, in fact, developing when it is trying to provide support for critical resistance. In the second sense, postcritique is an alternative to poststructuralism, that is, as what poststructuralism ought to develop into if it is to be ethically and politically relevant. Thus, each chapter ends with a section on post-critical attempts to overcome the potential problems that Hoy identifies in the poststructuralist approaches under consideration.

The author has written a very clear, lucid, and well-structured introduction to the problem of resistance and normative justification in poststructuralist philosophical and political thought. This is the real value of the book, and it will no doubt prove useful for teaching the politics of poststructuralism to advanced undergraduate and graduate students. Nonetheless, the book also has its limitations.

The first limitation is not necessarily a problem, but something one must nonetheless be aware of. Hoy tries to be fair to the theorists he is dealing with and criticizing, often giving them the benefit of the doubt and defending them against their critics. As such, he stays true to the largely hermeneuticist position he is coming from. Yet there are two ways in which his hermeneuticist standpoint shines through in his readings of the poststructuralists. First, he aims to establish the unity of their individual works. This is somewhat ironic in the context of poststructuralist thinkers, as these theorists precisely argue that there is no text, work, or author that is perfectly homogeneous. A *poststructuralist* reading, in contrast to Hoy's *hermeneuticist* reading, would aim to show the heterogeneity of a text, a work, or an author. Second, one must bear in mind that the standards by which Hoy judges the poststructuralist thinkers—their ability to give answers and to provide more or less clear distinctions—are the standards of a non-poststructuralist point of view, something that he also recognizes. They are certainly valid questions, and any philosophical and political tradition must engage with other traditions. Yet one should not lose sight of the questions raised by poststructuralist thinkers themselves that often put into question the very possibility of giving (conceptual, final, etc.) answers, as well as the possibility of establishing conceptual distinctions. While Hoy does gesture toward the problems involved in approaching poststructuralism in this way, *Critical Resistance* is sometimes limited by its more traditionally (hermeneuticist) philosophical outlook.

The best chapters are those on Nietzsche and Foucault, while those on Derrida and Laclau suffer from the fact that Hoy has not considered the most relevant literature in relation to their works. In both cases, and given the focus on resistance, Hoy could have chosen more relevant themes and literature. In Derrida's case, he considers the philosophical and political questions surrounding the concept of death. While his discussion of this in relation to resistance (and in relation to Derrida's critique of Martin Heidegger and Levinas) is interesting, there are more relevant themes in Derrida's work, including his relationship to Marx and to cosmopolitanism. In Laclau's case, Hoy focuses on the critique of Laclau by Mas'ud Zavarzadeh and Donald Morton; here it would have been more relevant to look at the critiques put forward by Norman Geras and Simon Critchley. In both the case of Derrida and of Laclau, Hoy could, thus, have gotten more out of their works in relation to resistance.

Bound by Recognition. By Patchen Markell. Princeton: Princeton University Press, 2003. 320p. $59.50 cloth, $19.95 paper.

— Alan Patten, *McGill University*

Mainstream views of justice have typically concerned themselves with the distribution of goods such as money, power, opportunity, and liberty. In recent years, however, the traditionally neglected good of recognition has become a major focus of attention in contemporary politics and political theory. Stirred by a growing awareness of the pluralistic character of modern societies, many people now believe that recognition is something that is owed as a matter of justice. Just as poverty and a denial of liberty can have catastrophic implications for a person's well-being and self-development, misrecognition and nonrecognition can demean and insult an individual, leaving him or her with a crippling feeling of inferiority. In response to perceived failures of recognition, identity-related groups have called for significant changes in public policies and institutions: Political debates about everything from the college curriculum to laws regulating marriage, to language rights and race-conscious districting, to institutions of self-government for indigenous peoples and national minorities have been framed as "struggles for recognition."

For many political theorists, this turn to recognition has been something of a disaster. Critics charge that the new politics of recognition distracts the public and theorists alike from more urgent questions of economic justice. Worse still, it is argued, some of the policies that are demanded in the name of recognition seem offensive from a liberal standpoint. They extend benefits and privileges to groups with illiberal outlooks, or they create special distinctions between citizens and thus go against an ideal of equal, and hence undifferentiated, citizenship that has been historically central to the liberal vision.

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In addition, the politics of recognition is accused of reinstating a new form of "essentialism" that is ultimately just as insensitive to difference as the old universal politics. To recognize some particular identity is apparently to take a view of what that identity's essential features are and to attach public significance to those features. But this process of essentializing an identity risks imprisoning bearers of the identity into a "script" (as Anthony Appiah has called it) that heavy-handedly defines the "proper" way to be a bearer of that identity, and subtly devalues those who articulate the identity in a different way.

In a smartly written new book on recognition, Patchen Markell develops what is effectively a distinctive and novel challenge to the politics of recognition, albeit one that has certain affinities with the essentialism objection just mentioned. In Markell's view, the guiding ideal of equal recognition rests on incoherent, even impossible, ontological foundations, and the attempt to realize it is likely to lead to unfortunate social and political consequences. It may often be preferable to refuse equal recognition to subordinated groups and instead to direct what he labels a "politics of acknowledgment" at privileged and dominant groups.

The politics of recognition, Markell argues, is driven by a discredited "fantasy" of sovereign agency: a belief that if only agents could be known and respected for what they really already are, they could regain autonomous control over their lives. Drawing on Hannah Arendt, and developing his argument through readings of Sophocles' *Antigone* and (surprisingly and creatively) Hegel's *Phenomenology,* Markell seeks to show how the finite and nonsovereign character of human action continuously subverts the attempt to secure identity and agency through recognition. Recognition is not the key to undoing subordination and oppression. Instead, he concludes, subordination and oppression arise from a failure of acknowledgment, from a futile attempt by the powerful to achieve a semblance of sovereign agency through practices that are harmful to others. Subordination and oppression are thus most effectively redressed through a politics of acknowledgment that would encourage the powerful to come to terms with their own vulnerability and finitude.

Markell develops this argument by contrasting two different views of the connection between temporality and agency. He finds in the proponents of recognition a particular view of this connection that is one-sidedly backwards looking. An individual's identity is seen as fixed by the past, principally by the community into which he or she is born and its history. Sovereign agency, or "authenticity," consists in being true to this antecedently given identity, something that is assisted by the accurate and respectful recognition of identity by others and confused and thwarted by misrecognition or nonrecognition. The author contrasts this picture with one that he finds in the work of Arendt. For her, identity is something that is formed vulnerably, through risky public interactions, and so is not fully under the control of any particular individual. Our identity is something that we can perceive only in retrospect, and, as Greek tragedy teaches, achieving this final moment of self-understanding does not always produce happiness.

Markell's book is a serious piece of scholarship that should be considered essential reading for anyone interested in the topic of recognition. It develops an original line of argument and offers generally compelling rereadings of a variety of key authors, including Sophocles, Herder, and Hegel. In spite of these impressive strengths of *Bound by Recognition,* I found myself resisting its central line of argument.

The main problem, as I see it, lies in Markell's unsatisfactory treatment of the politics of recognition, whose main spokesman in the book is Charles Taylor. Markell thinks that Taylor oscillates between two different ideas of recognition, one that understands recognition as the cognition of a person's already given and fixed identity, and another that characterizes it as an act that brings something new into being (pp. 39-41). The first, cognitive sense of recognition is connected with a Herderian idea of authenticity: Recognition helps a person to be true to who she or he really already is. The second, constructive conception is associated with Taylor's emphasis on dialogue and interaction and his critique, elsewhere in his writings, of the aspiration to sovereign agency that the first view of recognition seems to reinstate. For Markell, there is a basic tension between these two conceptions. Taylor wants to say *both* that recognition should track a person's preexisting identity *and* that identity is the product of successful relations of mutual recognition (so that misrecognition can distort identity). But these views cannot both be true: "[T]he politics of recognition, then, is at odds with itself" (p. 59).

This critique does not really do justice to Taylor's account of recognition. For one thing, Taylor never characterizes authenticity as fidelity to a fixed, preexisting identity. He repeats the Herderian view that each individual has a unique identity, but he is pretty clear that the content of this identity is worked out dialogically, through risky and sometimes disastrous interactions with others (Taylor talks of "relationships" and "wrenching breakups"). Where Taylor does clearly differ from Markell (and perhaps from Arendt, who Markell cites approvingly) is in insisting that dialogue is not just something that extends unpredictably into the future. It also has a past, and that past can involve relationships of misrecognition and nonrecognition that distort or compromise the agency of some participants. The politics of recognition aims to ensure that nobody's social characteristics (skin color, sexual orientation, mother tongue, etc.) are made into a pretext for diminishing one's agency and thereby endangering the opportunity to participate as an equal in inherently unpredictable, identity-forming, and authenticity-articulating interactions with others.

I think that a way of laying this out more clearly, perhaps, than Taylor does would involve distinguishing three

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different forms of identity. My identity refers 1) to what really is of fundamental importance to or about me, something that, as Markell insists, may only be knowable retrospectively (my *retrospective* identity). It also refers 2) to what I *think* is essential to me at a given moment in time, in part because this is how others identify me (my *occurrent* identity). Finally, my identity could be understood as 3) what my retrospective identity *would* turn out to be under hospitable conditions of identity formation (my *authentic* identity). An adequate and plausible version of the politics of recognition seeks to establish the hospitable conditions needed for people to articulate their authentic identities by fighting against social relations that lock them into reduced and depreciated occurrent identities. Formulated in this way, the politics of recognition can withstand the important challenge developed in Markell's stimulating and exciting new book.

Citizenship and Education in Liberal–Democratic Societies: Teaching for Cosmopolitan Values and Collective Identities. Edited by Kevin McDonough and Walter Feinberg. New York: Oxford University Press, 2003. 464p. $60.00.

— Karen Zivi, *University of Southern California*

What is the purpose of public education in liberal-democratic societies? Is it to promote a national identity, to champion certain values, or to encourage a respect for difference? Are such goals suitable to the increasing cultural diversification of nations and the ongoing globalization of the world? And how should liberals respond when parents demand that children be exempt from classes that conflict with their beliefs or fight for state-supported religious education on the basis of group rights? These are just some of the questions taken up in this rich collection of essays. *Citizenship and Education in Liberal-Democratic Societies* brings together prominent political philosophers and educational theorists to discuss some of the most contentious educational policy issues confronting liberal-democratic states today. With a philosophical rigor often missing from such debates, the authors in this collection provide insight into the complex legal and moral issues at stake, locate the debates in historical and philosophical context, and present ethical arguments and curricular recommendations that advance dialogue. Read together, these essays defend the liberal state against charges that public education undermines parental authority and threatens cultural integrity, and they make a strong case for state involvement in education.

These arguments are advanced from a variety of theoretical perspectives entailing somewhat different policy recommendations, and thus the collection is divided into three sections. In the first, scholars take up broad themes, such as the meaning of liberalism or cosmopolitanism. In the second and third sections, we get the "dialogue" promised by the editors: Scholars in Section 2 argue for accommodating difference, while those in Section 3 urge restraint on the part of the state. More a disagreement about degrees of accommodation, these essays share far more than not.

In their introductory essay, Kevin McDonough and Walter Feinberg suggest that a philosophy of "affiliation liberalism" undergirds the essays that follow. Affiliation liberalism rejects the kind of "hyperliberalism" associated with unencumbered sovereignty and embraces the insight that individuals are shaped by their cultural and religious affiliations. Affiliation liberals, however, remain committed to a strong notion of autonomy, believing that individuals are not so embedded as to make choice impossible. While not all authors explicitly identify as "affiliation liberals," many, like Joseph Dunne, do acknowledge the significant role that "collective heritage" plays "in defining who one is," yet reject the idea that such affiliations dictate future choices (p. 96).

Developing such autonomy, "the agency to remake . . . connections according to [one's] own judgments," is one of the central educational goals of the liberal-democratic state (p. 210). In a comprehensive essay drawing on Kant and Aristotle, K. Anthony Appiah argues that autonomy is a moral good that the state is obligated to maximize. Autonomy, allowing individuals to make their own choices about how they live, requires exposure to alternative ways of life and the development of a capacity for critical self-reflection and a respect for difference. Since children are not born fully autonomous, the state should help cultivate their autonomy through education, teaching children skills, knowledge, and values, such as learning about one's local community in relation to global issues, participating in debates, or learning to listen carefully.

While all the scholars seem to agree that cultivating autonomy is of critical importance, some specifically highlight the importance of promoting a cosmopolitan orientation among children. In a very original argument, Melissa Williams links the development of autonomy to an understanding of "citizenship as shared fate." This notion of citizenship, less static or state-centric than "citizenship as identity," acknowledges that the past and present shape who we are and how we interact, and that our futures are inextricably intertwined. Education for the citizenship of shared fate requires teaching students the capacity for "enlarged thinking" or "an ability to see oneself in relation to different others [that] requires encounters with actual and immediate diversity" (p. 240). Jeremy Waldron offers examples of how education already promotes such citizenship, while Harry Brighouse illuminates the dangers of education for patriotic attachment alone. Their essays suggest that cultivating cosmopolitan citizenship requires appreciating the extent to which the legal, economic, and cultural practices in which we are already engaged are intertwined, such that good national citizenship often coincides with good global citizenship.

The exposure to alternative lifestyles and beliefs championed here is precisely what many parents and groups object to, leading to the claim that public education threatens

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group identity. Shelley Burtt's provocative essay suggests that such arguments have validity. Children do not need to go to public schools in order to become good citizens; "comprehensive education" can serve this purpose just as well. In fact, a comprehensive education that seeks to inculcate the "worldview, personal commitments, and moral understandings" of parents and religious communities may promote autonomy better than do secular schools (p. 179). By encouraging children to turn inward—to ask questions such as "What sort of person am I? and why?"—comprehensive education enables the development of virtues, such as practical reason, moral courage, and imaginative empathy (p. 204). Thinking independently does not necessarily emerge from exposure to different ways of living, a marketplace of ideas, or an external orientation to choosing. It may just as readily emerge from looking inward.

Of course, as Burtt acknowledges, not all comprehensive educations are equally concerned with promoting autonomy. What is to be done under such circumstances is the focus of Section 3. Here we find justifications for "tilting the playing field in favor of liberalism" (p. 385). Robert Reich and Susan Okin remind us that the accommodation of group difference often places individual freedom and equality in jeopardy. The right of exit meant to protect individuals from such perils is often inaccessible to children and women, or entails such significant costs as to make it an unattractive option. These essays thus caution against the uncritical accommodation of difference while defending state-supported educational constraints. "[T]aking a stand *against* discrimination and *for* other basic liberal values" by mandating attendance or certain courses of study is not necessarily "illiberal," argues Stephen Macedo (p. 428).

Or is it? So nuanced and far-reaching are the arguments in favor of the cultivation of autonomy, whether in the realm of secular or comprehensive schooling, that one wonders who could possibly disagree. The addition of strong arguments from nonliberal perspectives would go a long way toward creating the productive dialogue sought by the editors. Additionally, more careful policy analysis and recommendations would be needed to satisfy those wondering about how such education can be promoted, given budgetary constraints, the need for teacher training (an issue briefly mentioned by Terence McLaughlin), and new Bush administration policies. This latter criticism is somewhat unfair, given that the conference upon which the book is based was held prior to the Bush administration and that the collection is meant as a work of philosophy rather than policy.

Indeed, as such, the collection is a resounding success, contributing to scholarly debates in a wide range of fields and providing a valuable classroom resource for those interested in the intersection of theory and policy. Anyone new to the debates about education in a multicultural society or liberalism's place in a cosmopolitan world will find some of the most important statements on these issues, as well as a detailed, if somewhat disjointed, review of the literature on these subjects in this text. Scholars interested in educational policy will find more than a few recommendations to mull over, while those interested in liberalism will be forced to consider the educational implications of their own philosophical commitments. The philosophical depth of this collection, the very careful attention paid to the meaning of terms and the assessment of counterarguments, is one of its great treasures. The voices collected here exhort us to move beyond political sound bites that suggest stark difference to identify those principles upon which we already agree. It is from here, they suggest, that we can begin to craft educational policy for the new millennium.

Enlightenment Against Empire. By Sankar Muthu. Princeton: Princeton University Press, 2003. 348p. $19.95.

— Jacob T. Levy, *University of Chicago*

In this rich and elegantly written book, Sankar Muthu breaks the now-stereotypical association of Enlightenment political thought with an arrogant universalism that was implicitly or explicitly allied to imperialism. He suggests than an important strand of eighteenth-century political thought generated, and was intended to generate, a searching critique of the conquest of the world by European states. Indeed, far from being uniquely aligned with imperialism, eighteenth-century political thought was distinctive for generating such an opposition. Neither the two centuries that preceded it nor the century that followed saw such a systematic stand against imperialism from European philosophers.

Muthu's provocative thesis is that moral universalism did not suffice to ground opposition to empire. Natural rights theory, for instance, proved all too compatible with conquest. But neither did antiimperialism find its roots in a simple embrace of cultural relativism. Instead, the opposition to empire flowed from a subtle blend of universalism and pluralism—from the view that one of the universally shared and morally fundamental human capacities is the capacity for culture creation, and that the cultures thereby created are each, and incommensurably, valuable. This is opposed by not only explicitly racist views but also by accounts of natural human equality that characterize non-European peoples as "natural," that is, in the state of nature, and therefore presocial and, crucially, precultural.

Muthu's method is to delve deeply into the arguments of a few thinkers. This is not a book about the *political project* of empire, nor about the political positions of eighteenth-century theorists. The most politically engaged eighteenth-century antiimperialist thinker, Edmund Burke, is hardly mentioned; and the greatest share of the book is devoted to one of the least, Immanuel Kant. This is a book about the *philosophical underpinnings* of imperialism and antiimperialism, the intellectual and moral rather than the political structure of the eighteenth-century intra-European debate

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about Europe's conquest of the world. If this move comes as something of a surprise, after so many works about John Locke's involvement with the Carolinas or Burke's work on India and Ireland, it turns out to be a most worthwhile surprise. *Enlightenment Against Empire* moves beyond accounts of political positions into thoughtful reconstructions and syntheses of arguments, and among its many virtues is to remind us that work in the history of political thought need not be done in an antiphilosophical fashion.

The major figures of the book are Jean Jacques Rousseau (as something of a foil) and Denis Diderot, Kant, and Johann Gottfried von Herder. Muthu brings Kant and Herder, in particular, much closer together than they are usually understood to be, by stressing both Herder's moral universalist concern with humanity and Kant's theories of cultural agency, antipaternalism, and noninterference. The treatment of Herder is an exceptionally careful and subtle one, and Muthu persuasively draws out of him a greater coherence of normative argument than is sometimes perceived in his work. Nonetheless, the association of Herder with antiimperialism comes as no great surprise, and it has long been recognized that his views were subtler than a simple cultural relativism. Kant is the hard, counterintuitive case, and also the most important case. Accordingly, Kant is the subject of the largest portion of the book; and the book must be counted an important addition to the literature on his political and social thought.

These chapters contain three major steps. Muthu portrays Kant's theory of virtue and freedom as being, to an important degree, a theory of cultural agency. He argues that this leads Kant to a theory of cultural incommensurability. And he treats both of those two steps as grounding Kant's antipaternalism and his critique of European imperialism. Even reminding readers that Kant *made* such a critique is a valuable service, given the use and abuse of Kant as a synecdoche for arrogant cultural imperialism or utopian cosmopolitanism. But Muthu accomplishes much more than that.

The first of the three steps is masterfully accomplished. The third, the account of Kant's antipaternalism and antiimperialism, is compelling and persuasive, and serves to tie together themes from a wide variety of his writings. The account of incommensurability is not quite as successful. Muthu shows that Kant thought that no society had the right to rule another; that European states, in particular, were unworthy of being entrusted with the fates of other peoples; and that agricultural peoples were not superior in kind to nomadic or pastoral peoples. Those claims suffice to fill the gap between cultural agency and antiimperialism, without requiring the more radical and mostly inferred claim of wholesale cultural incommensurability. That a ranking of cultures does not authorize conquest, and that complete cultures are the wrong kinds of things to be ranked, is not the same as thinking that discrete cultural practices cannot be ranked along any dimension. The evidence Muthu has offered is compatible with, but does not compel, a reading of Kant as subscribing to incommensurability.

Two crucial substantive themes run through Muthu's discussions of Diderot, Kant, and Herder. The first is land use and property. The equation of settled agriculture with property rights, justice, and civil government was to be found not only in Locke but also in theorists of international relations such as Hugo Grotius and Samuel von Pufendorf, and in Rousseau's celebration of the naturalness of non-Europeans. For all of these thinkers, there was a difference in kind between the social organization of European agricultural and commercial states and the precivil existence of nomadic and pastoral peoples. Defeating imperialist arguments required denying this radical asymmetry. In particular, it required refuting both the idea that no settled agriculture meant no *property,* so that non-Europeans could be understood as having rights against dispossession, and the idea that no settled agriculture meant *no government or sovereignty,* so that non-Europeans could be understood as already living under systems of law and social organization that should not be replaced by European rule.

The second theme is the distrust of European states. Muthu suggests that this distrust diminished in postrevolutionary Europe, and that this helps to account for the decline in antiimperialist thought. In any event, he shows the degree to which Diderot, Kant, and Herder integrated their critiques of imperialism with critiques of the domestic character of eighteenth-century European states and societies.

One important historigraphic claim recurs several times and occupies the book's concluding chapter. The argument, not entirely novel but receiving a particularly forceful and effective defense here, is that more is lost than is gained by thinking of a category called "The Enlightenment." "It is high time," Muthu writes, "that we pluralize our understanding of 'the Enlightenment' both for reasons of historical accuracy and because, in doing so, otherwise hidden or understudied moments of Enlightenment-era thinking will come to light" (p. 264). He has offered a marvelous example of what research on such "hidden or understudied moments" can accomplish.

Plato Through Homer: Poetry and Philosophy in the Cosmological Dialogues. By Zdravko Planinc. Columbia: University of Missouri Press, 2003. 152p. $37.50.

— Kateri Carmola, *Middlebury College*

This is the kind of book that gives great pleasure to a reviewer. Not only does it reward the act of taking time away from one's own reading agenda to evaluate an unknown and undiscovered text, but it warrants a strong recommendation that others do the same. This review will provide a summary of the refreshingly simple and provocative

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argument of Zdravko Planinc's *Plato Through Homer,* and a short explanation as to why this small volume is worthy of our attention.

I must admit that at first glance, I was not so well inclined. Although the topic of the connections between Plato and Homer sounded good, the author was unknown to me, and the first impression from the Acknowledgments did not bode well (do we need to admit that this is where we academics begin looking for intellectual roadmaps and the human being behind the scholarship?). Beginning with a promising description of which kinds of translations and texts helped the author's interpretation (Harold Bloom, Seth Bernadete, Richmond Lattimore, the Loeb series, and the Perseus online reference library), the author goes on to acknowledge the kinds of conversations that promote an understanding of Plato and Homer (those with "students and colleagues, with business men and poets," "less frequently with other scholars," and best of all, with the "one you love"). This bespoke a certain ominous campiness, and the final paragraph, listing intellectual influences as diverse as Miles Davis, François Truffaut, and Martin Scorcese, and revealing a "misspent youth playing in a rock group," as well as moments of profundity, including "moonrise at a borrowed cottage in the Kawarthas; a stone, a leaf, a door," seemed to seal the evaluation. What was this book? Who was this person? Did I need to care (or know that the cottage was borrowed)?

These initial impressions, however, soon gave way to a real admiration for both the disarmingly simple argument of the text and the style with which it is made. The argument is as follows: In order to fully understand the meaning of some of Plato's most crucial dialogues—the *Republic,* the *Laws,* the *Timaeus,* the *Phaedrus,* and the *Critias*—we have to understand the ways in which Plato maps these dialogues onto one of the most canonical stories for the Greeks: Homer's *Odyssey.* In Planinc's terms, Plato "refigures" the dramatic events and insights of Odysseus's journey and homecoming into the dramatic arguments and conversations of these dialogues. The epic journey of wandering and homecoming is thus reenacted in the Platonic dialogues, not as an epic of war and homecoming but as a journey of the "greater hero," Socrates, who finds his own homecoming in the *Laws.* (See also Planinc's earlier book, *Plato's Political Philosophy: Prudence in the Republic and the Laws.*)

There are a number of themes that guide this refiguring process, themes common to both "journeys": curiosity about other peoples and other ways of thinking, a search for a kind of inner balance and harmony, love of another and the erotics of recognition, and the possibility of failure and attendant destruction, of warlike virtues, and of an inadvertent love of honor destroying cities and souls.

Planinc thus interprets significant moments in Plato's dialogues through the lens of their equivalent events in the *Odyssey.* The provocative beginning of the *Republic,* wherein Socrates recounts the day's events to an unknown interlocutor by saying "I went down . . ." is mapped onto the descent of Odysseus into Hades, or in Planinc's terms, Plato "refigures" Odysseus's descent into that of Socrates. Similarly, the Demodocus's song of Ares and Aphrodite in the *Odyssey* is refigured in Timaeus's Pythagorean cosmology. And the struggles of Odysseus with his crewmen are refigured in Plato's account of Socrates' struggles with his interlocutors in the *Phaedrus* and *Symposium.*

Central to Planinc's argument is the strange symbol of the shaman, strange to Plato scholars certainly, though not to interpreters of literature or epics. Admittedly, I found this idea at first hard to swallow, but the point is simple enough: Both Odysseus and Socrates can be seen as shamanistic characters, who descend into the depths of the underworld and ascend into other heavenly or divine worlds by means of an axis that joins the heavens and the underworld. This method yields significant results, and some of the perennially obscure passages in these dialogues are illuminated. Especially good is his analysis of Teiresius's prophecy and Odysseus's return to Penelope, which he argues is refigured in Plato's myth of Er, in Book X of the *Republic.* Strange as it may seem, the argument works.

Both Plato and Homer have, of course, been interpreted and reinterpreted, used and abused, for centuries by all sorts of scholars. The fact that the beauty and mystery of their writings continues to enthrall us, and the fact that none of the scholarship can do justice to the delight in reading the original, should give us pause. What really continues to elude us in these works? How can we approach them best? How can we listen, as we are repeatedly invited to do, and not get them wrong?

Planinc makes his own argument with very little reference to other scholarship except in the footnotes. He approaches both texts on his own, with no discernible political or scholarly agenda. The scholars' views that he refers to, however, represent a broad range: Eva Brann, Wendy Doniger, Jacques Derrida, Martha Nussbaum, Seth Bernardete, to name a few.

The field of Plato scholarship is often tasked with helping us read these enigmatic dialogues. Whether they are seen as a coherent whole, with carefully constructed commentary on political life or metaphysics, or virtue, or epistemology, or whether they are deconstructed by word or image, and seen more as reflections on themes that only poetry can encompass (art, image, imitation, eros), they call out for interpretation. Even so, typical interpretations attempt to explain away the mysterious aspects of the texts. In contrast, Planinc approaches the works of both Plato and Homer by means of the experience of reading them—awe, delight, confusion—and shows how by seeing each as commenting on the other, certain aspects are revealed as deeper and richer than previously thought. The reader is drawn into the argument, which is at times dense and impenetrable, and at other times scattered and disorganized, but the end result is a reawakened delight at the richness of

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both texts and the pleasures of simple reading. Both the substance and the technique of this small volume result in a renewed desire to read Plato and Homer, and to see them as not only political and philosophical texts but as interconnected works of literature.

The Cultural Defense. By Alison Dundes Renteln. New York: Oxford University Press, 2004. 416p. $45.00.

— Philip Kronebusch, *St. John's University, MN*

While there is a burgeoning scholarly literature on such topics as cultural pluralism, ethnic diversity, and the politics of recognition, there has been relatively little systematic attention given to ways that courts can embrace or thwart the maintenance of cultural traditions. Yet, as is often the case, court cases provide concrete examples of conflicts that capture the attention of the popular press and the public. If polygamy is part of a religious or cultural tradition, can state and federal laws forbid it within the United States? If a recent immigrant to the United States belongs to a tradition that permits an "honor killing" to punish the improper sexual behavior of a female relative, to what extent is that immigrant legally culpable? Many of the cases discussed in the book present extraordinarily difficult conflicts between cultural traditions. The author should be praised for presenting them unflinchingly.

The chief virtue of the book is the author's cataloging of these cases of cultural conflict. In her organizational scheme, Alison Dundes Renteln distinguishes among cases that involve homicide, children, drugs, animals, marriage, attire, and the dead. A few of these cases have risen to the U.S. Supreme Court. In the chapter on drugs, *Employment Division, Department of Human Resources of Oregon v. Smith* (1990), which involved the use of peyote by Native Americans in religious ceremonies, is an example. But many of the dozens of cases that the author uses are cases that may have seen only a trial court, or perhaps a midlevel appellate court. She also sought to include cases from countries other than the United States, but these form a minority of examples.

Renteln argues that litigants should be able to offer a cultural defense, which "would require judges to consider the cultural background of litigants in the disposition of cases before them" (p. 5). Part of her argument is based on evidence that courts, on occasion, do already include consideration of the cultural background of a defendant in dismissing or lowering a criminal charge, or in reducing a sentence. But current practice is inconsistent. Often, courts will exclude testimony about a defendant's culture, or else the court will bend a traditional defense, like "diminished capacity," to have the same effect as a cultural defense would. Open recognition of a cultural defense, Renteln argues, would improve the candor of courts and permit defense attorneys to bring in expert witnesses who could testify on the cultural background of a defendant. She also argues that a cultural defense is supported by the U.S. Constitution's guarantees of equal protection, freedom of association, religious freedom, effective counsel, and the right to a fair trial. The presentation of a cultural defense would not necessarily exonerate a defendant, but a cultural defense could provide a "partial excuse" and "juries could decide whether cultural factors were determinative in a defendant's behavior" (p. 188).

One example from the chapter on "Animals" is the 1986 federal prosecution of James Billie, a Seminole Indian, who hunted a panther that is listed as an endangered species on reservation land in Florida. The district court explicitly rejected Billie's cultural defense that claimed panther parts were important for a religious ceremony. The court found that Billie had not demonstrated that panther parts were required for the ceremony, but were only preferable. Renteln responds that the court ought to have acknowledged the religious significance of the panther parts and "then discussed an appropriate compromise," because Seminole Indians, themselves, would not want the panther to become extinct (p. 99).

While the dozens of cases discussed are the book's chief virtue, the quick treatment of so many cases leaves important details and complexities unexplored. For example, a paragraph on a Canadian case describes a Sikh man who filed a complaint with the Canada Human Rights Commission against an airline because he was not allowed to board an aircraft unless he removed his kirpan, a small dagger that Sikhs wear as a matter of religious duty. Renteln provides a short summary of the conclusion of what she calls "a fascinating decision" (p. 153), though a fuller discussion of that decision would have helped her reader see some of the complexities involved in a court's attempt to evaluate cultural evidence. The commission's decision includes a discussion of the testimony provided by three expert witnesses on the beliefs and practices of Sikhs and on whether there is agreement among Sikhs on the size of the kirpan or whether or not the kirpan must have a sharp blade. The commission's own summary of this testimony was that Sikhs hold many different views regarding the kirpan and that there is no single agreed-upon position. A fuller discussion of this decision would have allowed Renteln to further develop her conception of how courts, in practice, could evaluate evidence of the importance of cultural traditions.

The extensive endnotes and bibliography will serve as an excellent resource for those seeking a summary of arguments both in favor of and opposed to a cultural defense. However, a few arguments opposed to the adoption of a cultural defense, or at least arguing for a more limited cultural defense, are given cursory treatment. For example, a significant number of scholars have argued that a cultural defense is likely to be invoked by men who are accused of violence against women. Men may, it is argued, use a cultural defense to defend cultural traditions of forced marriages, honor killings, and female genital cutting, for example. Susan Moller Okin's *Is Multiculturalism Bad for*

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Women? (1999) is perhaps the best-known representative of this view. While Renteln recognizes the argument at a number of points, Okin's work is dismissed quickly as an attempt "to trivialize the cultural defense by associating it mainly with female genital cutting and forced marriage" (p. 227).

The specific cases that the author summarizes and the bibliographic references make *The Cultural Defense* an extraordinarily valuable resource for further research into the role of courts in evaluating and using cultural evidence.

Machiavelli, Hobbes, and the Formation of a Liberal Republicanism in England. By Vickie B. Sullivan. New York: Cambridge University Press, 2004. 284p. $75.00.

— Lee Ward, *University of Regina*

In this important and insightful book Vickie B. Sullivan offers an impressive and ambitious examination of the philosophical roots and historical development of liberal democratic theory in early-modern England. In the Introduction, Sullivan frames her analysis in the context of the various contours of the debate about the character of early-modern thought between the advocates of classical republicanism, on the one hand, and the proponents of the liberal school, on the other. The opening chapters on Machiavelli and Hobbes, respectively, provide a provocative interpretive lens through which to evaluate and critique the prevailing liberal and republican paradigms. The lion's share of the book deals in five successive chapters with the way in which English republicans from the civil war era through the early Hanoverian period—including Marchamont Nedham, James Harrington, Henry Neville, Algernon Sidney, and the coauthors of *Cato's Letters,* John Trenchard and Thomas Gordon—modified, balanced, and ultimately synthesized the Machiavellian republican and Hobbesian liberal elements of their complex philosophical inheritance. With the final consummation of this synthesis in the commercial republic of *Cato's Letters,* Sullivan argues, a distinctively modern form of liberal republicanism was born.

The central argument of this thoughtful and engaging book is that the current scholarly debate between liberals and republicans is fundamentally misguided and counterproductive. Sullivan argues that rather than viewing liberalism and republicanism as moral and political antipodes, we must recognize that both concepts share a common root in a characteristically modern idea of human nature and the limited purposes of government. Her illumination of the inherently "complicated, intertwined nature of liberalism and republicanism" (p. 2) seen in the relationship between the thought of Machiavelli and Hobbes is a tour de force. In contrast to most commentators who associate republicanism, and by extension Machiavelli, with the classical Aristotelian conception of political life, Sullivan exposes with subtlety and clarity the underlying connection between Machiavelli's republicanism based on popular political participation in the service of imperial expansion and Hobbes's liberal philosophy of government directed to peace and the security of rights. She argues persuasively that Hobbes and Machiavelli express divergent, but interconnected, commitments emanating from a common source in the modern rejection of the ancient idea of citizenship, organic community, and the good life (pp. 4–5, 22). Perhaps the most compelling piece of evidence Sullivan presents for the underlying connection between Hobbes and Machiavelli is the palpable sense in which English republican theorists freely incorporated ideas from both thinkers into their own arguments for fundamental political change in early-modern England. Thus, these two very different figures cast a long shadow over the English commonwealth tradition, profoundly shaping all efforts to produce a modern republicanism that "absorbed liberal purposes and improved upon ancient political practices" (p. 3).

The heart of this book's account of the formation of liberal republicanism is a carefully developed and tightly reasoned treatment of the English commonwealth tradition from Nedham to Cato. Admirably combining attention to historical context with close textual analysis, Sullivan argues that despite important differences between and among the English opposition writers, they shared a common project of trying to blend, balance, and ultimately harmonize the insights of Hobbes and Machiavelli. The tension between Harrington's and Sidney's endorsement of acquisitive imperialism and Nedham's and Neville's experimentation with contract theory culminates, Sullivan suggests, in the synthesis of liberalism and republicanism manifest in Cato's vision of a commercial republic in England. In Cato's "final reconciliation between liberalism and Machiavellian republicanism" (p. 251), we see the product of the fusion between Machiavelli and Hobbes, a republicanism purged of militarism and the liberal idea of equality and rights freed from the hold of authoritarian politics.

Machiavelli, Hobbes, and the Formation of a Liberal Republicanism in England makes a valuable contribution to the study of early-modern political thought that extends beyond illuminating such seminal thinkers—itself an impressive feat—and goes further to provide a comprehensive account of the way in which a number of well-defined ideas and principles shaped the development of nascent liberal and republican theory in England. Sullivan compels us to reevaluate what we know (or think we know) about the commonwealth tradition first brought to prominence by Caroline Robbins, J. G. A. Pocock, and Bernard Bailyn more than a generation ago. However, this study does more than most previous efforts to connect the historical context of the English constitutional struggles of the turbulent seventeenth-century with the fundamental philosophical reflections on first principles à la Machiavelli and Hobbes that informed these fierce political debates. Some readers will no doubt be introduced for the first time to such thinkers as Nedham and Neville, while all are encouraged to examine more familiar figures, such as Harrington and Sidney, with new eyes

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sensitive to the philosophical undercurrents of their arguments. Sullivan repeatedly demonstrates that there is more of theoretical import going on in the texts of these commonwealthmen than first appears as they worked to assimilate Machiavellian and Hobbesian philosophical materials in a distinctive liberal republican formulation.

For those readers interested primarily in Machiavelli and Hobbes, this book offers penetrating and refreshing interpretations of these important thinkers. Building on her previous, highly regarded work on Machiavelli (*Machiavelli's Three Romes: Religion, Human Liberty, and Politics Reformed,* 1996), Sullivan illustrates the thoroughly modern basis of Machiavelli's republicanism, which advocates a political populism rooted in the needs of imperial expansion, rather than a celebration of individual rights. In her provocative reading of Hobbes, Sullivan suggests that the egalitarian philosophical premises of the English archroyalist actually revealed "impulses [that] are more democratic" (p. 83) than the Florentine, and thus she constructs a persuasive argument that the English republicans may be understood to have radicalized the process of deepening "Machiavelli's democratic leanings" begun by Hobbes (p. 138). For others more concerned with the broader scholarly dispute about liberalism and republicanism, Sullivan brings probity and rigor to a debate too often marked by sweeping generalizations and antagonistic interpretive paradigms. This book challenges the assumption that republicanism is inherently classical in origin, as well as the conventional wisdom regarding the incompatibility of the logic of individual rights and popular participation in government.

John Locke, so often the fulcrum in the debate about the Founding of America, the commercial republic par excellence, is an important, if largely implicit, presence in this book. Locke's impact on the formation of liberal republicanism is most evident indirectly via *Cato's Letters* (pp. 234–39), the only post-Lockean work treated in this study. One implication of the book may be a welcome call to reevaluate the status of Locke in the early-modern period with an approach recognizing him as neither cipher nor cynosure, but rather a complex thinker who contributed to the formation of liberal republicanism by offering his own unique vision of individual rights and popular government. This admirable book invites such a line of investigation as Sullivan encourages us to follow her tracks through Machiavelli, Hobbes, and the English commonwealthmen to a fuller understanding of what is distinctively Lockean in the Anglo-American tradition.

AMERICAN POLITICS

Overruled?: Legislative Overrides, Pluralism, and Contemporary Court–Congress Relations. By Jeb Barnes. Stanford: Stanford University Press, 2004. 219p. $50.00.

— Forrest Maltzman, *George Washington University*

Students of the interaction between the judicial and elected branches of government typically assume that each branch can understand the other's intentions and capabilities. In classic separation-of-power models, justices do not craft opinions they know will be overridden, and Congress does not pass laws likely to be struck down or interpreted in a manner hostile to Congress's interests. In reality, the assumption of complete information is tenuous: The transmission of information between the branches is less than perfect. Members of Congress lack the capacity to perfectly anticipate future judicial decisions, and courts cannot anticipate how elected officials will respond. Hence, congressional goals are regularly thwarted by the courts, and on occasion, judicial attempts at policymaking are overruled by Congress.

In *Overruled?,* Jeb Barnes asks a question that students of American politics have ignored for too long: What happens when Congress adopts legislation that undoes the judiciary's interpretation of a federal statute? He explores this question from two perspectives. First, he asks how federal courts respond to congressional efforts to override previous interpretations of federal statutes. Second, he explores the character of the legislative process when Congress attempts to overturn judicial decisions.

Barnes randomly identifies 100 episodes in which Congress attempted to override a decision of the bench that involved statutory interpretation. He then compares judicial consensus (that is, the existence of agreement among all of the federal appellate and Supreme Court justices called upon to interpret the statute) before and after congressional action. He discovers that judicial consensus is significantly more likely after Congress attempts to undo a previous judicial interpretation, suggesting the courts' willingness to be overruled by Congress. Barnes also explores the openness of the decision-making process used by Congress in responding to the courts. To assess this openness, he examines whether competing interests are afforded the opportunity to testify before congressional committees during consideration of measures to overrule the courts. He concludes that the legislative process is typically quite open during such times, suggesting the receptivity of Congress to multiple interests in reviewing the work of the courts.

This book is a great example of persuasive social science research. Barnes asks innovative questions, advances our understanding of complex phenomena, and systematically marshals convincing data to test empirical conjectures. Throughout, Barnes combines both case studies and quantitative evidence to convince the reader that his conclusions are sound. When the coding process is subjective, he confirms the reliability of his judgments by relying on several colleagues to independently code the data. Rather

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than merely reporting the patterns he observes, he gives the reader enough information to independently assess the significance of these patterns. The result is an important book that advances our understanding of the relationship between Congress and the courts. It generates a number of hypotheses that students of American politics will have to grapple with in the years ahead. From a normative perspective, Barnes's book leaves the reader optimistic about the democratic character of the American political system. Contrary to those who fear an imperial judiciary, elected branches of government are able to keep the courts in check, overruling their interpretations of law and correcting judicial mistakes. And contrary to those who believe that narrow special interests may hijack the legislative process, Barnes suggests otherwise.

The author's analysis raises a number of questions for students of Congress and the courts. First, as Barnes recognizes, his sample of legislative efforts is drawn from a list of cases in which Congress successfully overrides a previous court decision. But if Congress fails to act if it suspects another challenge from the judiciary, conclusions about the courts' acquiescence to Congress may be premature. As he himself makes clear, his empirical tests are designed to determine the consequences when Congress successfully overrides the federal courts.

Second, Barnes's empirical tests do not distinguish between statutory decisions made by the Circuit Courts of Appeal and those made by the Supreme Court. As it turns out, the majority of the override efforts involve decisions of the lower federal bench. This pattern has implications for the author's conclusions about the rise in judicial consensus after congressional action. One possibility is that the increase in judicial consensus after such action stems from the behavior of strategic groups who "circuit shop" after Congress strikes down a decision stemming from a particular circuit. Another possibility is that judicial consensus may appear greater after congressional action if the case in question was reviewed by the Supreme Court before—but not after—congressional action. Consensus is typically greater when the panel is smaller, as on a lower appellate bench. Another possibility is that Congress has a different relationship with the federal appellate courts than with the Supreme Court. Given that decisions of the Supreme Court are not subject to further judicial review, the Supreme Court may be less deferential to Congress than are the Circuit Courts of Appeal. Although these complications are unlikely to account for the entire increase in judicial consensus before and after congressional action and may account for an insignificant amount of the increase, future empirical work based on a larger number of cases and multivariate controls should explore these alternative explanations.

Finally, future research might return to Barnes's conclusions about the openness of the legislative process, as determined by the mix of individuals testifying before Congress during override attempts. Such testimony certainly indicates who is afforded the opportunity to shape legislative measures. But it may also reflect Congress's efforts to create the appearance of openness, before it proceeds to mark up legislation ignoring the full range of views presented in congressional hearings. Additional indicators of congressional openness would bolster Barnes's intriguing findings.

Regardless of any limitations, *Overruled?* should become must reading for students of Congress and the courts and of the strategic interaction of the branches. Barnes has significantly advanced our understanding of the separation of powers, and in doing so, has made a persuasive case that our political system may be healthier than we typically recognize.

Real Democracy: The New England Town Meeting and How It Works. By Frank M. Bryan. Chicago: University of Chicago Press, 2003. 312p. $49.00 cloth, $19.00 paper.

— G. Thomas Taylor, *University of Maine*

This book joins a small number of empirical studies about direct democracy and, specifically, the town meeting. Frank M. Bryan is author, coauthor, or editor of 11 books, including *Politics in the Rural States* (1981) and *The Vermont Papers: Recreating Democracy on a Human Scale* (1989). Here, he unveils an incredible set of discoveries that fills a major gap in the literature on democracy and the town meeting. Other prominent scholars who have tackled some of these salient issues are Joseph Zimmerman (*The New England Town Meeting,* 1999) and Jane J. Mansbridge (*Beyond Adversary Democracy,* 1980), both of whose works are referenced extensively in *Real Democracy*.

Bryan has performed yeoman's work in this landmark comparative study of town meetings that has evolved over three decades from 1969 to 1998. With help from his students as a part of their course responsibilities, he designed a framework for data collection and analysis of 1,435 Vermont town meetings held in 210 different towns (p. 266). Typically, his research teams would observe and document approximately 50 town meetings each year. One example of a significant finding from this massive longitudinal study was that "on average only 20 percent of the town's registered voters attended their yearly town meeting . . . and only 7 percent of them spoke out" (p. 280). Collecting data from hundreds of these towns is a remarkable feat because they often meet simultaneously in March, causing a logistical problem. Before Zimmerman's book, there were no aggregate data on attendance rates of the New England towns.

To analyze this huge set of data, Bryan employed standard statistical techniques of multiple regression, correlation coefficient, and the Gini index. In addition, he crafted several original indices, for example, the raw best democracy index (RBDI) and the controlled best democracy index (CBDI). He and his students identified 238,603 acts of participation by 63,140 citizen-legislators that form the basis for this study.

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The author argues that what happened at a town meeting in Athens, Vermont, in 1992 was not unusual or random, or even very unique, but real democracy. In the United States, town meetings have predated representative government. He further claims: "It is accessible to every citizen, coded in law, and conducted regularly in over 1,000 towns. In my state of Vermont citizens in more than 230 towns meet at least once each year to pass laws governing the town" (p. 3). Moreover, using the word "democracy" to mean a representative system is, as Robert Dahl concluded, "an intellectual handicap" (Dahl, "The City in the Future of Democracy," *American Political Science Review* 61 [December 1967]: 953–69.) Real democracy for Bryan happens only when eligible citizens of a general purpose government function as the legislative body. They must meet in a deliberative, face-to-face assembly and agree to follow the laws and other decisions they have made. In their visible laboratory, real democracies evolve in better or worse forms and promote good and bad policies. Real democracy functions best when those who dwell in a community are citizens and all are able to participate (p. 4).

Bryan states that the purpose of his book is "quite modest: to take the guess work out of fundamental things we ought to know about real democracy" (p. 18). His remarkable comparative study of Vermont town meetings analyzes data about real democracy never before assembled. Some of his key questions are the following (p.18): What percentage of registered voters attend and speak? What issues are discussed and what is the length of meetings? Do a few persons dominate? Are there significant differences between women's and men's participation? How much conflict occurs? Do officers dominate discussion?

The book is a complex blend of the author's own colorful and witty partial autobiography with his persistent effort to conduct both a quantitative and qualitative analysis of Vermont town meetings. Its organizational structure includes a preface and introduction, which explain his purpose, passion, and methodology. He also integrates a meaningful comparison between Athenian democracy (500 B.C.) and that in Athens, Vermont, where the former also includes the "demes" (139 small village governments). What follow are 11 fascinating chapters, including an essay on the town meeting (a well-documented historical account that traces the image and reality of the American experience with real democracy through all forms of literature). Another chapter introduces the readers to several town profiles and the importance of size; two chapters each are devoted to attendance, public talk at the meeting, and equality and women's participation; other chapters include issues and participation, best and worst cases of town meeting practices, and a conclusion on their purpose and future. Readers also may consult a thorough and useful lab report at www.UVM/~fbryan.

Numerous insightful findings appear throughout this seminal work: "[O]ne thing is certain: community size must lead the attempt to build a working model of real democracy" (p. 72). Size matters: "When we take into account its curvilinear properties, the size variable is extremely powerful, explaining 58 percent of the variance in attendance at town meeting. As far as citizen presence is concerned, real democracy works best in small towns. Of this there is no doubt" (p. 81). In addition, "Beyond meeting size, issues are the most important determinant of discussion at town meeting" (p. 233).

Searching for shortcomings boils down to different preferences and the author's discretion. For example, there is an inordinate amount of significant and juicy commentary in many of the chapters' copious footnotes that could perhaps be better integrated into the main body text, thereby making the book more reader friendly. Another option would have been to locate some of the more technical tables, plots, and figures in a methods-oriented appendix (for the more advanced readers).

The prospective audience for this significant book will likely be diverse, including political scientists (and their students in advanced undergraduate and graduate courses, especially in democratic theory and state and local government), as well as appointed and elected local government officials. It also would naturally appeal to community and neighborhood advocates and general readers who are rightly concerned about the future of American democracy.

The irony is that *Real Democracy* is very strong on comparisons within Vermont but has only a few passing references to the other New England states. Maine, for example, has twice as many towns and still retains the pure town meeting in most of its smallest towns. However, an appointed executive, the town manager, has been grafted onto the traditional board of selectman–town meeting form in many of its communities. There are other thorny issues: Is the meeting affected by the growth of staff and professional management? Is the town meeting a luxury enjoyed by communities under 2,500 in population? How many meeting abandonments (or other reforms, such as the representative town meeting) have occurred in the New England states? Frank Bryan has opened the door of his archive (which he will certainly continue to mine for even more "jewels") and will thereby challenge other scholars to venture beyond representative democracy and join him in the search for and understanding of the real democracy that most modern-day political scientists have bypassed.

The Politics of Child Support in America. By Jocelyn Elise Crowley. New York: Cambridge University Press, 2003. 217p. $65.00 cloth, $23.00 paper.

— Lael R. Keiser, *University of Missouri, Columbia*

Jocelyn Crowley argues that child support policy is an example of innovation through entrepreneurial activity over time. Relying on secondary and original sources, she identifies four different sets of entrepreneurs—social workers,

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conservatives, women legislators/women's groups, and fathers' rights groups. Crowley claims that over the long term, policies continuously evolve and actors enter, exit, and reenter the political arena. Policy entrepreneurs are the central actors who create policy change. She defines policy entrepreneurs as people who are alert to new opportunities, persist in advocating their ideas, and employ rhetorical ingenuity to frame their ideas in novel ways (p. 8). Groups, as well as individuals, have these entrepreneurial characteristics. Changes in child support policy, in the author's view, can be explained by changes in the domination of the political arena of different entrepreneurial groups.

Policy change occurs, therefore, because new entrepreneurs (challenger entrepreneurs), displace the entrepreneurs who successfully influenced the policy process in the past (incumbent entrepreneurs). According to Crowley, the success of these entrepreneurial groups depends on the strategies these individuals and groups use to displace incumbent entrepreneurs. Building on literature in economics, she argues that two strategies are important. First, entrepreneurs must follow a risk-reducing strategy. She defines this strategy as spreading the risk of political involvement across as many activists as possible by organizing cooperatively, rather than rallying around a single individual (p. 14). Second, successful entrepreneurs must follow legitimate shakeout strategies, such as lobbying, media exposés, and redirection of rules and procedures, to reduce the power of incumbent entrepreneurs. Illegitimate shakeout strategies, such as the use of violence, harassment, illegal manipulation of the rules of the game, and intentional false portrayals of the opposition, will cause the entrepreneur to be unsuccessful. This occurs because the general public will disapprove of such tactics, causing the entrepreneur to lose political support (p. 18).

Crowley traces how our approach to providing for children in single-parent families has shifted from a reliance on charity groups and law enforcement to welfare reimbursement through administrative means, to tough administrative collection strategies for all families, and, currently, to helping noncustodial parents provide for their children. She argues that each of these transformations can be explained by the rise of challenger entrepreneurs that redefine the policy issue and create new policy solutions. Challenger entrepreneurs are successful because they pursue strategies of risk reduction and legitimate shakeout.

For scholars interested in the development of child-support enforcement policy over time, Crowley's analysis provides valuable insights about how different groups view the problem of child support enforcement and how these groups have changed the issue definition of child support. Her account provides an excellent summary of the ways in which the assumptions that policy actors make regarding noncustodial parents have changed over time and how the political activity of groups representing different professions, especially law enforcement and social work, shape policy responses and issue definitions relating to poverty.

That said, however, some flaws exist in Crowley's analysis that limit the usefulness of her book to those interested in the policymaking process in general, but not child support policy per se. First, her discussion of the different groups that are active in creating child support policy does not fit well with her theory that entrepreneurs drive policy change. It is unclear how the groups she discusses are entrepreneurs, rather than simply interest groups. Interest group theory and work on social movements would have been more helpful for understanding how child support has changed over time than a theory of policy entrepreneurs.

The author admits that broadening the definition of entrepreneurs to "include entire movements of like-minded individuals is not helpful because it makes the term so elastic that it is meaningless" (p. 8). However, her discussion of conservative entrepreneurs does just this. She never clearly specifies who is a "conservative entrepreneur" and who is not. Both Democrats and Republicans were part of this group. She offers very little evidence that policy actors, who made similar arguments, actually worked together.

Flaws exist in Crowley's argument that following a risk reduction and legitimate shakeout strategy explains successful entrepreneurship. First, the concept of legitimate shakeout strategy encompasses a large number of activities. Legitimate shakeout strategies include such far-ranging activities as using the gender gap (p. 129) and supporting automatic welfare programs to reduce political risks (p. 111). This concept encompasses so many activities that it provides little leverage for understanding policy change. The one case she discusses in which entrepreneurs used illegitimate shakeout strategies is male advocacy organizations in the mid-1980s. The illegitimate shakeout strategy these organizations used was making disparaging comments about women in congressional testimony (p. 179). Although the congressional testimony of male advocacy group members reflects an anti-women attitude, Crowley does not provide any evidence that it was these negative comments that turned public opinion and political elites against male advocacy groups. The lack of interest group resources in these groups is a plausible alternative explanation, which she does not address. Furthermore, it is unclear exactly how this rhetoric is anymore "illegitimate" than the rhetoric used by conservatives concerning deadbeat dads discussed in earlier chapters.

The argument that risk reduction strategies predict entrepreneurial success also does not give us much leverage in explaining policy change. Crowley argues that entrepreneurial groups were successful because they formed wide-ranging coalitions, rather than relying on the magnetism of one "star" personality. The only example given of a high-risk strategy is the efforts of the University of Wisconsin's Institute for Research on Poverty to lobby for an assured child benefit. Crowley attributes its failure to a lack of coalition building (p. 198). As she points out, however, other

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reforms advocated by the Institute were adopted, which weakens the argument that this entrepreneur group was ineffective due to a lack of coalition building.

Despite the weaknesses discussed above, *The Politics of Child Support in America* provides a very interesting and compelling discussion of how issue definition concerning the family has shifted over time and about the role that different groups have played in shifting that definition. It is well written and organized and is a good text for upper-division undergraduate and graduate courses to illustrate how policy rhetoric used by groups helps to shift our conceptions of social problems.

On Deaf Ears: The Limits of the Bully Pulpit. By George C. Edwards, III. New Haven: Yale University Press, 2003. 320p. $35.00.

— Jeffrey K. Tulis, *University of Texas at Austin*

The funeral of Ronald Reagan marked a celebration not only of the president's political accomplishments but also of the idea that the core of presidential leadership is mastery of the bully pulpit. Published shortly after Reagan was laid to rest, Bill Clinton's autobiography also reflects our modern preoccupation with rhetorical leadership. Clinton credits many of his political victories—most notably fending off an impeachment charge—to the power of rhetorical appeals. He also attributes many of his failures to an inability to communicate effectively. George Edwards thinks that Reagan, Clinton, and the conventional wisdom they exemplify are just plain wrong. In a thorough and forcefully articulated study, Edwards argues that public opinion is never altered by presidential speech. Efforts to advance a president's political agenda through rhetorical appeals over the heads of Congress to the people are futile wastes of time and energy.

Edwards is not the first scholar to warn of the pitfalls of the rhetorical presidency, but he is the most unequivocal in his critique of it. Previous accounts of the limits of rhetorical leadership prompted the question: Under what conditions are presidents likely to succeed or fail? The notion of a "limit" implies that there is some possibility of success. Even those scholars who think successful rhetorical appeal to be very unlikely still concede that there are some conditions under which popular leadership is effective and necessary. Replace the word "limits" in the subtitle of this book with the word "futility" and one gets a better sense of the central claim. Edwards thinks that attempts to explain the conditions for rhetorical success need to be replaced by a different kind of question, such as why presidents continue to adopt strategies doomed to failure.

The crux of the argument is presented in Chapters 2 and 3. The author reviews every major presidential policy initiative and every major speech between 1981 and 2003, a period that includes the Reagan and Clinton years. He makes the good methodological point that the limits of the bully pulpit for all presidents can be revealed by its failure for the most rhetorically skilled presidents. He distinguishes between transformative leaders who persuade the public to change its mind and facilitative leaders who push the public in a direction toward which it is already headed. The argument of these chapters is that there have been no instances of transformative leadership. Presidents have rarely seen their poll results improve by 6 percentage points or more, either for approval of their job overall or for specific policies. In the relatively few instances in which there were significant opinion shifts after a speech, Edwards finds these due to factors other than the speech (for example, to events themselves to which the speech is a response, like the terrorist attack on the World Trade Center.) Presidents cannot fundamentally change public opinion. They cannot "reshape the contours of the political landscape." At best, "they may endow the views of their supporters with structure and purpose and exploit opportunities in their environments to accomplish their joint goals" (p. 74). This is a provocative and important thesis that merits the attention of students of the presidency, journalists who cover the White House, and presidents themselves.

On Deaf Ears reads like a well-crafted and well-written legal brief. Almost all competing hypotheses and counterexamples are anticipated and discussed. However, like a lawyer's brief, the discussion is asymmetrical. Counterexamples are treated critically and invariably dismissed, while supportive evidence is always accepted at face value. Clinton's approval increased 10 percentage points after two speeches on Iraq in December 1998. Edwards attributes this result to the public's opposition to impeachment, rather than to the speech. Clinton's approval increased 7 points after his 1996 State of the Union address. Edwards mentions this but dismisses its significance without comment (p. 33). Support for Reagan's Strategic Defense Initiative increased significantly after his speeches, according to the Gallup data that Edwards uses for all of his cases, but he dismisses its significance because the White House's own poll showed some decline in support for SDI over a two-year period. But the White House poll shows support at a higher level than Gallup recorded in 1986, declining to a level in 1988 that is also higher than the level reported by Gallup (p. 58)! Apparent presidential successes are invariably treated as actual failures. On the other hand, apparent failures of presidents to move public opinion after a speech are never interrogated for hidden success. It is at least plausible, in some instances, that presidents halt or interrupt greater loss of public support than may have happened absent a speech.

Competing findings by other scholars are merely mentioned and dismissed, rather than refuted. For example, in a study of public opinion between 1949 and 1980, Lyn Ragsdale found that speech making positively affects the president's level of public support, and that its magnitude of influence exceeds that of positive events, military

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activity, consumer prices, and unemployment (*American Political Science Review,* December 1984). We are reminded of this study but not told why or where it falls short (p. 27).

Chapters 4 through 7 are devoted to explaining why presidents have such difficulty moving public opinion through speech. Attributes ascribed to the speaker, the message, and the audience are described to show how they serve to inhibit the president's effectiveness. Presidents continue to devote wasted energy to a rhetorical strategy because of the way they were politically schooled, through the primary-based selection system. Presidents also may "go public" for reasons other than affecting public opinion polls—to alter the national agenda, satisfy a constituency with symbolic benefits, prepare the public for a policy shift, or to alter elite debate. These objectives are offered as alternatives to the effort to move public opinion. They also raise the question of whether public opinion is always, or best, captured by public opinion polls. Taken together, might not these "alternatives" to seeking public support sometimes be components of a longer-term sophisticated strategy to shape public opinion and a political legacy?

Projections of Power: Framing News, Public Opinion, and U.S. Foreign Policy. By Robert M. Entman. Chicago: University of Chicago Press, 2003. 200p. $16.00.

— Bartholomew H. Sparrow, *The University of Texas at Austin*

Which news frames prevail in the coverage of U.S. foreign policy? Robert Entman introduces a "cascading activation" model to explain news *framing*—"the process of selecting and enhancing some aspects of a perceived reality, and enhancing the salience of an interpretation and evaluation of that reality" (p. 5). According to the cascade model, news frames flow hierarchically, from the White House to Washington elites to the media—although they may also flow directly from the administration to the media—and then to the public. Whereas the "indexing" model holds that news reflects the homogeneity of Washington elites, Entman's model has political communication going both ways: Presidential administrations and Washington elites (congressmen, ex-government officials, lobbyists, academic experts) are guided and constrained by the news (actual or anticipated), just as news organizations tailor the news according to public opinion (actual or anticipated). Because different news frames activate different thoughts and feelings at each stage of the process (p. 9), some frames succeed better than others.

Entman analyzes news frames in the *New York Times, Washington Post,* ABC News, CBS News, NBC News, *Time,* and *Newsweek.* He shows how the White House successfully promoted many of its frames during the Cold War (Korean Airlines 007, 1983; Iran Air 655, 1985), but had difficulty with ambiguous interventions (Grenada, 1983; Libya, 1986; Panama, 1989). Although the Bush administration successfully reframed the 1990–91 Iraq war, the end of the Cold War generally led to more independent coverage of foreign policy (Somalia, 1993; Haiti, 1994). The author recognizes, though, that this last finding may be the result of the Clinton presidency, since Republicans are better at projecting their frames than Democrats.

The author is surely right that the media have grown in influence over the last few decades, a phenomenon attributed to the end of the Cold War consensus (p. 5), and he valuably shows the uneven treatment that the media give to presidential administrations, political elites, and public opinion with respect to different foreign policy issues. Not only may the media ignore differences within the Beltway, but the media themselves (instead of elites) may challenge presidential news frames. Moreover, the author's study of how the media framed the nuclear freeze movement, military spending, and foreign aid reveals that news organizations, political elites, and the White House do not necessarily represent public opinion. In fact, it is extremely difficult—perhaps impossible—to disentangle public opinion from the media (pp. 130–43). Communications scholars would do well to supplement models by Robert Erickson, James Stimson, and Michael McKuen (*Macropolitics,* 2002) and others that attempt to account for policymaking without factoring in the media (pp. 144–45).

Projections of Power contains interesting findings and insights. Its multitiered, interactive analysis constitutes a welcome step forward. Nonetheless, it is hard to know what to make of the cascade model.

One problem is that author tracks news frames as they proceed down (and up) the news hierarchy, but he provides no independent measures of administrations' or elites' news frames. No baseline of elites' thoughts or discussions verifies whether or not elites are, in fact, silent (p. 73), or if journalists are simply deciding not to publicize dissenting views. Too, if governing elites and leading journalists have cozy and cooperative relationships (p. 2), then media frames might coincide or even precede White House frames. We also know that elites may want to stay off the record or use back-channel connections. Since there is no measure of elites' or presidential frames apart from what the media publicize, however, the indexing model is not disproved.

Another issue is the omission of alternative explanations. The author attributes Republicans' success in promoting new frames to their greater political skill (pp. 21, 95–96, 106, 114, 121, 152–53), but he does not discuss the GOP's "issue ownership" of foreign policy since 1980, possibly since 1968. But we might expect elites' and news organizations' treatments of the Reagan, Clinton, and Bush administrations to reflect this partisan advantage.

Or Republican success could be the result of the rise of media conglomerates (e.g., Viacom [CBS], Disney [ABC], General Electric [NBC], News Corporation [Fox], Time-Warner). Even The Washington Post Company owns other newspapers besides the *Post, Newsweek,* TV stations, and a cable company, while The New York Times Company owns

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the *Boston Globe, International Herald Tribune,* 16 other newspapers, and eight TV stations. But if journalists are strongly career oriented (pp. 13, 15, 18, 73, 154), and if media executives share Republicans' positions on taxes, antitrust, labor, welfare policy, and defense spending, then savvy journalists will reflect their bosses' priorities. The Republicans' advantage at projecting their news frames may be less a matter of skill and more because of the magnitude and repetition the media bestow on Republican news frames. Disney's initial decision not to distribute Michael Moore's *Fahrenheit 9/11* so as not to jeopardize valued tax breaks is on point.

Entman notes, too, the media's use of culturally resonant frames (pp. 14–17, 147–48, 154–55). Yet the core American values—individualism, democracy, liberty, and equality—are themselves in tension. As Paul Frymer shows elsewhere, the equality frame ("script," for Entman) resonates more than do "limited government" and "individualism" frames (Frymer, *Uneasy Alliances,* 2002). Cultural resonance is what journalists make of it. And some stories become part of popular culture no matter their dissonance (e.g., Monica Lewinsky, Abu Ghraib).

A third problem is that an examination of the text's cases casts doubt on *news frames* as the most appropriate way to understand political communication. The author assumes the underlying facts to be identical (p. 27), hence, the importance of (substantive) *frames* to define problems, specify causes, endorse remedies, and make moral judgments (pp. 23–24). But if frames are malleable and vulnerable to counterframes, then different facts could lead to different frames. Fact control becomes all-important, and presidential administrations work hard at managing information. Governments are in a position to determine the "official" facts and, thereby, effectively define political reality (Mark Fishman, *Manufacturing the News,* 1980). The text does not tell us what determines when the White House is able to define the problem and when it cannot. The facts, though, cannot be assumed.

The author accepts the conventional wisdom. In the media's version of 9/11, the cause was "ideology, envy of U.S." according to the dominant frame (pp. 24–25). The author omits the actual reason for al-Qaeda's terrorism: bringing down the Saudi regime. The remedy for the prevailing (false) news frame is *war,* though, whereas the remedy for the actual cause *might* be war (U.S. defense of Saudi Arabia) *or* reconsideration of U.S. Middle East policy. Nowhere is this discussed. The author also writes of the USSR's cold-blooded downing of Korea's KAL 007, although research points to the plane's being shot after an aerial battle between the U.S. and Soviet air forces (Michel Brun, *Incident at Sakhalin,* 1995; John Keppel, "Korean Airlines Flight 007," paper presented at Harvard University, April 1992; David E. Pearson, *KAL 007,* 1987).

Similarly, the author writes that President Carter's failure to free the Iranian hostages cost him reelection in 1980 (p. 137), but he does not discuss William Casey's and former CIA Director (and vice presidential candidate) George Bush's efforts to delay the hostages' release; their release before the November election would have been the "October Surprise" (Barbara Honneger, *October Surprise,* 1992; Robert Parry, *Trick or Treason,* 1995; Gary Sick, *October Surprise,* 1991). Neither does Entman's account of the 1990–91 Iraq war discuss the planted "incubator babies" story, the untruth that hundreds of thousands of Iraqi troops were massed on the Saudi border, or the inaccuracy of the "smart" bombs. Yet such misinformation was crucial to the war's success. In short, the text's case histories ignore the discrepancy between the conventional wisdom and known facts. Because such discrepancies have such impact on foreign policy, the author does not establish that communications scholars should focus on *news frames.*

Other observations: Entman observes that criticism often appears on newspapers' editorial pages, while the news proper often uncritically transmits the White House's news frames (pp. 78–84). He recommends the creation of "liaison editors" to overcome this disjuncture (p. 165). *But why does he think that managing editors, executive editors, and publishers are unaware of this disjuncture?* The inconsistency between the news and editorial pages (Grenada, Panama), as that between procedural and substantive criticism (1990–91 Iraq war), are managerial choices.

If measured and perceived public opinion allow for the legitimation and viability of political action (pp. 162–64), then research needs to examine polling itself (per the premise of the title, *Projections of Power*). What gets asked? When? When and where are results disseminated? How is information presented? Who sponsors the polling? Survey results on the likely war with Iraq following 9/11 were not released until the autumn of 2002, for example. Why? How were questions phrased? Who sponsored the polls? To what effect?

Last, the author refers to the Cold War consensus (pp. 95, 120, 147). But there was always disagreement on how *far* to take anticommunism, thus the dissensus over Korea, Eisenhower's "rollback" program, the use of nuclear weapons and neutron bombs, levels of defense spending, and other issues. And despite the end of the Cold War, common cultural referents remain. Only they are from World War II: "Hitler," "genocide," "Munich," "appeasement," and so forth.

Choosing Your Battles: American Civil–Military Relations and the Use of Force. By Peter D. Feaver and Christopher Gelpi. Princeton: Princeton University Press, 2004. 236p. $37.50.

— Lauren Holland, *University of Utah*

The book addresses two practical and timely research concerns: the existence and significance of differences between civilian and military opinions on the commitment and nature of military force. Quantitative (mostly survey data from the Triangle Institute for Security Studies) and qualitative (case

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studies) data are used to systematically measure opinions about military operations among the civilian and military elites and the public, and to correlate these with national security decisions from 1819 to 1992. The authors anticipate most questions that could be raised about their methods and identify caveats and research questions yet to be addressed. *Choosing Your Battles* makes both scholarly contributions and intuitive sense.

The data convincingly demonstrate that important differences exist between nonveteran civilian and military elites on the use and nature of military force. While both civilian and military elites support a commitment in cases involving imminent threats to America's vital national interests (termed "realpolitik" scenarios, like the Gulf War), they disagree on whether the deployment should be massive or limited, with nonveteran elites defending restricted intercession. In cases involving interventionist scenarios, such as international human rights or humanitarian ones (like Somalia and Bosnia), military elites (including veterans) are more inclined than nonveteran elites in Congress and the executive branch to support withholding forces. Once a commitment has been made to commit forces, however, military elites are more prone to support overwhelming force (the Powell Doctrine) than are nonveteran civilian elites who defend a more incremental and circumscribed approach.

As to the significance of these differences, the authors contend that the disparities between civilian and military elites have "profoundly shaped" the manner in which the United States has employed military force abroad since 1816 (p. 184). Drawing upon extensive historical data, they conclude that there is a significant relationship between the number of veterans (surrogate measure for military influence) serving in the executive and legislative branches and America's foreign policy commitments: As the proportion of veterans in leadership positions in the federal government increases, the propensity of the United States to initiate the use of force decreases. Moreover, once a militarized commitment is made, the presence of veterans correlates with a higher level of escalation.

The policy implications are threefold. Foremost is the authors' contention that continuing differences of opinion between military and nonveteran civilian elites will undermine civil–military cooperation and, thus, military effectiveness (or success). Of additional concern is the declining number of veterans in Congress and the executive branch. In the absence of a forceful military voice, foreign policymakers may err in decisions to commit troops in a timely fashion and/or impede success by failing to commit sufficiently massive force.

What further obscures military decision making is that foreign policymakers too often are reticent in their response to national security threats because they mistakenly believe that the public cannot tolerate fatalities, according to the authors. Drawing upon public opinion data, they debunk the casualty phobia thesis and demonstrate that the public's reaction to fatalities is not visceral but rational. The public's tolerance for casualties reflects a complex process that involves a rational calculation that factors in ideology, confidence in government, and, most importantly, the likelihood of success. People make "reasoned judgments" about the number of fatalities (costs) they are willing to tolerate, given their perceptions of the importance and likelihood of achieving the goals of a particular military mission (benefits). Moreover, the public's tolerance for casualties is similar for "high-intensity" (such as the defense of South Korea) and "lower-intensity" (such as Iraq and weapons of mass destruction) missions.

The book is carefully researched, meticulously argued, and cogently written. The authors begin each chapter with a summary of previous findings that help situate the new information being introduced. However, scholars and practitioners unfamiliar or uncomfortable with the sophisticated quantitative methods used may find it difficult to stay focused on the thematic contributions, even if these are reiterated numerous times. Since I am one of these people, I am modest in tendering these few questions about the methodology.

The first concerns the authors' testing of the casualty phobia thesis, which is central to their argument. In discrediting the thesis, the authors conclude that most (as many as 70% of) citizens are willing to accept casualties in excess of 500 people if there is the "perception" that the mission will succeed. While this finding makes eminent practical sense, using hypothetical examples to support it is risky, a caveat the authors themselves recognize.

A second concern relates to the presumed influence (as opposed to the confirmed presence) of elite veterans in policymaking situations, also central to the book's conclusions. Two thoughts come to mind. The first concerns Irving Janis's theory of Groupthink. To what extent are advisors, even veterans, so enamored of the president's position and power that they accommodate themselves accordingly? A related thought involves the issue of a critical mass. What the research findings do is confirm the value of having advisors and members of Congress with military experience contribute to decisions about war and military engagements. Regardless of whether one is a dove or hawk on military matters, having the real-life experience that veterans bring to the debate is critical. What is not clear is at what point do veterans have sufficient numerical presence to truly influence civilian leaders? Answering this question is particularly important since the gaps or differences between nonveteran civilian and military elites are "not dramatically large" (p. 6). One final suggestion for future research is to set the findings into the context of a broader theory of national security (particularly crisis) decision making.

Written for an academic audience, the book has a message that is important to legislative and executive decision makers: Be prudent in using military force abroad; consult and consider the advice of the military; employ force requisite to ensuring a successful operation; and convince the American people that the use of force is both justifiable and feasible.

The Financiers of Congressional Elections: Investors, Ideologues, and Intimates. By Peter L. Francia, John C. Green, Paul S. Herrnson, Lynda W. Powell, and Clyde Wilcox. New York: Columbia University Press, 2003. 205p. $59.50 cloth, $22.50 paper.

— Vincent G. Moscardelli, *University of Massachusetts, Amherst*

This book begins with a simple but important premise: "When political activists have distinctive views and special access to policymakers, representation and democracy may become distorted" (p. 18). Building on previous research showing that such representational "distortion" is particularly large in the case of campaign contributors, Peter Francia, John C. Green, Paul S. Herrnson, Lynda Powell, and Clyde Wilcox collect and analyze an ocean of new survey data on the characteristics, motives, participation, and activities of significant donors (those who contributed more than $200 to at least one congressional candidate in 1996). The authors reach the "troubling" conclusion that significant donors do, in fact, "have a greater voice both in elections and the policymaking process than do most other Americans. . . . The degree to which donors hold diverse views on policy and possess different motives for giving serves to mitigate but not eliminate this distortion" (p. 162).

The analysis confirms some of the current conventional wisdom and calls other elements of it into question. Not surprisingly, significant donors are wealthier and better educated than the general population. They are overwhelmingly white and male, and they tend to be members of a variety of groups and associations (although only a tiny fraction are members of labor unions). The donor pool "has a significant pro-Republican tilt" (p. 37); approximately one-half of significant donors identify with or lean toward the Republican Party. Of significant donors, 97% contributed less than $5,001 during the 1996 election cycle, and 92% gave to a total of five or fewer candidates. Using factor analysis of responses to their questionnaire, the authors find that like those who participate in other dimensions of political life, significant donors are driven by a variety of motives—material (investors), purposive (ideologues), and solidary (intimates). The authors show that candidates and professional fund-raisers are aware of this variety of goals and factor it into their solicitations. Because campaigns lean heavily on those who have given in the past, and because so many of those who have given in the past continue to give, significant donors receive multiple solicitations. This results in a higher yield for candidates, but "reinforces the static composition of the donor pool" (p. 16). Like political action committees, significant donors give disproportionately to incumbents to ensure access, and they follow up on their contributions by contacting members of Congress—81% have, at least once, contacted a member or aide in an effort to influence the legislative process (p. 136). Finally, despite their central role in the system—or perhaps because of it—significant donors expressed substantial dissatisfaction with the pre-McCain-Feingold campaign finance regime and supported many of the reforms that found their way into the Bipartisan Campaign Reform Act (BCRA) of 2002.

The Financiers of Congressional Elections has a number of strengths. Arguably, its key contribution is the identification and discussion of the diversity of donors' motives. The authors show that significant "donors are more diverse than the popular stereotype," and illustrate this state of affairs through the introduction of three "composite" donors. "Investors" like "Tom Smith . . . contribute to advance their economic self interest," while "intimates" like "Dick Jones are interested in the social side of giving." Finally, "ideologues" like "Harriett White" are more purposive in their giving—they are "interested in broader public goods, such as a clean environment" (p. 67). In addition, this rich, original data set will be a resource for students of political participation for years to come.

The book's few weaknesses are minor, but several merit mention here. First, probably because it was written by five authors over an 11-year period (p. vii), on occasion the authors seem to lose sight of their primary purpose. Most of the book reads as a traditional social scientific study, but in places it reads more like a "how to" manual for fund-raisers (see, e.g., pp. 78–83). Second, the graphical presentation of data in Chapter 3 ("Donor Motives") may overstate the degree to which material, purposive, and solidary motives are related to various social and political variables. While this presentation highlights the differences (in income, gender, partisanship, etc.) between donors with different motives, it downplays the fact that despite being driven by different motivations, significant donors are, in fact, very much alike socially, politically, and economically. Third, in Chapter 5 ("The Contribution"), the authors show that "investors emerge as most likely to indicate that business concerns, such as ensuring their business is treated fairly or that the candidate is friendly to their industry, motivate their giving" (pp. 103–4). However, the evidence for this conclusion is circular; the battery of questions from which the "investors" dimension was generated includes questions about the importance to donors that a candidate treat their business fairly or that a candidate is friendly to their industry. The authors acknowledge this possibility in an endnote, but then proceed to commit the same error in subsequent discussions of ideologues and intimates. Fourth, in their effort to present this wealth of new data and analysis, model results often receive limited, and in some instances insufficient, discussion. For example, the authors find that after controlling for other factors, women are more likely to be solicited by candidates for contributions than men. However, this interesting result receives only a brief discussion at the top of page 87, and

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it left the reviewer wishing that the authors had investigated it in greater depth.

Finally, the authors conclude that to the degree the current system is characterized by "representational distortion," it might be reduced by a system of publicly financed elections (ironically, the major reform of the system most unattractive to the significant donors they surveyed). Yet their discussion of public financing ignores many of the major critiques of such a system. For example, would a poorly funded system of publicly financed elections actually increase, not decrease, the advantages incumbents have over challengers in getting their messages out to the electorate, as some have argued?

In the end, though, the story of this book is not its minor weaknesses but its innovation and creativity, its rich empirical underpinning, and the care with which the authors conducted the research and analysis. The book increases substantially our understanding of who these significant donors are, what motivates them, and how they behave. The recent doubling of the limit on contributions by individuals to candidates from $1,000 to $2,000 per election may enhance the role of these significant contributors in future elections, making the book even more timely. By tying their empirical work directly to a rich literature on political participation, the authors expand its relevance to a wide audience of political scientists and practitioners. It should be read by any "Tom, Dick, or Harriet" who considers him- or herself a serious student of congressional elections, campaign finance, or political participation, whether inside academe or inside the Beltway.

Barbershops, Bibles, and BET: Everyday Talk and Black Political Thought. By Melissa Victoria Harris-Lacewell. Princeton: Princeton University Press, 2004. 336p. $35.00.

— Richard Iton, *Northwestern University*

The central claims of this book are that the conversations ordinary black folk have with each other matter politically, and that there is a diverse range of ideological perspectives endorsed by African Americans. The author, Melissa Harris-Lacewell, supports these contentions by means of survey research, experimental studies, and ethnographic research. This comprehensive approach represents a significant accomplishment and, in combination with the theoretical contributions of the text, makes the book an invaluable addition to the African American politics canon.

In the first chapter, the author, using the work of James Scott, Robin Kelley, and Jane Mansbridge, among others, discusses the significance of black "everyday talk" and the relevant spaces—the church, the barbershop, the beauty salon, and black-controlled media—where these ideas are expressed and developed. Harris-Lacewell's ambition here is to identify the central concerns of black political thought; she also identifies nationalism, conservatism, feminism, and liberal integrationism as the four major ideological systems that blacks use to fulfill these tasks. In the spirit of Michael Dawson's groundbreaking *Black Visions* (2001), she also argues that it is important that ideological discourse be "restored" to its proper place in the study of African American politics, in particular, and American politics, in general.

The interior chapters are devoted to distinguishing these ideologies in action. The use of multiple approaches allows Harris-Lacewell to straighten out the causal arrows. Is everyday talk simply a reflection of elite discourse? Does exposure to such discourse affect the ways in which individuals orient themselves ideologically, or do individuals select discourse because of previous ideological commitments? She answers some of these questions by means of an ethnographic study set in a Baptist church in North Carolina. While acknowledging the limitations of her data (for example, the author notes that she had no "way of knowing how many men and women heard [the church's leader] speak only once and refused to return because of their opposition to his political views" [p. 77]), Harris-Lacewell suggests that, indeed, everyday talk does affect the political orientation of individuals.

In the third chapter, the author uses survey data to identify the stability of the ideological perspectives she is employing in order to organize the study and the correlation between exposure to various forms of black discourse and particular orientations. Working with the 1993–94 National Black Politics Survey (NBPS), she identifies some significant differences among the nationalist, conservative, and feminist clusters and, relevant to her broader concerns, suggests that engagement in everyday black talk (as registered by exposure to church sermons and black media, music, and literature) tends to correlate with certain ideological perspectives (e.g., nationalism and feminism, rather than conservatism). Another chapter involves two experimental studies of black college students—the first conducted in Durham, North Carolina, in 1998 and the second in Chicago in 2001—on the basis of which the author is able to make some claims regarding causality and suggest that "group interaction [in and of itself] has a discernible impact on political attitudes" (p. 152).

The fifth chapter, cowritten with Quincy T. Mills, is the most engaging in the book. It involves a second ethnographic study, this time in a South Side Chicago barbershop. Here, the study seeks to establish the contours of black everyday talk in a space reserved primarily for African American males. The marginalization of black lesbians and gays is highlighted as the boundaries of black community (to borrow from Cathy Cohen's crucial text *The Boundaries of Blackness* [1999]) are probed and revealed. In this chapter, the authors also capture the performative dimensions of black discourse: the things that are said "behind closed doors" that would not be repeated elsewhere (a tension the authors recognize as also relevant to the controversy associated with the release of the 2002 movie *Barbershop*). The chapter is an ethnographic gem.

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There are some areas in which the text could be improved. The classification of the radio talk show host Tom Joyner as a nationalist will, as the author acknowledges, raise eyebrows, although she does not provide a convincing explanation of why she chooses to stick with this classification (especially given the availability of more logical examples), and indeed, she might have troubled the nationalist/integrationist dichotomy more profoundly. With regard to Harris-Lacewell's attempts to draw conclusions concerning the significance of rap music on the basis of questions about whether one listens to the music, a more useful question, for the purposes of analysis, might be to ask to which kinds of rap—that is, to which artists—do respondents listen. A related concern has to do with regionalism. As hip hop has always had different regional variations, Harris-Lacewell in her analysis of the survey and experimental data could have devoted more attention to the ways in which regional factors matter. For instance, how safely can one compare the experimental results based on focus groups composed of students in North Carolina with those involving students in Illinois? The author might have also tried to test for the duration of the impact of "everyday talk" in these experimental settings with follow-up surveys (or at least considered that their impact might be ephemeral).

There are instances where the narrative could be more probing (e.g., with regard to the parishioners' apparent silence around issues of sexuality in the second chapter; on the topic of relations between Caribbean Americans and African Americans; and the striking reticence regarding class issues in the barbershop chapter). The author also consistently fails to account for the impact of market processes, and white-owned media spaces, upon black everyday talk. Joyner's "nationalist" discourse is broadcast by ABC and sponsored by Walmart; the debate about the movie *Barbershop,* which Harris-Lacewell recounts effectively and vividly, takes place in, among other venues, the *Los Angeles Times, Chicago Sun-Times,* and *St. Petersburg Times.* What does it mean that black everyday talk often takes place in spaces owned by individuals who are not black (e.g., the rap music industry, the BET television network)?

Ultimately, these are mostly minor quibbles. *Barbershops, Bibles, and BET* is well written and original in its conception, and it represents a remarkable achievement. It will undoubtedly generate more work in the future that probes the sources and character of black political thought, as well as the ability of ordinary black folk to think for themselves.

Polling to Govern: Public Opinion and Presidential Leadership. By Diane J. Heith. Stanford: Stanford University Press, 2003. 216p. $50.00 cloth, $19.95 paper.

— David J. Lanoue, *University of Alabama*

Those who are troubled by the ubiquity of public opinion polls, particularly members of the press and public, often complain that polling has turned political leaders into followers, afraid to champion policies unless they are already broadly popular. Bill Clinton was often accused of conducting a poll-driven presidency, particularly after his party lost control of Congress. Similarly, George W. Bush's key political adviser, Karl Rove, is seen by many as the architect of a strategy in which nearly all of his boss's actions are taken with at least one eye on reelection.

These claims are, of course, exaggerated. President Clinton made a number of choices that were, if not clearly unpopular, at least not obviously "safe." As for President Bush, the war with Iraq, regardless of one's feelings about its necessity, was unquestionably a triumph of opinion leadership. There were no polls showing serious public demand for toppling Saddam Hussein until the administration forced the issue onto the national agenda. Anecdotally, there is plenty of evidence that the claim that polling has turned the presidency into a "permanent campaign" is exaggerated at best.

Nevertheless, it is important to demonstrate such things empirically, and this is what Diane J. Heith attempts to do in her book. Heith's major contribution to this debate is her use of archival data from five administrations (Nixon through Bush I), as well as public documents and memoirs from those five and the Clinton presidency. These archival data enable the author to document every instance in which public opinion information shows up in "formal and informal memoranda, handwritten notes, pollster reports, and other written documentation" (p. 9). This, in turn, allows her to draw conclusions about how polling data are used, by whom, and to what effect. The result is an interesting look into the ways in which six presidents gathered and evaluated information about public opinion.

In her first chapter, Heith discusses the relationship between polling and theories of presidential leadership and also introduces her data set. In particular, she discusses the idea of the permanent campaign and distinguishes it from traditional notions of governing that involve consensus building and compromise. She lays out her argument that while "[t]he behavior and values of a campaign have indeed penetrated the White House, [they] have not overwhelmed or subsumed the governing process" (p. 8).

The second, third, and fourth chapters are devoted, respectively, to discussions of who is most likely to seek and use polling information, what sorts of data receive the greatest attention, and how presidents use polls to identify supporters and adversaries within the electorate. Heith argues that the White House generally attributes the highest value to information that can help advisors assess voters' reactions to various policy alternatives and sell preferred policies to the public. She notes that "[a]pproval ratings," despite their prominence in the news media, "were not the presidential public opinion data of choice" (p. 57). (Of course, it is also possible that with presidential popularity figures so widely known and broadly available, it was simply unnecessary for the White House to generate them or discuss them in much detail.)

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In her final chapters, Heith uses various case studies to demonstrate her point that while all modern presidents make extensive use of polls, they are "not abandoning traditional governing behavior in legislative battles in favor of a campaign approach" (p. 121). One exception to this rule, however, comes in those rare cases when presidents seek to avoid impeachment and removal from office. In such cases, and especially that of Bill Clinton, White House polling strategy becomes more consistent with the expectations of the permanent campaign model. As Heith correctly points out, surviving scandal has little in common with developing and passing a legislative program. During times of (possible) impeachment, presidents are, in effect, campaigning to remain in office: Single-minded attention is devoted to "winning," and it is generally recognized that maintaining public support is essential to prevailing in the upcoming battle with Congress.

All in all, this book represents a worthy effort to bring some new evidence to bear on important questions of presidential governance. There are, however, a couple of potential quibbles that could be raised. First, the author's efforts to debunk the notion of the permanent campaign result in something of a straw-man argument. I am not aware of many political scientists (as opposed to pundits or voters) who actually believe that presidents have abdicated their duty to lead and to bargain with Congress in favor of slavish devotion to public opinion polls. It hardly comes as a surprise, therefore, when she concludes that most administrations, while they may carefully scrutinize polling data, generally continue to govern in the same ways that their predecessors did.

In addition, a significant amount of data is presented, and sometimes the forest seems lost in the author's effort to detail the trees. It is useful (though not shocking) to learn that different presidents handle public opinion data in different ways, but Heith might have done more to tie these findings together. I do not wish to exaggerate this point; there is some very good summary discussion. But at times, it was difficult to see how the author's data supported her broader argument.

Despite these concerns, *Polling to Govern* is a very useful addition to the literature. It should be read by students of the presidency and scholars concerned with American public opinion. It is written clearly and accessibly and should be of value to undergraduates, as well as graduate students and faculty.

Politics, Policy, and Organization: Frontiers in the Scientific Study of Bureaucracy. Edited by George A. Krause and Kenneth J. Meier. Ann Arbor: University of Michigan Press, 2003. 352p. $65.

— Gayle Avant, *Baylor University*

This text consists of papers originally presented at the Fifth Public Management Conference meeting at Texas A&M University in December, 1999. The editors in the introductory chapter aptly define the challenges facing contributors: "We know little about how bureaucratic (a) agencies make decisions . . . (b) structures affect responsiveness and performance . . . (c) agencies resolve goal conflict on a day-to-day basis, (d) external performance criteria affect bureaucracies and (e) organizations socialize their members" (p. 15).

The editors assert that embracing these formidable challenges collectively constitutes a "new organizations" approach to the study of public administration. This approach is "bureaucratic" and can be understood as an extension of the "new institutionalism," which focused more on analysis of impacts of democratic institutions (the presidency, the Congress, and the courts) than on bureaucratic behavior and decision making: "Bureaucratic agencies . . . contain their own incentives, procedures and preferences for administering public policy" (p. 306).

As might be expected, the papers vary in focus and content. Any academic interested in the nexus between government and bureaucracy will find several of them interesting. The editors ordered papers under "theory," "methodological technology," and "empirical studies" labels.

In the theory section, Daniel Carpenter's analysis of the optimal stopping point in the Food and Drug Administration's drug approval regimen is well articulated and sophisticated. To this reviewer, George Krause's treatment of agency demands for discretion treats these demands as a unity, failing to recognize that each bureaucratic level seeks discretion from other levels. One of Carpenter's assumptions, that increasing task complexity is linked to decreasing demands for agency discretion, is counterintuitive. As Thomas Hammond states in his piece on "veto points," "bureaucrats may know more than elected officials about what needs to be done and how to do it" (p. 74)—which supports a claim that greater task complexity leads to greater demands for agency discretion. The final paper in the theory section is David Spence's analysis of agency discretion from the public choice perspective. Public choice hewed to a simple principal-agent theory. The people and their elected representatives (principals) instructed agents (agencies), so public choice theorists initially had little sympathy for notions of agency discretion. Spence clarifies well those occasions when people might prefer agencies, rather than their elected officials, to solve public problems.

Under the methodological technology label, John Brehm et al. provide an enlivening break from high-level theory by reporting analysis of the 1977 Police Services Study using the Dirichlet distribution. The authors report that police spend more time on official duties when under direct observation by supervisors and fellow officers. Andrew Whitford develops a model of a "baseline agency" and poses three "levels of redirection: comparison, punishment and imitation" (p. 161). This piece is abstract and could better be placed in the theory section.

Several of the five papers in the empirical section merit at least a brief mention. Steven Balla and John Wright's study of federal-level consensual rule making asks if this procedure more quickly results in a rule, as compared with the APA's usual review and comment procedure. After examining 170 rules issued by "nearly forty agencies," they conclude that consensual rule making does not yield quicker results. A careful reading of this paper will give readers a better understanding of the challenges facing scholars researching federal procedures.

Lael Keiser's explanation of the Supplemental Security Income (SSI) program is the clearest read by this reviewer. Even with refined formulation of the independent variable, only meager statistical support is given for logical and anecdotally supported relationships explaining differences in SSI approval rates by state bureaucracies.

Kevin Corder's study of the numerous federal credit programs finds that some are operated by cabinet departments, others by independent agencies, and others by "government sponsored enterprises"—and only the latter entities were designed to be substantially removed from partisan political influence. Generally, government enterprises can borrow from the private sector, while other entities must rely on appropriations for funding. Federal credit entities use their funds for direct lending and for loan guarantees. The Congress treats current loan guarantee costs as nondiscretionary and currently initiated guarantees as spending deferred to a future date. After careful formulation of the independent variable, Corder's conclusions are limited. He concludes that lending by all federal credit entities is responsive to election results and congressionally mandated structural changes.

Kevin Smith, analyzing data from a 1998 survey of teachers of kindergarten through grade 12 finds that when compared to teachers in public schools, private school teachers place a much higher priority on promoting moral/religious values and only slightly more emphasis on encouraging academic excellence.

Certainly the call for more rigor and a more empirical focus in public administration research is laudable. The editors' excellent overview of recent research leads this reviewer to conclude that their distinction between the "new organizations" approach and earlier approaches to public administration research is overdrawn and a bit of a straw man. Perhaps the fault lies more in the "unit of analysis problem" or a type of myopia within political science.

Empirical research in any discipline requires a clearly defined unit of analysis, linked to innovations in some units but not in otherwise comparable units. Decisions are made by individuals and can be aggregated, as in Supreme Court rulings and elections. By definition, a bureaucracy has multiple members and levels of authority. It is with good reason that bureaucratic decision making is sometimes referred to as a "black box."

The editors of and contributors to *Politics, Policy, and Organization* are exclusively focused on American phenomena. No attention is given to earlier attempts to better understand bureaucentric decision making in a comparative context. As part of the Alliance for Progress, the U.S. Agency for International Development formented preparation of national development plans in most Latin American countries. Albert Waterston commendably compared these efforts. Fred Riggs led a Ford Foundation effort in the 1970s to develop a subfield of comparative administration.

Good bureaucentric research may also exist in other fields. This reviewer suspects that much research has been done by educationists comparing responses of school districts to state and federal mandates. Health care academicians may have compared the responses of hospitals and health maintenance organizations to federal requirements. Likewise, law enforcement research may have focused on varieties of local responses to the federal Law Enforcement Assistant Act and other mandates.

Compared to others interested in bureaucracy and governance, the contributors to this volume and to the new organizations approach are empirically oriented and methodologically sophisticated political scientists. Collectively, these studies support a claim that the move from case studies to middle-range theory in the study of agency structure on output will be challenging indeed.

The Front-Loading Problem in Presidential Nominations. By William G. Mayer and Andrew E. Busch. Washington, DC: Brookings Institution, 2004. 226p. $49.95 cloth, $22.95 paper.

— Stephen J. Wayne, *Georgetown University*

The authors are right, but . . . Front-loading has skewed the nomination process. It has given states holding early caucuses and primaries an advantage. They have more influence on the final outcome, more attention from the candidates and the news media, more economic benefit from the spending of the campaigns and the organizations that cover them, and more citizen involvement and higher voting turnout.

Since 1988, the front-loading trend has accelerated the beginning and condensed the period during which the nomination is effectively decided. William Mayer and Andrew Busch document and explain this trend and its impact. Their documentation is largely descriptive and their explanation both descriptive and analytic. The authors do a thorough job.

When they get to the consequences, however, Mayer and Busch impose their own value judgments on their empirical-based analysis. Front-loading is bad for the electorate, the parties, certain candidates, and the democratic process. It is bad for the electorate because "it greatly accelerates the voters' decision process and thus makes the whole system less deliberative, less rational, less flexible, and more chaotic" (p. 56).

Because voters must reach judgments more quickly on the basis of limited information, the authors conclude that the process is less deliberative. Less deliberative than what—the postreform pre-1988 nomination period? an ideal democratic process? The authors do not say.

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Mayer and Busch do present poll data to show that the electorate initially has little information about the candidates and needs time to become informed about them. Of course, they are correct. As the Annenberg studies of the 2000 presidential nomination and the polls of the 2004 process reveal, much of the electorate does not tune in until the year of the election, after extensive media coverage and candidate advertising, and usually not until the campaign confronts them directly. As a consequence, many people lack detailed knowledge, and if they participate in primaries, may be forced to make superficial judgments. The authors contend that an elongated process would provide more time to acquire more information, a conclusion that Thomas Patterson disputes in his book *The Vanishing Voter* (2002). Whether more time would lead to greater rationality, more flexibility, and less chaos, terms which the authors do not define, much less operationalize, is also debatable.

Another of the authors' complaints is that "front-loading works to the advantage of the front-runner" (65). It does, but is that consequence detrimental to the party and its nominee's chances for victory in the general election? Does it adversely affect the party and its nominee's ability to govern? Well-known candidates who tend to be the front-runners tend to be more experienced in government, better acquainted with national and state party and elected leaders, and more likely to have an established public record that voters can assess.

Mayer and Busch properly raise the equity issue with respect to lesser-known candidates; they need more time to raise money, build an organization, and decide on their issue positions and priorities. They also need to demonstrate their qualifications for president. But as the condensed 2004 Democratic nomination process illustrates, Vermont Governor Howard Dean was still able to overcome many of these resource challenges. That Dean did not win the Democratic nomination was not a consequence of front-loading.

A third outcome of the front-loading process, which the authors consider detrimental, is the interregnum, the period between the date on which a candidate has effectively wrapped up the nomination and the date on which the convention officially anoints its standard-bearers. Mayer and Busch point out that the successful candidate needs to extend the campaign to the convention but may not have the financial capability of doing so. Nor can the national party help out as it did in 1996 and 2000 with soft-money-generated generic advertising. The Bipartisan Campaign Reform Act (2002) prohibits national parties from soliciting contributions that exceed the federal limits, although a loophole for nonprofit, issue-oriented advocacy groups, the so-called 527s, was permitted in 2004 because the Federal Election Commission refused to rule on the issue before the election.

The interregnum, however, serves important functions for a lesser-known candidate and/or a candidate who is challenging an incumbent. It provides time for the candidate to repair an image that has been damaged by opponents in the nomination process, overcome a divisive battle within the party and help to unify it, energize its supporters, and transition from a partisan appeal to a more generic issue-oriented message. John Kerry's campaign during the 2004 interregnum illustrates the benefits of this period for a nonincumbent challenger. Kerry used the interregnum to help offset his opponent's financial advantage, to compete against the sitting president's "bully pulpit," and to defend himself from his opponent's negative advertising. However, the Kerry experience also suggests that unless the campaign finance law is changed to permit fund-raising by a candidate who accepts matching funds, no serious candidate will participate in public funding at the nomination stage of the process.

If front-loading is so bad for many candidates, the party, and the democratic process, then why not change it? As the authors, who would like to change it, point out, it is not that easy. The beneficiaries—the states that hold their primaries early, the front-runner, and perhaps even some national party leaders—would probably oppose change. Mayer and Busch critique the comprehensive plans that have been advanced to alter the process. They contend that these proposals would not alleviate the problem and have negative consequences of their own. The authors, however, see more hope with incremental change. They favor increasing contribution limits and matching-fund ratios; providing incentives for states to hold their primaries later; easing ballot access and giving candidates more flexibility in slating delegates; removing fund-raising limits after the nominee has been effectively determined for a candidate who accepts public funds; and moving the beginning of the window period, the date at which caucuses and primaries can be held, to later in the year (p. 150).

There is much to recommend in these proposals and in *The Front-Loading Problem in Presidential Nominations* itself. The book is well written and accessible to undergraduate students. The authors support their arguments with both qualitative and quantitative data. Although the topic is relatively narrow for an elections course, the subject is treated comprehensively. For those bothered by front-loading or simply interested in exploring the subject, this is the book to read. Although it was written before the 2004 nominations, most of the authors' generalizations and many of their conclusions are applicable to that process as well.

To Form a More Perfect Union: A New Economic Interpretation of the United States Constitution.
By Robert A. McGuire. New York: Oxford University Press, 2003. 395p. $24.95.

— Jeffrey D. Grynaviski, *University of Chicago*

Charles Beard's (1913) *An Economic Interpretation of the Constitution of the United States* has long been a focal point for scholarly debate on the motivations of the American

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Founders. Beard argued that each of the Founders could be classified as merchants with "personalty" interests or as farmers with "realty" interests and that political conflict during the Founding could be interpreted through the prism of conflict between these two classes. This work came to be greeted with growing disdain by scholars who found his "Marxist" interpretation of the Founding inadequate, but given the rise of rational choice theory as an important analytic tool for political scientists, another look at Beard's theory through modern conceptual lenses is long overdue.

To Form a More Perfect Union breathes new life into accounts of the Founding premised on the assumption that the Framers were rational, self-interested actors whose preferences over constitutional provisions were motivated by personal economic interests. Robert McGuire decomposes Beard's classification of delegates as personalty and realty agents into more specific categories to better understand which economic motivations were relevant to behavior and which were not. Specifically, he argues that delegates with merchant and commercial interests, with holdings of public or private securities, or with western landholdings were more likely, *ceteris paribus,* to support a strong national government and the Constitution. Alternatively, delegates with agricultural interests, with debt, or with slaveholdings were less likely, *ceteris paribus,* to support the Constitution.

For quantitatively oriented political scientists, McGuire's approach to testing these competing theories will be refreshing. In contrast to previous research, his work is not based on case studies of one or a few Founders or simple cross-tabulations of votes with delegate circumstances. Instead, choice models from applied econometrics are used to study the voting behavior of delegates to the Constitutional Convention in Philadelphia and to the various state ratifying conventions. The value of the econometric choice model is that it controls for the possibly confounding effects of delegate ideology and constituents' interests, allowing the author to identify the partial effects of delegates' economic interests.

Data for McGuire's analyses are drawn from an impressive variety of sources documented throughout the book and in a detailed appendix. A close examination of the debates at the Constitutional Convention is used to identify 16 crucial issues put to a recorded vote. Important issues included prohibitions on export tariffs (passed) and an absolute federal veto over state laws (failed). Because votes at the convention were based on state delegations, rather than delegate counts, substantial detective work goes into teasing out the choices of individual delegates. Roll call data are also collected for state ratifying conventions where the Constitution was put to a final "yes" or "no" vote.

For each delegate at the Constitutional Convention and approximately 75% of the more than 1,600 participants at state ratifying conventions, measures of economic interests are identified in addition to control variables for delegate ideology and constituents' economic and ideological interests. Needless to say, these data are extraordinary. However, I must admit that I am somewhat unsatisfied with the interpretation of many theoretical concepts in the model. For example, such variables as ownership of public securities may provide evidence of delegates' ideological commitments through investments important to the development of a strong national state, rather than purely economic considerations. To the author's credit, he demonstrates an awareness of these limitations, but these alternative interpretations raise concerns about the validity of the statistical inferences.

Having identified the key variables of interest, McGuire presents the results of an extensive series of analyses of voting behavior at the Constitutional Convention and the state ratifying conventions. By my count, the book presents coefficients for more than 250 logistic regressions, and while reading, I was overwhelmed by the amount of information.

For the Constitutional Convention, McGuire finds in support of his core theoretical claim that at least one personal economic variable is statistically significant for all 16 votes; however, there is no personal economic variable consistently significant across issues, and on many votes, the p-value for the lone significant economic variable is only $p < .20$. While the author is correct that it may be appropriate to set the bar a bit lower, given the small case count (n = 53), with 20 predictors the models seem badly overparameterized, and I am not confident that the asymptotic properties necessary for reliable standard error estimates are satisfied. In contrast, variables related to constituency characteristics seem to have been more influential across a range of votes than the personal economic variables. For example (despite my reservations), it appears that veteran officers from the American Revolution were much stronger supporters of provisions for a strong national government than were other delegates.

Stronger evidence for an economic interpretation of the Founding is found in the state ratifying conventions, especially in models where votes across states are pooled together. In this case, McGuire finds that delegates who were merchants, western landowners, private creditors, and farmers were significantly more supportive of ratification, holding constant personal ideology and constituent interests; and that opponents of the Constitution were debtors and slave owners. McGuire is careful to qualify his claims because his analyses of individual ratifying conventions suggest that there was some variation in behavior across states. Nevertheless, one cannot help but be impressed by the basic stability in the coefficients for the personal economic interest variables nationally, providing good evidence that the political cleavages were grounded on common economic interests nationwide.

The book's analysis of the economic interests of the individuals who drafted and ratified the Constitution makes an important contribution to the study of the American Founding. Future research on the motivations of the delegates in Philadelphia and the state ratifying conventions will certainly have to wrestle with McGuire's conclusions. Taking

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seriously these motivations is crucial for understanding the particular institutional bargains struck in the United States, and more generally, for understanding the conditions under which rational agents cede authority to national governments during periods of state formation.

Pluralism at Yale: The Culture of Political Science in America. By Richard M. Merelman. Madison: University of Wisconsin Press, 2003. 325p. $55.00 cloth, $26.95 paper.

— Rogers M. Smith, *University of Pennsylvania*

Because Richard Merelman was a Ph.D. student at Yale during the period that he writes about in this book, his project instantly arouses the suspicion, as he knows, that it stems more from personal than scholarly preoccupations. As a result, *Pluralism at Yale* is, if anything, excessively scholarly in its architecture and arguments. But regardless of where the motivation for his work ultimately comes from, Merelman's book exemplifies an unusual but eminently defensible, potentially illuminating mode of serious political science research. Ironically, its limitations come not from the fact that it centers on a remarkably in-depth study of a single political science department but from the ways it does not take that study far enough.

As his title indicates, Merelman's subject is pluralism, examined as what he calls a "legitimating discourse." He defines "pluralism" as comprised of four premises. First, there exist "multiple, competing centers of power" (the "pluralist" premise). Second, policymakers react to pressures and problems much more than they initiate changes (the "reactive" premise). Third, political leaders are "tolerant coalition builders" (the "elite tolerance" premise). And fourth, these features produce neither deadlock nor radical upheavals, but rather "graduate, moderate," incremental political reform (p. 18).

One could quarrel with this definition, particularly the insistence on the "reactive" and "tolerant" elements. Logically, pluralism might well be confined to the first, "pluralist" proposition. Merelman's interest, however, is not in what the idea of political pluralism logically requires. It is in how a certain community of self-defined pluralists talked, thought, wrote, and acted on pluralist precepts; and he may well be right that at Yale, this is how many talked, wrote, and perhaps thought and tried to act in the decade and a half that he examines.

Merelman's notion of a legitimating discourse is slightly more problematic, but only because he strives so hard to claim that it is something rarely studied. He defines such a discourse as a body of ideas, images, or practices that portrays a political regime as working the way its power holders say that it should work, thereby supporting their power (p. 9). He insists that this is different from kindred notions like "dominant ideology." But he assigns to theories of dominant ideology a number of premises—such as "power holders are irrational," ideologies always "mystify reality," power holders completely "control" intellectuals, and more—that many analysts of dominant ideologies would not recognize (p. 10). The distinctiveness of his approach does not really arise from his concept of a legitimating discourse. It lies in his concern to examine pluralism not just as a set of ideas but as a key element in the lived experience of an actual human association, the Yale Political Science Department as it was from 1955 to 1970.

Candor requires me to note that I taught at Yale from 1980 to 2001, well after the more seminal era that Merelman examines, but with many of the figures he discusses still present as longtime colleagues and friends. But so far as I can judge, that experience simply helps me to know that Merelman, who examined departmental records and interviewed 129 Yale political scientists and former graduate students, largely gets his facts right. I have heard many of the stories he relates told somewhat differently, but given the inevitable varieties in perspectives and fallibilities in memories, all he recounts sounds plausible enough.

Still, why look so closely at one university academic department, and why Yale? In those years, initially under James Fesler's leadership, the Yale department rose from lowly depths to be widely rated as the top department in the nation. It did so largely through becoming known as the home of pluralist theoretical and empirical accounts of American politics, as embodied in the now-classic works of Robert Dahl, Charles (Ed) Lindblom, Robert Lane, Herbert Kaufman, and more, and in the books and articles written by the students they taught, Aaron Wildavsky, Nelson Polsby, Raymond Wolfinger, Bruce Russett, and others. In this period, Merelman suggests in a late chapter, academic textbooks and popular magazines like *Time* often portrayed American politics in pluralist terms; and the Yale pluralists articulated this legitimating strain in American political discourse at least as well and as influentially as any group one could name. And Merelman wants to look not just at texts but at a group or community espousing pluralism, because he believes that one can learn more about what pluralism or any legitimating discourse really means by seeing people trying to live according to that discourse. Still more boldly, he believes that the evolving content and the eventual partial weakening and discrediting of pluralism at Yale reflected "departmental dynamics," what happened "*within* Yale," as much as they did developments in the external world "outside" the windows of Yale's classrooms (pp. 146-47).

This is an intriguing hypothesis. It is indeed possible that one can fully understand a particular legitimating "discourse" or dominant ideology (sorry, Professor Merelman) only by examining what happens when people who profess it try, at least to some extent, to act according to it on a day-to-day basis. Methodologically, that possibility can only be explored through close study of a particular community. And again, if one has to choose a particular community to study pluralism as a legitimating discourse, there are few

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more obvious candidates than Yale political science in its original glory days.

So unlike, I suspect, many readers, I believe Merelman's project is fully justified intellectually. Yet I cannot unequivocally applaud it because Merelman's implementation of this intriguing enterprise is surprisingly narrow. His focus is almost exclusively on how the Yale pluralist professors dealt with one another in collegial relationships, and how they dealt with their graduate students. (And yes, it includes the famous story of how Karl Deutsch left the department in a huff over his depiction in *Tell the Time to None,* a fictionalized account of departmental life penned by Robert Lane's novelist wife, Helen). The basic account Merelman gives is that in their early excitement over emerging pluralist ideas, Yale professors lived (or, as he prefers, "performed") pluralism in their teaching and collegial relationships rather successfully. But as time went by, inspiration faded and privileges grew. Many then failed to deal with the jarring new problems of the late 1960s, and the expectations of their junior colleagues and students, in true "pluralist" fashion. So pluralism ceased to "blossom" in their texts and in their lives, and it was by the early 1970s "dethroned at Yale" (pp. 99–102).

Merelman then goes on to argue that some Yale students nonetheless continued to affirm and sustain pluralist views in various forms, generally using "scientific" narratives, while others either never accepted them or came to reject them, often using "fictionlike" narratives (pp. 237–38). He casts this argument partly as refutation of those who believe that pluralism is simply "dead," citing a 1997 essay I wrote as implying this claim (p. 186). For the record, then and now I agree with Merelman that pluralism as a political science theme "declined after 1970" (p. 103), without ever dying—sitting in New Haven when I wrote that essay, I did not suffer from that delusion!

I do not, however, think Merelman is right to say that defenders of pluralism write "scientific," while critics write "fictionlike" narratives. For my money, James Scott's observations on peasants in Malaysia—or Merelman's own writings on political culture—are not less scientific than Arend Lijphart's consociational case studies or even Gary Jacobson's statistical studies of the role of money in campaigns. More generally, Merelman's quick closing sketches of alternatives to pluralism and his assessments of whether they or any form of pluralism can today serve as a legitimating discourse (answer: no) are too underdeveloped to be fully persuasive, or to provide a fitting conclusion to this interesting study.

Nonetheless, its largest shortcomings come earlier, as Merelman strives to document the growing failure of leading Yale faculty members to "perform" pluralism effectively. He shows how as the department grew, many junior faculty members came to experience departmental governance as not genuine democratic pluralism but as hierarchical, especially when the department had overhired in the late 1960s and had to cut back. But is it really *so* shocking that an academic department is more an aristocratic (or oligarchic) republic than a pluralist democracy? Merelman also shows that some graduate students with more conservative outlooks, and some with more radical views, felt disaffected and sometimes mistreated, though others with similar views did not. Those experiences do show some of the limits of the faculty in living up to pluralist ideals, but again, not pervasive hypocrisy. The author is strongest on showing how many, probably most, female graduate students in those years felt disrespected in the department, including some instances of sexual misconduct. Those patterns still echoed during my days there. But I should note that it is also true, however unexpected, that a not especially deferential feminist, Catherine MacKinnon, speaks positively of her time there (pp. 135–38, 168).

Yet after discussing these very pertinent issues, Merelman strangely fails to raise some of the most obvious limits to Yale pluralism in those years. Although he recounts how the faculty squeezed out cantankerous conservative theorist and *National Review* cofounder Willmoore Kendall in the mid-1950s, he has only one brief reference to Joseph Hamburger, a successor conservative theorist who regularly criticized his liberal pluralist colleagues as ideologically intolerant from the 1960s until his death in the 1990s. And although Merelman recognizes the role of pluralism as anticommunist ideology, he does not discuss departmental controversies over whether Marxists like Herbert Aptheker could be approved to teach even one seminar, much less whether any Marxists could be appointed to the faculty.

Most glaringly, Merelman is silent on the obvious but hardly trivial fact that in this pluralist heyday, although the graduate students were a slightly more diverse lot, the Yale Political Science Department had no female faculty members, no African American faculty members, no Latinos, only an unbroken sea of white men, mostly of northern European ancestry, with an overrepresentation of admittedly brilliant Scandinavians. As a performance of pluralism, this was not. And although there was no conscious bigotry (I did once hear a nervous senior faculty member accidentally refer to the department's first African American assistant professor, Philip White, as Philip Black), I can attest that despite genuine pluralist ideals, over the years change has not come more easily at Yale than in most of America.

Were there viable female and/or African American or Latino or Asian or Marxist or conservative candidates for faculty appointments from 1955 to 1970 who the pluralists considered, debated, and turned down? Merelman does not say, though at least some of his interviewees could have told him. This failure to explore the makeup of the faculty itself and its hiring processes greatly limits the persuasiveness of his central argument.

Again, he wants to suggest that as important as events in the external world—the civil rights movement, Vietnam,

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the War on Poverty, eventually the women's movement—were in challenging Yale pluralism, failures of internal performance did as much or even more to put pluralism on the path to decline in New Haven as the 1960s wore on. He seems to feel impelled to make this argument in order to vindicate his choice to focus on this relatively small sample of American political life.

Yet he might well have been able not only to sustain but to strengthen his case for his approach if he had cast his net a bit more widely; for I suspect the two factors he wants to compare were more tightly linked than Merelman conveys. It is hard to imagine how the visible embodiment of a traditional white male, largely Protestant hegemony within the department, during times when precisely those hierarchies were being assaulted outside it, with Yale faculty support, could have failed to foster growing consciousness of the limits of pluralism on the part of all involved, faculty and students alike. But was that indeed the case, and if so, how was it experienced, and how did it play out? Those are fascinating and possibly quite revealing questions that go unexplored here.

Hence, the ironic result. This valuable if idiosyncratic book succeeds in many ways in vindicating its startling decision to put one single department under a social scientific microscope. It falters largely because in the end, it does not look either closely enough or broadly enough to see all the glaring discrepancies in pluralism at Yale and in America that were then, and in many ways still are now, all too politically significant.

Virtual Inequality: Beyond the Digital Divide. By Karen Mossberger, Caroline J. Tolbert, and Mary Stansbury. Washington, DC: Georgetown University Press, 2003. 208p. $19.95.

— Charles C. Hinnant, *University of Georgia*

Whether or not certain demographic groups within society have sufficient access to information and communication technology (ICT) has become a major subject of debate. So-called digital divides have been examined in regard to many demographic categories, such as race, gender, socioeconomic status, and even the level of urbanization. Most empirical efforts to examine issues of technological inequities within the United States have been primarily descriptive in nature and theoretically limited in scope. *Virtual Inequality: Beyond the Digital Divide* adds a valuable contribution to the debate by examining not only who has access to ICT but also to what extent they have sufficient skills to truly make use of such technologies and the information that they potentially provide.

The work is grounded in the belief that disparities in the ability to use ICT may serve to accentuate preexisting political and economic inequities. The first chapter is devoted to examining previous literature regarding digital divides and discussing various government and nonprofit initiatives that have attempted to provide access to various groups of citizens. This chapter also lays out the research methodology that the authors employ for their study, a national telephone survey conducted during the summer of 2001. This substantial empirical effort employed both a sample from the general population and a sample of low-income respondents, the latter in sufficient numbers in order to examine the proposed inequalities in regard to the public's access to, and use of, ICT. The introductory chapter is then followed by subsequent chapters that examine four different divides: the access divide, the skills divide, the economic opportunity divide, and the democratic divide.

As the authors examine each of these divides, they employ various regression models to see if demographic variables, such as age, education, race, political affiliation, and economic status, affect the respective dependent variables employed to measure the particular divide. While the use of such techniques is not new in studies of ICT, political science, or public policy, much of the research to date examining the digital divide has been purely descriptive in nature. Therefore, the ability to statistically control for the possible influence of multiple predictors when examining one divide is a step forward in examining possible social inequities in the public's ability to use ICT. For example, in their examination of the access divide, the authors find that respondents with increased age, lower economic status, and lower levels of education are less likely to have a computer at home, an e-mail account, or Internet access. Furthermore, a respondent's racial group was also found to influence access to ICT, with African Americans and Latinos both less likely to have access to ICT than whites or Asians. Republicans were more likely to have an e-mail address and home computers than Independents, while Democrats were less likely than Independents to have access to the Internet. While these specific results are not necessarily surprising, given the findings from previous research on which demographic groups have access to ICT, the analysis presented in this text is a step beyond most previous efforts. The only potential deficit is a lack of attention to the possibility of endogenous relationships among the four digital divides themselves. The authors allude to this possibility in discussing a link between access and skills (p. 123), but they do not seem to explore the possibility empirically.

The authors' most important contribution is in expanding the discussion of digital divides in order to examine factors that may influence a respondent's skill level in using ICT, as well as respondents' attitudes toward various public access and learning preferences. This focus is reminiscent of the research conducted by scholars in information science that examines the computer self-efficacy of individuals within complex organizations. Such research is grounded in the assumption that access to ICT is not enough. Rather, an individual must also possess the requisite skills to use ICT in order to fully realize its potential benefits. The analysis indicates that technical competency in using computers is

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influenced by a respondent's age, education level, economic status, race, and gender. With the exception of gender, the same variables also influenced a respondent's general level of information literacy. In addition to the discussion of technical competency and information literacy, the authors also present evidence regarding how respondents perceived public accessibility of computers, as well as what factors influence preferences in regards to learning to use new forms of ICT.

While there seems to be inequities regarding access and skill attainment, the authors present convincing evidence that many traditionally disadvantaged groups do perceive ICT to be an important factor in economic achievement. For instance, African Americans, women, younger adults, and the unemployed all considered the ability to use ICT a necessity in keeping up economically. Many groups, especially African American respondents, indicated a willingness to undertake skills training. The authors finally present evidence that there are differences in regard to using ICT as a means of participating in the democratic process. Not surprisingly, respondents who were younger, were better educated, and identified themselves as Democrats had more positive perceptions toward the use of ICT for political participation activities or dissemination of government information. The authors acknowledge that many of their findings regarding the use of ICT in political activities are the same factors that are often found to influence political activity overall.

The final chapter is devoted to discussing how inequities regarding ICT access and skills may be addressed though public policy. While several federally funded programs, such as the E-Rate program, attempt to assist in providing public access to ICT within schools, libraries, and community technology centers in low-income areas, such programs are not primarily concerned with skill development. The authors disagree with those who believe that increased diffusion of ICT within society will ultimately eliminate inequities regarding the use of ICT. They present a much more proactive stance concerning the role of government and believe that government programs should focus not only on increasing access but on assisting with skill development in using ICT. Several potential policy initiatives are suggested, including a focus on skills development at public access sites and experimentation with ICT vouchers to low-income participants. Furthermore, they recognize that disparities in regard to ICT are also tied to broader disparities within the U.S. education system.

Ultimately, reducing the digital divides may only be achieved by improving general educational opportunities for the public at large. While achieving such goals will become increasingly important, the possibility for major interventionist policies on the part of the federal or state governments may be limited in the near future due to economic and political realities. Regardless, Karen Mossberger, Caroline Tolbert, and Mary Stansbury present an interesting and important study that expands academic and policy discussions pertaining to digital divides.

Taming Regulation: Superfund and the Challenge of Regulatory Reform. By Robert T. Nakamura and Thomas W. Church. Washington, DC: Brookings Institution Press, 2003. 141p. $46.95 cloth, $18.95 paper.

— Evan J. Ringquist, *Indiana University*

The central question for this book is how government agencies can "reconcile the necessary use of coercion in regulatory programs with the need to retain popular support" (p. 1). The authors answer this question by examining changes in the Environmental Protection Agency's (EPA) "Superfund" program during the latter half of the 1990s. In the late 1980s and early 1990s, reforming Superfund was a salient political issue. Scholars and stakeholders alike considered Superfund a failed program, and during this period, more than a dozen bills were debated in Congress with the aim of significantly reducing its scope or eliminating the program altogether.

In response to these pressures, the EPA undertook two sets of far-reaching administrative changes that fundamentally changed the nature of Superfund. First, the agency changed how potentially responsible parties (PRPs) were treated under the law, particularly how cleanup costs were allocated among these parties. Second, it revised environmental standards for completed cleanups and replaced an emphasis on removing contaminants from hazardous waste sites with an emphasis on in-site treatment of these contaminants. According to Robert Nakamura and Thomas Church, in implementing these changes, the EPA had to overcome substantial obstacles stemming from organizational culture and legal doctrine, but the reforms themselves dissipated the energy for Superfund reform within Congress. The lessons from *Taming Regulation,* then, are about how significant policy change can be fostered and managed within regulatory agencies, subsequently avoiding having unwanted policy changes imposed from without.

This book has many strengths, among which is the clear, concise, and informative summary of changes in the Superfund program during the late 1990s. After studying the program for more than 15 years, no one is better equipped to provide such a summary than Nakamura and Church. In addition, the book does a very nice job of placing these changes within the broader context of regulatory reform. Unlike most recent literature on the topic, this discussion does not begin with an automatic dismissal of the value of government regulation. Instead, the authors offer a refreshing review of the empirical literature, concluding that traditional command-and-control regulation is not only sometimes necessary but often works. This leads them away from prescriptions for replacing the current regulatory apparatus to the more practical question of improving the existing system.

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The careful reader will note that the strengths are ancillary to the main goals of the book: explaining how regulatory reform can be managed internally and drawing general lessons from this experience at the EPA. Unfortunately, the book is much less successful at meeting these goals. Specifically, the authors employ a questionable definition of "success" when evaluating Superfund reforms; offer a debatable explanation of the source of these reforms by misdiagnosing their source; and draw "lessons" for regulatory reform that are neither supported by the evidence nor come from a generalizable case study. One reason for these shortcomings might be the scarce attention actually paid to reform within the EPA: Only 30 of the 108 brief pages of text are devoted to describing Superfund reforms and explaining why they succeeded.

First, Nakamura and Church "regard EPA's effort to reform the administration of the Superfund program as a major implementation success" (p. 70). Viewed through a less sympathetic lens, these "successful" Superfund reforms consisted of providing immunity to some PRPs, reducing the fines paid by others, and rolling back environmental standards at Superfund sites. These regulatory retrenchments were perfectly consistent with the wishes of polluting firms and the new Republican majority in Congress. Many EPA stakeholders saw these reforms as "successful" only insofar as they staved off elimination of the program altogether. From this perspective, the story of "successful" reform is a story of political survival through appeasement, and in this light, administrators were hardly the "heroes" profiled in Malcolm Sparrow's *The Regulatory Craft* (2000) and cited approvingly by Nakamura and Church.

Second, the authors attribute the success of Superfund reforms to the endogenous factors of leadership, management, and attention to individual incentives within the EPA (Chapter 6). These factors allowed the agency to overcome the legal and cultural obstacles mentioned earlier. While these factors are not irrelevant to the success of Superfund reform, most evidence indicates that the sources of reform were exogenous. First, consider that if the EPA had not acted to reform Superfund, there was near certainty that the program would have been reformed for them, or eliminated altogether, by Congress. Second, none of the reforms were invented by the agency to deal with the congressional threat. In 1993 the Clinton administration put together a task force made up of EPA officials, congressional officials, and Superfund stakeholders in order to craft a set of reforms to be codified in the reauthorization of the legislation predicted to take place in 1994. A deal had been struck on these reforms, and while Republican members of Congress pulled out of the deal in advance of their sweeping victories in the 1994 elections, the point is that the impetus for these reforms was largely exogenous to the agency. The real story of leadership and management is not in how these reforms were implemented but in how the reforms were negotiated in the first place. *Taming Regulation* is silent on this issue.

When considering any general lessons that might be drawn from the Superfund reforms, it is worth considering just how unusual this case is. Superfund officials at the EPA, faced with the certain restructuring and possible elimination of their program, had at their disposal a set of off-the-shelf reforms that had been vetted by all major stakeholders and political principles during two years of intense negotiations. These same officials then employed these reforms to stave off program annihilation. Rather than serving as a source of general lessons about successful regulatory reform, this case exhibits a remarkable confluence of fortuitous circumstances that is unlikely to be repeated often. Moreover, the positive lessons from this case presented by Nakamura and Church have very little to do with the evidence presented in support of the case (pp. 105–7).

The authors puzzle over how EPA officials were able to surmount legal and cultural obstacles in implementing Superfund reforms "in an organizational, legal, and political environment that was heavily weighted against success" (p. 88). Many scholars might disagree with this assessment. All stakeholders were dissatisfied with Superfund, a set of Superfund reforms acceptable to all groups with veto authority was available, and Congress was ready to implement its own reforms if the EPA did not act. Indeed, it would have been far more remarkable if EPA officials had managed to botch the implementation of reform under these circumstances. Now *that* would be a story with lessons for public managers.

Signaling Goodness: Social Rules and Public Choice. By Phillip J. Nelson and Kenneth V. Greene. Ann Arbor: University of Michigan Press, 2003. 261p. $55.00.

— Michael C. Munger, *Duke University*

Are people good? Can they be? Is goodness intentional, or consequential? One way to think of goodness derives from Adam Smith's baker, whose selfishness drives him to bake good, cheap bread. If "good" institutions are enough, we can design mechanisms (with markets being one, but only one archetype) where the collective consequences of self-interest are not harmful. In politics, this invisible hand is the focus of Federalist #51: "Ambition must be made to counteract ambition."

A second project might focus on moral education and an endogenous creation of the "self." Proponents would argue that law and morals should not be viewed as external constraints on selfish men and women. Instead, we must inscribe the laws on their hearts. Each of us is imbedded in a larger context, with ties to one another and to the larger good. Phillip Nelson and Kenneth Greene argue a third alternative: "Goodness" is hardwired, a key feature of human evolutionary success. The key analytic concept they use is "asymmetric goodness," an advocacy for causes or people whose welfare is advanced only at personal cost to the advocate.

Living in a setting characterized by both sociality and scarcity, humans had to solve problems of cooperation. Group survival required that humans adopt a set of social norms that softened the edges of pure market outcomes, giving more weight to the future and more resources to the poor than would occur in a "natural" setting.

The argument is lengthy, but the claims are fairly simple. Imagine that you, along with four other people, have to live in a foxhole in wartime. A fragmentation grenade may at any time be thrown in. The person closest to the grenade can jump on it. Otherwise, everyone shrinks back against the wall, and the grenade kills everyone.

Suppose everyone has the same preference: survival. We talk, and sign a promise: "I swear I'll jump on the grenade." What value does such a "promise," even signed in blood, have for the way that people in the foxhole form expectations about one another's actions? Without knowing more about the individuals, we cannot say. Nelson and Greene are a little mysterious about what we should expect, noting that there is something more at work than simply a taste for asymmetric goodness, but not spelling out just what that something is.

The promise signed in blood is a trope, and it seems to have force, but why? We cannot tell the difference between a real cooperator, who means what s/he says, and a faker, who signs in blood but will cower against the wall if the grenade actually drops from above.

As Nelson and Greene recognize, both in their title (*Signaling Goodness*) and throughout the text, *being* good is not enough. They categorically reject simple altruism as a sufficient explanation. For a society, what is required for the *public* good is an interconnected set of expectations, fulfilled in equilibrium, and couched in terms of reciprocal obligations to act in the larger public interest, even if it means sacrificing private interests.

The problem is that there is no observational ex ante difference between people who intend to act in the public good and those who simply intend to profit from being perceived as wanting to act in the public good. More simply, because we care about the appearance of acting for the public good, that very appearance itself becomes a public good. But that means that the "good," be it political correctness, kindness, charitable acts, or volunteerism, is subject to dilution by mimicry from selfish people. Acting good, but being bad, defies detection in many settings. Everyone promises, but no one jumps on the grenade.

The usual way out, in such circumstances, is reputation. That requires close, extensive personal dealings for trust. Michael Taylor's well-known requirements for "community" (in *Community, Anarchy, and Liberty,* 1982) are a means by which the common-knowledge basis for cooperation can be established. But is there any other way to do it? After all, we tell children "No, that's wrong!" or "You should do what is right." Can we write these rules on their hearts, or are the rules just window dressing, to be practiced by others but not really believed in by anyone?

Nelson and Greene make, quite self-consciously, a group selection argument. There are three roles that are key to "signaling goodness." One can be a practitioner, or an enforcer, or an advocate of moral action. Practitioners simply give up the material benefits of cheating; in forbearance, they sacrifice their own welfare for the greater good. Enforcers punish those they perceive to have acted badly, or to have violated social mores of cooperation. And advocates expend resources in hortatory messages, encouraging moral behavior. The point is that each of these types achieves some credibility for their claim that they are good, precisely because each one incurs some cost in the pursuit of a reputation. "Signaling" good only works if there is some content to the signal. And there is only content if there is some cost.

In my view, this solution is tenuous. Given the private benefits to being perceived as good, most selfish people are likely to adopt hypocrisy. It is easy enough to appear virtuous in public, to admonish the impolite, and to intone pious sentiments about goodness. Nelson and Greene recognize this, but they seem undisturbed that they have raised an ancient problem in a new guise, while doing little to solve it.

The reason, in my opinion, is that the world of the economist is a bit cockeyed. The goal is not to achieve the good society, or to ensure that people are virtuous. Instead, Nelson and Greene (and the economics profession) focus on the "mystery" of why people would want to be perceived to be virtuous. To me, that has never seemed all that mysterious.

How Congress Evolves: Social Bases of Institutional Change.

By Nelson W. Polsby. New York: Oxford University Press, 2004. 257p. $29.95.

— Nicol C. Rae, *Florida International University*

A new work on Congress by one of the most prominent scholars of American government in the past half century is a major event. In this book, Nelson Polsby revisits the U.S. House, a subject on which he authored two major articles in the late 1960s ("The Institutionalization of the U.S. House of Representatives," *American Political Science Review* 62 [March 1968]: 144–68; and Nelson W. Polsby, Miriam Gallaher, and Barry S. Rundquist, "The Growth of the Seniority System in the U.S. House of Representatives," *American Political Science Review* 63 [September 1969]: 707–807). Observing the contemporary House in this volume, Polsby finds a chamber transformed. The "permanent" Democratic majority has been broken, the "solidly" Democratic South now sends a Republican majority to the House, committee chairs are subordinate to the party leadership, and levels of party voting are at their highest since the 1890s.

Polsby's work adds to a substantial scholarly literature on the transformation of the House, headed by David Rohde's *Parties and Leaders in the Postreform House* (1991) and Barbara Sinclair's *Legislators, Leaders and* Lawmaking (1994). In comparison with these works, Polsby's deals more with the technical, social, and electoral factors underlying

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congressional change. He sees electoral realignment in the South as critical, particularly the migration of northern Republicans after World War II that laid the basis for the emergence of a viable Republican Party in the southern states. Hence, Polsby's recurring theme: The invention of air-conditioning, which made the South more habitable to non-Southerners, ultimately proved crucial to change on Capitol Hill.

The book is divided into four substantive chapters, a shorter summary chapter, and a 20-page appendix on methods and sources. Here, Polsby sets out his "quasi-anthropological" approach, which, he argues, "attends to the ways in which the values and status systems of these [well-bounded] communities shape the fortunes of their members as well as the policies and strategies they pursue" (p. 156). (In so doing, he revisits his theory of "institutionalization," a process by which a political body develops strong internal boundaries, becomes internally differentiated, and adopts its own peculiar rules and norms of behavior.) Polsby's method allows him to utilize an array of material that he has accumulated in 40 years of studying Congress, including personal observations; interviews; narrative accounts by members, journalists, and scholars; and ideological and voting data.

The first chapter summarizes the features of the House at the time of Speaker Sam Rayburn in the 1950s: weak party leadership, strong committee chairs, strict adherence to the seniority rule (which delivered crucial committee chairmanships disproportionately to southern conservative Democrats), and, despite the ostensible Democratic majority, ideological domination by a bipartisan "conservative coalition" of southern Democrats and Republicans. Chapter 2 illustrates the erosion of the conservative House regime in the 1960s and 1970s as the more junior, liberal members in the Democratic majority revived the party caucus and secured passage of reforms that eroded the seniority rule and the power of committee chairs, and further weakened the grip of the conservative coalition. Chapter 3 examines the electoral factors behind this liberalization, which Polsby views as primarily a consequence of the emergence of a viable Republican party in the South, engendered by in-migration and economic development. Gradual Republican gains in turn reduced the numbers of white southern conservatives in the House Democratic caucus and expedited the passage of reforms backed by the liberal Democratic Study Group.

Chapter 4 examines the consequences of these reforms. As the Democratic House majority became more homogeneously liberal, the power of the party leaders and levels of partisan voting and polarization rose, too. This, in turn, begat a furious reaction from the increasingly southern, and hence more conservative, Republican minority. When the Republicans took advantage of a number of short-term factors—an unexpectedly advantageous 1990 redistricting, Democratic retirements, congressional scandals, and the short-term unpopularity of President Clinton—to win control of the House in 1994, the new partisan House politics continued unabated. Chapter 5 summarizes what has gone before and elaborates the factors that determine congressional evolution according to Polsby: institutionalization, technology, sociopolitical movements, and innovations elsewhere in the political system.

How Congress Evolves is an intelligent, eminently readable, and accessible study that accurately summarizes how Congress has changed in the last half century and the reasons behind that change. The book also has the merit of reminding us that while it is an extraordinarily "well-bounded" legislature by contemporary standards, as a popularly elected body in a separated governmental system, Congress cannot be impervious to the wider social and political universe of which it is a part. Given the narrow institutional and methodological focus of much contemporary congressional research, this is entirely salutary. Readers may find Polsby's emphasis on the rise of the Republicans in the South as the fundamental explanation of congressional change somewhat overstated. Other contemporaneous changes in the electoral universe—the decline of machine politics, the rise of ideological and single-issue interest groups, political action committees closely affiliated with one or the other of the major parties, the rise of the news media, and the sharp decline in the number of electorally competitive districts—have surely also been instrumental in the emergence of the partisan, centralized House. Moreover, even within the southern context, Polsby somewhat underplays the importance of the 1965 Voting Rights Act in congressional realignment.

These caveats aside, Nelson Polsby has produced another valuable addition to his considerable corpus of scholarship on American government that will assist congressional experts, undergraduate and graduate students, and the politically aware general reader in understanding the contemporary Congress. The lesson that change in Congress can only be fully explained in the context of change in the wider American political universe may seem obvious, but it is one that American political science more than ever needs to learn today.

Economics, Bureaucracy, and Race: How Keynesians Misguided the War on Poverty. By Judith Russell. New York: Columbia University Press, 2004. 244p. $62.50 cloth, $24.50 paper.

— Lawrence M. Mead, *New York University*

The question Judith Russell asks in her book is why the War on Poverty did not include an assault on adult unemployment. The Economic Opportunity Act (EOA) of 1964, the centerpiece of the War, chiefly provided service and education programs for children and youth. Russell believes that only "jobs programs" for adults could have cured poverty. With them, the emerging problem of black joblessness might have been forestalled. Without them, she asserts, the War failed, leading to a conservative era where poverty persists.

The key explanation, Russell argues, is that the Council of Economic Advisors, led by Walter Heller, convinced the

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Kennedy administration to fight joblessness by Keynesian means—cutting taxes to heat up the economy. Russell sides with Willard Wirtz, Kennedy's secretary of labor, who argued in vain for "jobs programs" aimed at "structural unemployment." By this she seems to mean training to raise skills and, above all, government jobs to guarantee employment. She thinks that Lyndon Johnson, who became president upon Kennedy's assassination in late 1963, might have supported such efforts. But by then, the planning for EOA was too advanced to challenge.

Secondarily, the drafters of EOA doubted that the Department of Labor (DOL) could administer an ambitious jobs program. The DOL was dominated by the Employment Service, a labor exchange that catered to employers and resisted control from Washington. Policymakers outside the DOL doubted that it would serve disadvantaged job seekers well, a view that later experience confirmed. Thirdly, the administration ignored pleas from black organizations to address joblessness, something Russell blames on "institutional racism."

The author did exhaustive research in the Kennedy and Johnson presidential papers. These sources, plus interviews with Wirtz and other policymakers, give her book a "you are there" authenticity. In this respect, she surpasses other accounts of EOA that I have read. Her analysis of the DOL's internal problems is valuable. She puts on paper shortcomings of the Employment Service that experts on federal training policy have long known. She also writes forcefully, though with some repetition. In most respects, *Economics, Bureaucracy, and Race* is solid policy history.

I do question her claim that black pleas for jobs programs were ignored. On her own evidence, black leaders did decry black joblessness and demand employment, but what they recommended was not principally government jobs but antidiscrimination measures, economic expansion, and improved education and training to widen black opportunity in the private sector. This they got from the Civil Rights Act and tax cut of 1964, and from federal education subsidies, leading to the emergence of a black middle class.

The author's policy argument fails to persuade. While adult employment programs were omitted from EOA, some were enacted outside it, Russell admits. The Manpower Demonstration Training Act (MDTA) provided adult training. Other programs offered government jobs and development in depressed areas, albeit on a small scale. In the 1970s, the Comprehensive Employment and Training Act (CETA), the successor to MDTA, provided government jobs on a larger scale—up to 750,000 slots a year. But none of these programs evaluated well. This, not just conservative politics, motivated cutbacks in them after 1980.

Meanwhile, Keynesianism triumphed. The 1964 tax cut helped to trigger the boom of the later 1960s, creating more and better jobs for rich and poor alike. Over 1961–69, unemployment for nonwhites fell from 12% to 6%. Over 1959–74, black poverty plummeted from 55% to 30%, the sharpest fall ever recorded. Over 1990–2000, driven by a later boom, black poverty dropped from 32% to 23%. Russell believes, with Marx, that a capitalist economy is "incapable of creating enough jobs in normal times for everyone who needs a job" (pp. 38, 163–64). That view is hard to credit when, in recent decades, the nation has attracted millions of unskilled immigrants to fill jobs that, for whatever reason, poor adults do not take.

It is true that entrenched joblessness and poverty remain, especially among blacks. But, experience suggests, this is not due to "structural unemployment" in the impersonal sense spoken of in the 1960s. Already under MDTA, administrators realized that the work problem lay not in discrimination or automation but in "hardcore, disadvantaged individuals" with very low skills (pp. 44–45). The structures that bar employment lie chiefly not in the labor market but in a culture of defeat where the poor want to work but lack the hope to do so regularly. Russell treats the culture of poverty as blaming the victim, but no other approach explains how long-term, working-age poverty can exist in an affluent society where jobs sufficient to overcome poverty are readily available.

In the 1990s, legions of black welfare mothers left aid for jobs, driven by new work requirements, a hot economy, and higher spending in child care and wage subsidies. Except in New York City, public employment played little role. The key, rather, was that society both enforced work and facilitated it with new benefits other than jobs. Meanwhile, black men continued to withdraw from the labor force. In light of the welfare success and immigration, the reason cannot be that jobs are literally unavailable to them. Rather, it is harder to obligate poor men to work because they seldom draw welfare. Perhaps more men might be made to work to pay child support obligations or fulfill the conditions of probation or parole. Such thinking reflects an analysis opposite to Russell's. The work problem is seen to lie not in special barriers but in a lack of a serious onus to work. The jobless are seen not as too restricted to work but as too free.

Russell omits or barely mentions CETA, immigration, welfare reform, or policy events that cause one to question her viewpoint. This is partly because of her close focus on the framing of EOA. A deeper reason is her neglect of serious policy argument. She simply assumes that a more generous welfare state is the answer to poverty. That case must be argued more seriously, or long-ago decisions about the War on Poverty cannot appear as the missteps she claims.

Pathways to Prohibition: Radicals, Moderates, and Social Movement Outcomes. By Ann-Marie E. Szymanski. Durham, NC: Duke University Press, 2003. 344p. $89.95 cloth, $24.95 paper.

— Suzanne Marilley, *Capital University*

In this book, Ann-Marie Szymanski achieves what may have been an unintended goal: She demonstrates that the success of a social movement that seeks transformative public

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policy depends on *both* the development of flexible, highly adaptative strategies for local reforms *and* the emergence of opportune conditions for the consolidation of local successes into national—in this case, constitutional—reform. Her study of Prohibition politics and organizational strategies crafted over time brings together insights from new institutionalists who explain how the unique interests of governmental agencies and "states" shape policy outcomes, as well as from political scientists whose studies on the relationships between ideologies, political strategies, and small group dynamics have generated varied pressures for policy reform. If the social movement theorists pay attention to this study, they will enlarge their perspectives, build more sophisticated models, do more comprehensive research, and explain more fully and accurately how social movements actually generate successful political reforms.

Szymanski convincingly demonstrates that support of the "local option" and presentation of the same as the foundation of a "moderate" approach to the regulation of the sale and consumption of alcohol explains the creation of a grassroots base strong enough to assure the passage and ratification of the Eighteenth Amendment. She admits that the movement's leaders developed "local gradualism partly in response to recent modifications of the political system, namely the devolution of the state legislatures' liquor licensing power to the localities, and the judiciary's growing acceptance of these licensing regimes." (pp. 5–6). She astutely notes, however, that state legislatures' devolution of such power to local governments and judicial toleration of the same began *after* moderate prohibitionists strenuously promoted the local option. Thus, the adage that in the United States "all politics is local" may owe much to the efforts of moderate prohibitionists. The republican character of small cities, towns, and rural counties crystallized in the late nineteenth century. Whether one's hometown was once "dry" as opposed to "wet" matters much in local histories. Indeed, the national center of the Anti-Saloon League was situated in Westerville, Ohio, a suburb of Columbus that still prides itself on being "dry."

Despite the repeal of Prohibition, the mass mobilizations to limit or abolish the sale of "spirits" constitute the legacy of the first successful popular efforts to amend the Constitution. This movement had roots in the abolition movement whose leaders also recognized that local organization mattered. Abolitionist William Lloyd Garrison deliberately began his mobilization of support in the villages and small towns of New England. Once there was a base of support outside the cities, he brought the message to Boston, New York, and Philadelphia. A similar strategy was used to mobilize Chinese peasants in the 1920s and 1930s, a step considered as critical to the success of the Maoist revolution.

Szymanski invites scholars—as well as contemporary leaders of social movements—to pay more attention to local politics. Reform leaders who craft reform strategies that are locally based ensure that their movement possesses the widest boundaries from the start. Long-run success may prove elusive as it did with Prohibition, but the influence of the cadres and clusters of leaders who led the effort can be immeasurable. The Prohibition movement and its organized opposition developed networks and hierarchies of businesses, labor organizations, religious congregations, and political parties the outlines of which can still be detected today.

Szymanski's findings could serve as a resource for scholars who want to know the roots of republican traditions in the United States, especially regarding debates on the legalization of marijuana, the establishment of drug testing in secondary schools, and related community "standards." To scholars who might want to pursue such a research agenda, I offer my chief dispute with Szymanski: I consider her use of the labels that reformers used to identify themselves and their opponents overly confining and restrictive. Moreover, in American politics, the labels "radical" and "moderate" constitute political weaponry. From the campaign to ratify the Constitution to the current presidential campaign of 2004, groups and candidates who successfully promote themselves as "moderates" succeed far more often than those who portray themselves as ideologically correct.

In *Pathways to Prohibition,* Szymanski sets the "radical" supporters of prohibitionist aims off against the "moderates" who promoted local gradualism. But the reformers committed to Prohibition were more diversely motivated. Certainly those who created the Prohibitionist Party wanted consistent rules across their states and the nation. Rather than "radical," they were utopian idealists whose shared desire to purify the citizenry—person-by-person—took on an obsessive character. According to Rogers Smith's multiple traditions framework, the Prohibitionists were republican Americanists: Not every American could ever become a true American—white, Anglo-Saxon Protestant and obedient—but each could be and should be taught and expected to behave as if he or she were one.

At the same time, other supporters of prohibitionist aims seem to have been drawn to a long-term goal that might well never be fully achieved. As the president of the Women's Christian Temperance Union (WCTU) from 1879 to the mid-1890s, Frances Willard aimed primarily to activate the political participation of traditional women so that the polity would become less male dominated. She used the extremist, idealistic rhetoric of the Prohibitionists as a means to mobilize *women.* But she remained committed to the ethic of *temperance:* She urged her followers to accept reform goals that fell well short of Prohibition. Thus, Willard pursued a strategy that aimed for the truly radical goal of winning women full political inclusion, but her approach lacked the single-minded idealism of the Prohibitionists.

By using the value-laden political labels chosen by the local option reformers to describe themselves, Szymanski falls into the trap of advocating or vindicating their approach and rejecting that of the Prohibitionists. Unfortunately, what

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gets lost in all this is a sense of how together *both* the pragmatic localists and the idealistic Prohibitionists effectively thwarted the wets. In so doing, they shut out alternative social reform agendas that called for saving small farms, improving wages, making workplaces safer, and refashioning public buildings and spaces for more vigorous and robust sharing of community resources.

Relabeling the Prohibitionists as idealistic and localism proponents as pragmatic would not require Szymanski to alter her explication of the different pathways to Prohibition. She presents trenchant analysis of the different pathways, and each chapter offers key insights on the relationship between social movements and constitutional reform (as well as the transformation of a social movement into a political reform movement). She thereby opens up exciting possibilities for new, fuller, fairer, and sober studies on the effectiveness of contemporary social movements, such as the environmentalists, the Christian right, and many others. Her observations on the uses of local gradualism already made by some contemporary groups are inspiring.

Talking about Politics: Informal Groups and Social Identity in American Life. By Katherine Cramer Walsh. Chicago: University of Chicago Press, 2003. 264p. $57.00 cloth, $19.00 paper.

— Diana Owen, *Georgetown University*

Casual conversations that take place among groups of acquaintances can have significant implications for political identity formation. The core thesis of this impressive study by Katherine Cramer Walsh maintains that small groups work collectively to create social contexts that influence members' understanding of politics. Individuals develop perspective through their identification and experiences with a group. She argues that people's ability to engage in "oppositional processing" by juxtaposing their own social position to that of others shapes their political outlook. Race, not political ideology, is the most salient influence on political judgment.

Walsh uses participant observation to examine the group dimensions of political talk. For almost two years, she dropped in on the informal gatherings of the "Old Timers," a social group of middle-aged to older white men with conservative, patriotic political values who met daily at a corner store in Ann Arbor, Michigan. After winning the trust of most members, she was able to record their interactions, focusing particularly on their means of developing and clarifying social identities and the content of their discussions. Also at the corner store were a variety of racially diverse groups who had little interaction with the "Old Timers," but whose physical presence provided a constant out-group reference point for them. For comparison, Walsh spent time with the "Craft Guild," a women's group with the express purpose of working on projects to benefit their church. Because the "Guild" was an instrumental organization whose members had little in common, the group did not encourage the development of social identities, and political discussion rarely took place.

This study makes an important contribution to public opinion research by virtue of its innovative methodology. While political scientists rely almost exclusively on survey, interview, and focus group methodologies for studying opinion and identify formation, Walsh's participant observation strategy is vested in the tradition of well-respected work by E. E. LeMasters (*Blue Collar Aristocrats,* 1975) on working-class patrons at a Wisconsin tavern; Elliot Liebow (*Tally's Corner,* 1967) on low-income African American men in Washington, DC; Mitchell Duneier (*Slim's Table,* 1992) on African American men who meet at a Chicago cafeteria; and Nina Eliasoph (*Avoiding Politics,* 1998) on white suburbanites who frequent a country western bar. Participant observation is a welcome complement to studies employing survey and interview methodologies, with their inherent focus on the individual, and focus groups, with their artificial group dynamics simulated in a laboratory setting. Walsh observed the Old Timers in their own environment, which permitted her to document the informal rules that identified group members and set them apart from others who shared their space. This methodology also allows the conversation to be the unit of analysis, rather than the social network, as is usually the case. This focus has the virtue of enabling the researcher to examine directly how and why political discussions arise, rather than simply who is talking to whom.

Researchers employing participant observation run the risk of becoming co-opted by their subjects. Over time, Walsh's status with the Old Timers changed from observer to group member. However, her keen awareness of this transition, and her deliberate attempts to keep her interjections into the discussion to a minimum, prevent her from compromising her analytical purpose. The detailed methodological appendixes provide a road map for those seeking to undertake this type of study.

Another significant implication of this work is the challenge it poses to some core assumptions of social capital scholarship, which imply that enhancing opportunities for political discussion necessarily translates into positive democratic outcomes. As Walsh documents, not all face-to-face interactions strengthen social bonds or work to benefit the public good. Greater opportunity for political discussion does not ensure greater tolerance. In fact, personal interactions can widen social divisions, with group attachments strengthening as members continually define themselves in opposition to "those who are not one of us."

Walsh's study begs us to consider that informal discussion may be as relevant to the development of democratic orientations among average citizens as the town-square-type deliberations favored by social capital theorists. One of the most consequential findings of the study challenges models positing that elite rhetoric translates directly into mass opinion and permeates citizen discourse. Political talk in

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social groups largely arises out of discussions of other things. The author provides compelling examples of how everyday conversations about life, entertainment, and sports can transmute into meaningful political discussions that mostly reinforce the dominant values of the group. Although average citizens may discuss particular topics promoted by elites and publicized by mass media, they do not necessarily adopt dominant news frames. Instead, they engage in a process Walsh terms *circumventing,* whereby they interpret news stories with first-hand points of view drawn from their own experiences and group interactions. While elite/media frames help citizens to make sense of public affairs by identifying the core ideas and actors involved in policies and events, the public does not necessarily buy into these interpretations.

Talking About Politics lays the groundwork for future investigations into the development of political talk among citizens. Such research would benefit from a more precise specification of what distinguishes political from nonpolitical discourse, especially as the boundaries between social and political discussion are highly fluid among the mass public. In addition, Walsh's rich case study of the Old Timers considers a group whose members were socialized to politics in a distant era and now have the time and inclination to become regulars at the corner store. It would be fruitful to investigate whether the trends she observes are generational, and to consider the dynamics of political talk within groups of young people. In addition, the racial dynamics that influence group political discussion, which Walsh begins to account for this in this study, might be further fleshed out by focusing on social groups with diverse membership.

White Nationalism, Black Interests: Conservative Public Policy and the Black Community. By Ronald W. Walters. Detroit: Wayne State University Press, 2003. 360p. $49.95 cloth, $26.95 paper.

— Charlotte Steeh, *Georgia State University*

Ronald W. Walters dislikes the direction that race relations have taken in the present-day United States. In his book, he portrays race relations as a war between the radical conservative wing of the Republican Party and blacks. It goes without saying which group has been losing. It is a story told quite starkly with few nuances or nagging caveats—as the title suggests. The reader feels immediately the depth of Walters's anguish, and the strength of his argument builds as he lays out example after example.

Stated simply, Walters contends that a white nationalist movement began to develop among political conservatives in the late 1960s at the close of the Civil Rights era and gathered steam particularly during the 1980s and 1990s. Fostered by the economic and political progress of blacks that seemed to threaten white dominance and by the economic stagnation that plagued the white middle and lower classes, the movement sought deliberately to disadvantage the "offending culture" (p. 22)—that is, blacks—in an effort to reassert white control.

But Walters is interested not only in outlining this shift in political ideology; he is as interested in describing the practical effects that the resulting policy racism has had upon black life and opportunities. Thus, the book falls quite naturally into two parts. In the first part, he lays the theoretical foundation for his discussion of white nationalism, describes its tenets and historical antecedents, and focuses on the arguments of its leading proponents, epitomized by Newt Gingrich as Speaker of the U.S. House of Representatives and author of the 1994 Contract with America. Since whites are in control of societal institutions, they do not need to acknowledge the racist motives underlying public policies. This fact presents an evidentiary challenge to the author's argument. The only way to establish racist intent is to show how policies that have been justified in more general terms actually harm the targeted group. Thus, in the second part of the book, Walters lays out case after case in support of his position. The titles of the chapters reveal both the scope of his discussion and the racial animus that, according to him, undergirds national policies. There is a chapter titled "The Attack on the Black Poor," another called "The War on Blacks: Criminalizing a Race," and yet another, "Attacking Black Access to Education." To my knowledge, no other African American scholar has supported the underlying premise with so much careful and devastating detail.

Walters does establish beyond a shadow of a doubt that various public policies across a diverse set of issues have negatively affected the lives of African Americans, particularly poor African Americans. However, the proposition that "race has been a central determinant" in shaping these policies (p. 271) should be treated with some skepticism. Although he comes down solidly on the side of race in the scholarly debate that pits race against class in explaining discrimination and prejudice, he has not convincingly demonstrated the viability of this position. He has not shown that the policies promoted by the radical conservative wing of the Republican Party disadvantage blacks more than they disadvantage lower- and middle-class whites. To many observers, the policies implemented by recent Republican administrations, the current one in particular, have been decidedly class based, aligning the rich against the poor and the middle classes. For Walters, however, the only underlying contributor is race. Had he outlined the arguments for each side in this debate and squarely faced the implications of each, his focus on race alone would have been more defensible. However, comparing the impact of public policies on whites as well as blacks would have required empirical data to measure the size of the impact on each race and taken the book in a totally different direction.

Just as he does not discuss the race/class debate, Walters does not refer to a set of theoretical propositions that seem to fit very well the evidence he has amassed. Social dominance theory asserts that a majority group in a society, when

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threatened by a subordinate group, will seek to maintain its control by implementing policies that have detrimental consequences for this minority. The dominant group members do not act because they are individually consumed by antiblack feeling but because their overall group well-being is at stake. The theory also usually portrays the opposing groups as monoliths, that is, all blacks against all whites. Yet the author's examples suggest that there are distinctions within the groups. Not all conservatives are white nationalists, in his view, and presumably neither are any Democrats, since he hardly ever mentions them. By tying the desire for social dominance to party affiliation and thus implicating only certain segments within the majority group, Walters could have viewed his efforts as a refinement of social dominance theory. Regrettably, he does not do so.

In a nod to theory, Walters briefly points out in the final chapter that current research in political science relates ideology to the formulation of public policy. Popular opinion, on the other hand, has not accepted the view that racist ideology leads to discriminatory public policies. Walters sees this book as strong support for the existence of such a relationship.

Whether intentionally or not, because Walters never acknowledges it, *White Nationalism, Black Interests* turns on its head the argument, advanced particularly by Paul Sniderman and his colleagues, that conservatism and antiblack attitudes and behavior stem from two separate and distinct sets of ideas. Even though his analysis is not as rigorous as it could be, Walters shows that the two are indeed related, and his evidence makes it clear that the Sniderman position needs considerable modification. For anyone who is interested in documenting how many of the policies passed in the last 20 or so years have detrimentally affected blacks, this book will be of immense value.

Models of Voting in Presidential Elections: The 2000 U.S. Election. Edited by Herbert F. Weisberg and Clyde Wilcox. Stanford: Stanford University Press, 2004. 320p. $65.00 cloth, $24.95 paper.

— Lonna Rae Atkeson, *University of New Mexico*

This edited book on the 2000 election covers many of the traditional areas in voting behavior research, including the role of candidate evaluations, issues, and partisanship. Authors also rely on traditional models of voting behavior research, including the social-psychological, the sociological, and the rational choice model, to understand a wide variety of questions concerning the voters and vote choice in the 2000 election.

Thus, the chapter by Herbert F. Weisberg and Timothy G. Hill and the chapter by John H. Kessel use the social-psychological model to inform our understanding of voter choice, while Steven E. Finkel and Paul Freedman use it as a foundation in understanding the important question of voter turnout. The sociological model is well used in the very interesting piece by Harold W. Stanley and Richard G. Niemi that examines how groups have aligned and realigned with the parties over the last half century. Likewise, the Kristin Kanthak and Barbara Norrander piece on the gender gap also relies, in part, on the sociological model of voter choice. The rational choice model of behavior is used in the remaining chapters. William G. Jacoby focuses on the role of ideology in the 2000 election and compares his current findings with the value of ideology in previous presidential elections. Rational choice is also relevant to the chapter by Barry C. Burden in his analysis of the vote decision for minor parties and candidates, particularly as it applies to Ralph Nader's and Patrick Buchanan's roles in the 2000 election and how their ballot presence influenced the outcome of the 2000 election. Finally, it also is pertinent to the Janet Box-Steffensmeier, J. Tobin Grant, and Thomas J. Rudolph essay on the role of the campaign finance issue on turnout and choice in 2000, as well as to the chapter by Helmut Norpoth about the role of economic attitudes and incumbency to vote choice and to David C. Kimball's examination of split-ticket voting. This collection of essays, therefore, offers an opportunity for scholarly explorations and consideration of the major theories in political behavior research and their applications to a variety of questions in the context of the extremely close and fascinating 2000 election.

Many of the chapters rely on or expand similar works the contributors have completed in the past. This updating provides useful linkages with the past and demonstrates how different election contexts play a role in shaping the critical factors in any given election. For example, using the entire National Election Studies, the Stanley and Niemi essay is very insightful as it lays out the changing nature of the partisan coalitions and what that means for the future. Likewise, the Kessel, Finkel and Freedman, Kimball, Weisberg and Hill, Kanthak and Norrander, Burden, and Norpoth chapters all have historical components to their analyses.

The essays also present a wide variety of data sets and analytical methods. This is not only analytically appropriate but also a strength of the book because it provides an overview of the kinds of data and methods political scientists employ in this research area. Moreover, it presents these varying statistical techniques in a manner accessible to an undergraduate or less methodologically advanced audience. Most of the authors do not dwell on particular coefficients or the details of each model variable, but on how the model addresses the question at hand.

While this eclectic group of essays in no way tells a particular story or offers a coherent picture of the 2000 election, it does offer a broad look at it in a way that outlines many of the major questions in the subfield. In this way, it presents an overview of the kind of questions American political behavior scholars ask, the theories they use, the types of data they employ, and the statistical analyses and analytical approaches available. It also details the inferences

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that can be made from well-considered and tested questions in a contemporary election setting.

Given these qualities and the broad number of questions asked, this collection of essays would be best suited for an undergraduate political behavior course. Professors could guide discussion around different questions and models of political behavior, including the importance of attitudes, issues, ideology, and candidates on voter choice; the importance of candidate behavior on voter turnout; the changing makeup of partisan coalitions since the New Deal; the decision-making calculus for third party voters; and the motivation and contextual factors that promote ticket splitting.

Alternatively, professors could also consider discussions on the different conclusions found by different articles on the same subject. For example, one interesting question these essays explore is the role that outgoing President Bill Clinton played in the election outcome (compare Weisberg and Hill to Norpoth). Another question might be the value and use of ideology by voters in this election (compare Kanthack and Norrander to Jacoby). Or, alternatively, another interesting question is the current value of party identification and whether it is increasing or declining (compare Kimball to Finkel and Freedman). Other discussions may arise from this text, including questions about epistemology and the importance of measurement and good definition and general research design. Finally, the breadth of these essays can lead to valuable and interesting discussions on the types of factors that are most important (including characteristics of voters, candidate behavior, party ideology, the role of economics in good and bad times, the influence of a sitting president who cannot run, the influence of coalitions), as well as when and why they are most important in explaining the political behavior and attitudes of voters.

In conclusion, *Models of Voting in Presidential Elections* offers a solid examination of the 2000 election from a variety of vantage points and, in many cases, incorporates the history behind the questions being asked and their importance to political behavior research. In this regard, the book is a good demonstration of the scholarly activity in this subfield and, therefore, would be most appropriate, along with other readers and texts, in an undergraduate political behavior course.

COMPARATIVE POLITICS

Communities and Law: Politics and Cultures of Legal Identities. By Gad Barzilai. Ann Arbor: University of Michigan Press, 2003. 314p. $65.00.

— Donna E. Arzt, *Syracuse University College of Law*

This recent monograph—at least five years in preparation—might better be described as a doubleheader. While professing to be an original tract of communitarian political theory, it happens to utilize three communities in Israel to illustrate its multiple theses. This is the twenty-first publication in the interdisciplinary "Law, Meaning and Violence" series edited by Martha Minow, Austin Sarat, and Elaine Scarry, which explores how law's narratives, practices, and institutions embody and give voice to power and violence. The volume recently won the Yonathan Shapiro Prize for the best book in Israel Studies from the Association of Israel Studies. But instead of concentrating on the otherwise ubiquitous topic of Israel's national security vis-à-vis the occupied territories, it highlights the social and political rights and identities of insular, internal minority groups that choose between legal action and violence when confronted with state power.

Given its dualistic foci, *Communities and Law* is inevitably composed of two parts: In the first, author Gad Barzilai lays out his theoretical framework, expounds his philosophy of critical communitarianism, and theorizes in fairly abstract terms on the legal and political culture of nonruling communities (Chap. 1), as well as the domination and identity politics inherent in state law (Chap. 2). In the second part of the book, he devotes separate chapters to three examples of subordinate subcultures: Arab-Palestinian citizens living within the original borders of Israel (Chap. 3); various feminist groups, including Arab-Palestinian women (Chap. 4); and fundamentalist ultra-Orthodox Jews (Chap. 5).

The first part offers a seemingly repetitive, jargon-filled elucidation of what the author calls "critical communitarianism," a revision of communitarianism's rebuke of liberal individualism, which not only emphasizes the political culture of communities but, in particular, how "nonruling" or subordinate communities use their own communal versions of legal culture to resist the state's domination-through-legal-ideology. As Barzilai himself summarizes this conceptual material: "Communal practices include alternative hermeneutics, legal mobilization (i.e., utilization of state law, mainly in legislation and litigation), demobilization (rejection of state law), and countermobilization (action against state law by means of communal law or agents of state law)" (p. 281). Of the sections devoted to particular nonruling communities, Chapter 4 is the least cohesive of the three case studies, probably due to the fragmented nature of wide-ranging groups Barzilai broadly labels as "feminist." Although the ultra-Orthodox also consist of an array of subidentities—ranging from participants in the ruling government coalition to outright rejectionists who refuse to cooperate with state functions—the numerous lawsuits challenging Orthodox religious monopolies provide the author with concrete cases with which to examine ultra-Orthodox attitudes to

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legal institutions, particularly the High Court of Justice. Many ultra-Orthodox respect the court but scorn its justices and their decisions. Finally, the Arab-Palestinian chapter relies heavily on a person-to-person opinion survey that Barzilai conducted in July 1998 of attitudes toward legal and illegal political actions, perceptions of discrimination, and other aspects of their legal culture. Yet such a limited temporal snapshot (dated over two years before the start of the second Intifada in the occupied territories) can hardly do justice to the evolving and conflicted identity of such a distinctively situated ethnic minority.

By far, the most significant contribution of *Communities and Law* is found in the conclusion (Chap. 6), where the author compares how the three nonruling communities, respectively, mobilize the Israeli legal system in behalf of their political objectives, usually attaining only minor, if any, legal reforms but in so striving, legitimize the very hegemonic ideology that is contrary to their own nationalistic, gender, or fundamentalist religious identities. In some cases, the state has co-opted the group's acceptance for its own purposes. For instance, "[t]he founding of Israel as a Jewish state was grounded in the legitimacy acquired from those Jews who aspired to take an active part in the Zionist enterprise, including Zionist Orthodox Jews. The legitimacy conferred by Jews who matched the prototype of the 'original' [authentic] Jew . . . was considered to be vital to the Zionist cause" (p. 233). By contrast, while some Orthodox communities have made a utilitarian choice to participate in national politics, Arab-Palestinians have been reluctant to use a legal tool, law reform litigation, which implicitly recognizes a state structure they prefer autonomy from. Yet paradoxically, "Palestinian feminists wish to see more (not less) state legal intervention in the religious autonomy of the Sharia courts in order to attain more gender equality" (p. 108).

Conspicuously absent, however, is any sustained comparison to states other than Israel. At best, stray references to other countries are mentioned without analysis of the particulars. This is problematic because, in many respects, Israel is a unique case. No other country is a self-proclaimed democratic "state of the Jewish people." Indeed, there are few other countries purporting to be both democratic and formally religious, in the sense that religious values and injunctions are embodied in the national legal system, from the constitution on down. (Ireland qualifies but Turkey and India do not, being avowedly secular. Democratic states with an "established church" do not usually maintain separate religious courts or employ religious definitions in their immigration law.) But Barzilai does not analyze Israel's unique features in relation to similar phenomena elsewhere (such as Germany's immigration law), nor does he indicate which characteristics are common to other states. This would all be fine if his book were simply a contribution to Israel studies. However, its nonspecific title betrays the author's ambition to transcend that particular niche. He cannot establish the viability of his critical communitarian theory through such a limited application.

A further problem is the failure to provide sufficient background so as to accommodate the reader who is relatively unfamiliar with Israeli society and government and/or with general legal procedure and institutions. For instance, despite the numerous discussions of cases (usually undated) decided by the High Court, Barzilai never clarifies the court's structure and special role in a legal system without a comprehensive written constitution. Similarly, unexplained historical references to peculiarly Israeli traditions such as "the Orthodox and secular status quo" presume a predominantly Israeli audience. Such editorial oversights are perhaps the inevitable feature of such a dual-directed tome.

Locked in Place: State-Building and Late Industrialization in India. By Vivek Chibber. Princeton: Princeton University Press, 2003. 335p. $39.50 cloth.

— Ronald J. Herring, *Cornell University*

Vivek Chibber's book is exceptionally clear, fresh, empirically rich, and analytically tight. It clears some conventional cobwebs in thinking about developmental states. It should be read widely.

The great debate in development conventionally revolves around two axes: state and market. The developmental-state literature argues that rare successes among late developers come from particular configurations of state and society organized for economic growth (see Meredith Woo-Cumings, ed., *The Developmental State,* 1999). A more market-conforming literature holds that state interventions in the economy only generate rent-seeking behavior and distort market signals, creating inefficiencies. The heterodox position—that there are multiple paths to development and no single orthodoxy—has gained some heft since Joseph Stiglitz won the Nobel Prize in economic science (see his 2002 book, *Globalization and Its Discontents*). The analytical problem of developmental-state theorizing has always been that there is no parsimonious validated theory of growth. There are some consensus variables, but great dispute about their relative contributions, sequencing, path dependencies, conjunctural shocks, and outliers. Chibber tries to avoid the growth-theory trap, but of logical necessity employs a yardstick. He refers to "ineffective" state-guided capitalism, and explicitly compares India to Japan and Korea, which have installed effective institutions of the developmental state (p. 195–96). The implicit developmental-state metric is growth. No one would be interested in The Ministry of International Trade and Industry (MITI) if Japan had grown no faster than Sri Lanka.

Chibber's dense and original study of India is counterpoised to a useful reinterpretation of the Korean case. The "secret of the developmental state" is leverage to discipline domestic capital (p. 74). State autonomy from dominant

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classes matters fundamentally. Anyone who thinks that this formulation antiquated Marxian structuralism must read this book. Chibber's innovative historiography shows that industrialists of India, in the critical historical period of state formation after colonial rule—1947–51—were able to defeat the developmental strategies of the Indian National Congress (INC). They did this by showing the muscle of an investment strike and by intensive lobbying within party and state. Within the INC, Nehru was marginalized; labor was demobilized. Capital won the formative battle over who rules economic policy. Chibber's evidence on this point is very clear. Planning in India was thus launched without the institutional base necessary for planning. The result was drift toward an "ad hoc and informal" (p. 177) system in which authority was fragmented and regulators bargained with firms and established ties through networks. This system of planning produced what I have called "embedded particularism," rather than "embedded autonomy" (Herring, "Embedded Particularism: India's Failed Developmental State," in Woo, ed., op. cit.). It was not growth friendly.

Korea began with equally low income and a failed strategy of import-substitution industrialization (ISI) but switched in the mid-1960s to export-led industrialization (ELI). ISI dominated the poor world; any state can distribute boons and exhortations and protect permanently infant industries. To make the leap to ELI, the state must be able to demand performance in return, and hold capital accountable, to break into international export markets. Chibber argues that the Indian National Congress made "a fatal misreading of the situation" (p. 44) in thinking that it could install a developmental state in the teeth of resistance from capital, once labor was demobilized. The fatal misreading became "locked in place."

This bold title suggests very strong path dependency. What is locked in place is the incapacity of the Indian state to discipline capital. To be explained is not only the "installation" of the developmental state but also its "immutability" (p. 193). Yet both nations have significantly shifted development strategies over time. One wishes for a finer-grained sense of variance, in both strategies and politics. Pranab Bardhan argues that the Indian state made basic investments in the early period, and supported significant industrialization (more in terms of share of GNP than share of employment)—not so fast as some new nations, much better than others, and far better than the colonial record. But eventually, a form of democracy caught up with the commanding heights: Dominant classes in an uneasy coalition began to split up the state's surplus as largesse and to offer public consumption in the form of patronage and populism to maintain legitimacy (*The Political Economy of Development in India,* 1983). There is a question variance of analytical scale. Aseema Sinha's forthcoming book, *The Regional Roots of Developmental Politics in India: A Divided Leviathan,* demonstrates that developmental statism in India operates at multiple levels, with varied outcomes; some states—typically the size of average European nations—do quite well, some have temporally uneven records, and some do very badly. If one averages these state-level experiences, the mean is low, but this central tendency is like the mean temperature of Europe. One would want to know the temperature in Greece and Finland to make any use of the information. Sinha finds that the state of Gujarat, for example, does extraordinarily well, for reasons familiar in the developmental-state literature. The provincial state coordinates well with capital, intercedes on its behalf at the national level, and provides the incentives and aids that capital needs to be bribed into behaving properly.

Finally, there is a chicken-or-egg problem of causation. For Chibber, development models (ISI/ELI) orient preferences of capital, driving state-building outcomes. But "models" are the vector sum of pushing and pulling between state and capital in this account. Korean ELI was a " 'pact' between the state and its bourgeoisie"; it was also a "happy accident" (p. 82) of place and time. Chibber sagely acknowledges "the role of sheer luck" (p. 203). Specifically, Japanese capital smoothed the way for Korean capital to export as part of Japan's upward product-cycle trajectory; otherwise, Korean capital would not have given up the comfortable niche in ISI occupied continuously by the Indian bourgeoisie. Multinational corporations investing in India not only did not create export markets but also forbade exports, blocking Indian capital's interest in export-led industrialization. Collaboration between Japanese and Korean capital made export-led industrialization a politically feasible option in Korea. India (and Latin America) lacked this option, as capital was too strong (and comfortable) "within," and there was "no deus ex machina coming from without" (p. 204). Structural accounts allow for historical contingency, but luck, accident, and Japan sit uneasily with an argument for bringing domestic capital back into statist interpretations of developmentalism. Indeed, one is tempted to conclude: No Japan, no developmental state.

Islam, Charity, and Activism: Middle-Class Networks and Social Welfare in Egypt, Jordan, and Yemen. By Janine A. Clark. Bloomington: Indiana University Press, 2004. 256p. $49.95 cloth, $23.95 paper.

Why Muslims Rebel: Repression and Resistance in the Islamic World. By Mohammed M. Hafez. Boulder, CO: Lynne Rienner Publishers, 2003. 240p. $52.00 cloth, $25.00 paper.

Islamic Activism: A Social Movement Theory Approach. Edited by Quintan Wiktorowicz. Bloomington: Indiana University Press, 2004. 320p. $59.95 cloth, $24.95 paper.

— Eliz Sanasarian, *University of Southern California*

It is a pleasure to read three well-researched books that use a social movement approach to explain contemporary Islamic activism. A combination of rigorous fieldwork and focused use of theory offers an alternative viewing lens for a host of

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issues involving social work, women's activism, use of violence, recruitment strategies, protest movements, political participation, and economic reforms.

With meticulous attention to detail, Janine Clark examines three case studies in *Islam, Charity, and Activism:* charitable medical clinics in Cairo, the Islamic Center Charity Society and its commercial institutions (e.g., hospitals, private schools) in Jordan, and the Women's Sector of the Islah Charitable Society in Yemen. The result of a ten-year effort (1991–2002), her research methodology is explained with honesty and precision. In the process, a range of interesting issues are voiced. For instance, many secular clinics adopt Islamic names in order to gain greater "credibility and legitimacy" (p. xii); another is the fact that the author had easier access to male interviewees, particularly in a restricted Yemeni society, because Western women are considered "a third sex—one that is unbound by many of the social customs" (p. xii).

Clark's systematic study calls into question some of the basic assumptions about the role of Islamic social institutions (ISIs). While other scholars state that ISIs are arenas of political activity where middle-class professionals recruit and mobilize the poor into the Islamic movement, Clark argues the opposite. The book shows that due to strategic and operational reasons, the ISIs do not promote cross-class alliances. Using ideas of vertical and horizontal ties from the social movement literature, the study argues that ISIs develop and strengthen horizontal ties and benefit the middle class. They owe their existence to their ability to attract middle-class volunteers, employees, donors, and government contacts. Overlapping "networks between the home, mosque, workplace, other ISIs, clubs, and even friends living abroad" (p. 34) nurture and maintain homogenous social relations. In Cairo, for example, the best Islamic clinics are located in middle-class neighborhoods, and the ones in poor sections suffer both in terms of quality and quantity of services. While vertical ties to the poor do exist, and the ISI services do benefit the poor, it is the middle class that benefits most. The poor cannot afford school and commercial services offered by the ISIs, and informal gatherings (e.g., the Quranic study groups) generally exclude the poor, becoming vehicles of middle-class socializing and networking. The poor reach out for help wherever they can find it, regardless of ideological persuasion, including services offered by nonreligious and nongovernmental organizations. Clark's thoughtful study is an important contribution to the study of Islamic activism.

Mohammed Hafez admits that the title of his book, *Why Muslims Rebel,* is meant to echo Ted Gurr's *Why Men Rebel* (1970), but its main purpose is to challenge the leading explanations (including Gurr's) as to the causes for violent actions in the Muslim world. The author rejects the prevailing general arguments that economic deprivation caused by failed modernization, impoverishment, and/or psychological alienation due to excessive westernization has led to Muslim rebellions. While psychological and socioeconomic factors may help in the understanding of Islamists' existence, they cannot explain rebellion. Instead, Hafez favors focusing on the political process with three strategies derived from the social movement approach: political environment, mobilization structures, and ideological frames. Institutional exclusion blocks channels for political participation and conflict resolution and, when combined with reactive and indiscriminate repression, leads to large-scale rebellion. Islamist movements adapt by forming exclusive mobilizing structures that intensify their loyalty to the members and shield them from state repression. Rebellion gives rise to loosely structured factions that either oppose each other or compete: "Like a shattered vase, it is not easy to put together an Islamist consensus around peace and reconciliation once the movement has been fragmented into numerous exclusive factions" (p. 144).

The strongest segment in the book is the discussion on the nature of ideological frames (Chapter 5). Here, the author utilizes a combination of arguments (including psychological) to explain violence against civilian targets. Antisystem frames become all-encompassing and polarizing; moral codes against killing are deactivated and inhibitions are removed. Ethical arguments, comparative justifications, and shifting the blame for violence to the victims sanction murderous acts, and violence is viewed as necessary for the good of the people. Here, ideological intransigences "solidify into collective identities" (p. 161) and reject any attempts at reconciliation, even when popular support has been lost.

Hafez's attempt to show the dynamics of the political process and his use of social movement theory (and other approaches) are thought provoking and offer the reader an alternative view of Islamic radicals' use of violence. His central argument is an important one and deserves serious attention. The author admits in several places that due to time and resource restrictions, he had to focus on two countries, Algeria and Egypt, both entrenched in violent Islamic movements. His desire to show the relevance of the framework to Islamic movements in other parts of the world and draw on comparisons leads him to a brief mention of Jordan, Tunisia, Pakistan, Kashmir, the southern Philippines, Chechnya, Tajikistan, and the Al-Qaeda.

While both the "ambition" (p. 204) and the intellectual desire to expand is understandable, it can also be distracting. Since the coverage is uneven, it causes crowding and tends to generate questions about the relationship between the framework and the unique features of the case. For instance, in the early discussion about state repression and political exclusion, after detailed cases of Algeria and Egypt, the reader is taken into a tour de force of Jordan, Pakistan, and Tunisia concerning possible variations on inclusion and exclusion. Jordan is praised by the author as having adopted inclusive policies regarding Islamists while maintaining control over mosques, associations, and charity groups, thereby developing a successful "carrot-and-stick" approach. The

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legality of Islamist groups was not curtailed, but electoral and legal manipulations limited their influence. The focus is on state strategy, yet one inevitably asks, what may have been the role of ruler legitimacy? To what extent did King Hussein's popularity (combined with strategy) impose limitations on violent groups? Similar questions arise for all of the briefly covered comparative cases. It is important to leave room to expand on unique features (e.g., political culture) as intervening variables enriching comparative studies. Hafez's rich framework and in-depth knowledge may have been better served by two books, one with comparative focus on Egypt and Algeria alone, and another a comparative study of different Islamist groups in different countries without concern for time constraints or resource and space limitations.

Islamic Activism, the volume edited by Quintan Wiktorowicz, has two general goals: the exploration of the dynamics, processes, and organizations of Islamic movements (variations on mobilization strategies, structural changes, etc.) and an attempt to test the explanatory power of the social movement approaches. Charles Kurzman's clever review of the social movement and Islamic studies provides helpful insights. Violence and contention, networks and alliances, and culture and framing make up the three parts of this valuable book. Part I focuses on case studies of Algeria, Egypt, Bahrain, and Hamas. Fred Lawson's informative overview of the 1994–98 Shi'i uprising on the island of Bahrain shows that both the government and the opposition continued to change tactics. New groups were progressively added (women, small shopkeepers, students, etc.), and the movement turned violent in late 1995. The phases of government response began with diffuse, soft, and repressive strategies that became more forceful, moving to selective brutality by forces loyal to the regime and ending with softer and more tolerant tactics isolating those on the fringes of the political spectrum. Hafez argues in previous writings that state repression and brutality increase or trigger violence by the activists. In the case of Bahrain, we have two phases of forceful government response, and it begs the question: Is selective brutality better because it diminishes public support and limits participation in the demonstrations?

Political opportunity structure, mobilizing structures, and cultural framing are offered in an attempt to explain Hamas as a social movement. In an interesting piece, Glenn Robinson shows how changes in the external environment led to specific changes in the available opportunities for Hamas. Mobilization was facilitated through mosques and social service institutions, as well as universities. Here, the implication is that the mobilizing structure of these welfare organizations has been political, not merely charity based, as Justine Clark states with her case studies of social work agencies. The cultural frame is simply a broader discussion of ideology, where Hamas sees Palestine as a religious endowment and Islam as a solution, and engages in anti-Jewish propaganda.

In Part II, Diane Singerman helps us think about the overall interrelatedness of the repression of conventional forms of political expression in the modern Middle East and the rise of informal networks as alternative venues of expression. In the midst of these complex and vague networks, collective identities are forged domestically and regionally. Benjamin Smith successfully reflects on the elusive and complex role of the bazaar as a collective protest movement. The bazaar responds to the economic policies of the state, and its members can separate religion from politics when dealing with Islamic theocracy. His conclusions reminded me of a shopping spree in Tehran's "black" bazaar for an Islamic headcover that was hard to find. The experience revealed everything one needs to know about the separation of religious precepts, politics, and economic interests. The Iranian bazaar (with variations across cities) remains a fascinating and unexplored phenomena. Jillian Schwedler's analysis aptly shows how the formally institutionalized Islah party in Yemen declined as a broader field of alliances changed the landscape of political opportunities. Separating regime-movement alliances from personal alliances among a broad range of elite helps us see how a strong religious-based party may be weakened.

Part III, and the last section, explores the culture and framing of Islamic activism. Carrie Rosefsky Wickham skillfully explores recruitment and outreach strategies among high school and university graduates in three urban lower-middle-class neighborhoods of Cairo. Initially, interests motivate the students as they get involved in low-risk activities in neighborhood mosques or Islamic student associations. The progression to riskier Islamic activism highlights the importance of ideas and how activism is framed as a "moral obligation" demanding student loyalty and self-sacrifice. Although "on the plane of ideas" is where the Islamists achieve "their greatest success" (p. 247), the individual student's experiences and values play a major role in leading them to riskier activism. Gwenn Okruhlik's work, like Wickham's, challenges our views of the political role of Islamists. The impact of the Islamist social movement on the government of Saudi Arabia has been a more open discussion, the introduction of reform facilitating a convergence of cultural frames with the potential that, in the future, the Islamic frame may turn into a nationalist one. M. Hakan Yavuz's timely study combines cultural contention, political opportunity structure, and economic reform in a discussion of the state of the Islamist movement in Turkey. Yavuz concludes that while economic liberalization has created greater opportunities for the construction of a new Muslim identity, it has not promoted unification resulting in proliferation of competing Islamic projects.

All three books cover divergent forms of Islamic activism in a variety of countries. They show us the following: 1) Ideology is an integral part of the study of Islamist activism. All the contributors engage ideology in one way

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or another, and while it may be true that at times interpersonal "bonds come first and then ideology" (Clark, p. 25), ideology remains a multifaceted phenomenon. The social movement approach helps us delve into the anatomy of ideology. 2) Either as a movement or as part of the political structure, Islamist groups are susceptible to change. 3) Social movement approaches are highly adaptive to a diversity of cases. Their fluidity can perhaps help us gain insight into the present (and often ignored) secular movements in the Islamic countries. 4) It is important to remain open to the need to augment the social movement framework with other approaches for richer insight and analysis (e.g., state–society, ethnic politics).

Separation, Assimilation or Accommodation: Contrasting Ethnic Minority Policies. By Terrence E. Cook. Westport, CT: Praeger Publishers, 2003. 216p. $64.95.

— Jessica R. Adolino, *James Madison University*

Terrence Cook's book in the main focuses on ethnic minority policy strategies across the globe. The author distinguishes three main policy modes regarding ethnic relations: separation, assimilation, or accommodation. The book revolves around a consideration of these three main modes and two subtypes of each policy option—for example, assimilation as a strategy of a stronger ethnic group and assimilation when initiated by a weaker minority group, usually a minority. In so doing, he presents a fairly exhaustive catalog of global ethnic relations.

In the introductory chapter, Cook briefly mentions several goals for this work: 1) presenting "a clear classification of the kinds of policy strategies" pursued by dominant and minority ethnic groups, noting that "good classification is especially needed to put this policy domain into clear comparative perspective" (p. xviii); 2) discussing three principal directions of policy as separation, assimilation, and accommodation, each distinguished by sponsorship by either the strong or the weak (pursued in Chapters 1 through 6); and 3) presenting "fresh ideas about how to get out of dangerous games of ethnic conflict" (p. xxi) (explored in Chapters 7 and 8). Cook himself maintains that his work's primary contribution is analytical; the reader will find, however, that in fact, the bulk of the book is more descriptive than analytical. The book suffers both from the absence of a logical explanatory framework and a lack of systematic social science analysis, and of the author's three stated goals, only the discussion of policy directions is adequately accomplished.

The weaknesses are apparent from the outset. The introductory chapter involves no discussion of any existing literature on ethnic minority policies to frame the author's analysis. Rather, in the opening pages, Cook presents a rather opaque discussion of human psychology, the purpose of which is unclear in the context of the large work. This psychological discussion is not referred to again in the book. Beyond briefly stated intentions, the introductory chapter does not include a clear and thorough elaboration of the author's thesis, the book's format, or the study's methodology. The monograph also suffers from his failure to provide at the end a systematic or comprehensive summary of the arguments made in the discussion of the six policy models, or a more generalized overview of trends observed.

As noted, six of the substantive chapters focus on specific ethnic minority policy strategies. These chapters suffer from the absence of both introductory and concluding comments and the lack of an overall explanatory framework. Each instead reads like a descriptive catalog of one particular ethnic policy strategy. Some readers may find it useful to be able to consider the cases briefly discussed in each of the six chapters in the aggregate, especially since the existing literature (which is not considered by the author in any way) tends to treat ethnic conflicts as individual case studies, rather than in a systematically comparative fashion. However, the case discussions both within and across the chapters vary in depth and detail; some are multiple pages long, others no more than three sentences (in one chapter there is a discussion of 28 secessionist movements, in another just a brief mention of numerous examples). This variation limits the reader's ability to reach conclusions across the cases and chapters; the analysis would have been stronger had Cook employed a common framework within and across his examination of these policy strategies. Further, he fails at the end of each chapter to analyze any potential relationship or draw any conclusions across the examples discussed. Thus, although the examples do illustrate the pattern on which he is focusing (e.g., "separating as regional autonomy or secession," or a "policy of aggressive and collective assimilation") and the reader can intuitively make some connections across the cases, Cook himself never systematically draws conclusions or makes his own argument about the situations described. Hence, at the conclusion of these six chapters, we are no more able to explain why these situations occur, where they are most likely, or how prevent them then when we began the book.

In the final two chapters, the author makes a dramatic departure from the previous six and presents two hypothetical discussions of ethnic conflict resolution via the development of game theoretic models. In Chapter 7, he considers the Kashmir conflict between India and Pakistan from the perspective of a zero-sum game. In Chapter 8, he attempts to construct a more generalized model, a transformational or transgenic game theory, as he calls it, as a means for resolving ethnic conflicts. Although the discussion here is argued to be intended for the general reader, its logic is difficult to follow, and its significance relative to the book overall is hard to discern. These two chapters catch the reader off guard. Indeed, there is no explicit reference to any of the six models presented in the earlier chapters.

It is not immediately apparent who will find this monograph most useful. The discussion of ethnic minority policy strategies is not systematic, and there is no attempt to draw

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meaningful explanatory conclusions in reference to it. The strategy discussions are, however, interesting because there is some attention paid to lesser-known ethnic conflicts and because the coverage is generally comprehensive in a global sense. In this regard, those seeking to develop a broad sense of the kinds of ethnic policies pursued by dominant and minority ethnic groups across the globe may find this work a useful starting point. However, upon reaching the final pages of *Separation, Assimilation or Accommodation,* the reader is left with little of substantive value. Depending on their expertise, they may have some greater sense of the range of ethnic conflict policies witnessed around the globe, both historically and contemporaneously. But regardless of their expertise, they will have found no means for tying these policies together or conclusions to embrace (or disagree with). Overall, there is simply too little holding the individual parts of this book together, and ultimately it is difficult to discern a central argument in Cook's work. Those interested in a theoretically based and systematically comparative analysis of ethnic minority policies will find other volumes better suited to their purposes.

Minority Ethnic Mobilization in the Russian Federation. By Dmitry P. Gorenburg. New York: Cambridge University Press, 2003. 312p. $75.00.

— John B. Dunlop, *The Hoover Institution*

Dmitry Gorenburg has written a most useful book on the subject of minority ethnic mobilization within the Russian Federation during the Gorbachev and early Yeltsin periods. Gorenburg sets out clearly the aims underlying his ambitious study. The book, he writes, "seeks to explain how state institutions affect ethnic mobilization," through a focusing on the upsurge of nationalist sentiment that took place in both the Soviet Union and Eastern Europe during the late 1980s and early 1990s (p. xi). In addition, the book also intends to challenge the widely held perception that "governing elites can kindle latent ethnic grievances virtually at will" and "to shift the study of ethnic mobilization from the whys of its emergence to the hows of its development as a political force" (p. xi). (It is unclear, at least to this reviewer, why the "whys" are not equally as important as the "hows": Both questions, one would think, need to be carefully addressed by specialists.) In answering the "how" question, Gorenburg argues that "The nature of these processes . . . is determined by the ethnic and political institutions established by the state" (p. xii).

To test his core hypotheses, Gorenburg focuses upon four ethnic regions (autonomous republics) of the Russian Federation: Tatarstan, Bashkortostan, Chuvashia, and Khakassiya. Why were these four regions chosen? "The four were selected," he explains, albeit somewhat vaguely, "because they differ along institutional, economic and cultural lines" (p. 19). Presumably, such words could be applied to virtually all of the autonomous republics within the Russian Federation. Since both Chuvashia and Bashkortostan share a border with Tatarstan, one can understand why these three republics might have been selected by Gorenburg. (Tatarstan and Bashkortostan are, of course, considered "Muslim" republics, while the titular people of Chuvashia are deemed to be "Orthodox Christians.") But "shaminist" Khakassiya, with its small percentage of titular people (11% in 1989), is, by contrast, located a considerable distance away from those three regions, to the southeast. If the author wanted to select a "non-Muslim" region for comparative purposes, why did he not choose, say, either Tyva or North Osetiya, which, in my view, represent more interesting southern republics? The point I am making here is that he should have spent more than one sentence explaining why he chose the republics that he did.

The author describes the extensive fieldwork that he conducted in the four selected regions during the period from April 1995 through March 1998. He spent a total of 12 months in the field. While performing this fieldwork, he conducted interviews with nationalist activists, government officials, and local scholars; utilized archival materials describing the development of ethnic institutions in the regions; and carried out a content analysis of the local press in the four republics. He also analyzed electoral support for nationalist candidates, the size and frequency of public protests, and responses to polls and surveys conducted by social scientists. He did not sponsor any public opinion surveys himself, but he did assemble a newspaper database focusing on one newspaper in each region for the period 1988–93 (pp. 18–19, 121).

Somewhat surprisingly, most of what Gorenburg writes in his book is not directly underpinned by the fieldwork that he conducted, though that work obviously assisted him in arriving at his conclusions. More often than not, he relies on earlier pathbreaking research performed by others, in particular on "data from Western and Russian social science surveys conducted in 1993" (p. 119). In this regard, he makes extensive use of a national database of protest events that was compiled by Mark Beissinger, as well as of a chronology of "ethnic events" compiled by Galina Komarova of the Institute of Ethnology and Anthropology in Moscow. He also makes abundant use of an ambitious preelection survey conducted by Timothy Colton and Jerry Hough in 16 autonomous republics of the Russian Federation in November and December of 1993, as well as of a January 1994 survey carried out by the Institute for Socio-Political Research in Moscow. Finally, he utilizes an important survey on language and nationality conducted by David Laitin and Jerry Hough in Bashkortostan and Tatarstan in 1993. Gorenburg summarizes the results of these pioneering surveys as they apply to the four republics he is studying in a number of useful tables provided in his book. It should be noted that all of these 1993 surveys were conducted some two to five years earlier than his own fieldwork.

On one point, Gorenburg appears at times to reflect the widespread optimism and naïveté characterizing the work

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of a number of U.S. academics during the 1990s. Thus, he contends that the four republics he is studying were undergoing a process of "democratic transition" (p. 2) and maintains rather sweepingly that "the interaction between mass-based nationalist movements and local political elites has important implications for the future of center-periphery relations in the Russian Federation, foreshadowing a time when the forces of civil society will be able to constrain the policy opinions available to the governing elites" (p. 257). Under President Vladimir Putin's program of "strengthening the vertical," the role of civil society in Russia has, of course, been intentionally and significantly reduced.

It is interesting to note that toward the conclusion of his study, Gorenburg speculates on what the fate of the Russian regions would have been "had the Soviet state followed the Turkish route and refused to admit the existence of ethnic minorities within Russia" (p. 270). It is likely, he concludes, "that its subsequent efforts at Russification would have been even more successful than they were." This appears to be the conclusion that has also been drawn by the Putin leadership, which may yet seek to inculcate something resembling the "Turkish model" into Russia.

Gorenburg believes that his study "has important implications for explaining ethnic mobilization in other parts of the world," in addition to the former Soviet Union. There are, for example, he notes, "remarkable parallels between the mobilization process among indigenous ethnic groups in several countries [of Latin America] and ethnic mobilization in the Russian Federation" (pp. 261–62). While perhaps exaggerating the global significance of his research, Gorenburg is nonetheless correct, in my opinion, to highlight the potential value of his book for students of ethnic mobilization on an international scale.

The Politics of Property Rights: Political Instability, Credible Commitments, and Economic Growth in Mexico, 1876–1929. By Stephen Haber, Armando Razo, and Noel Maurer. New York: Cambridge University Press, 2003. 408p. $75.00 cloth, $29.99 paper.

— Adam L. Resnick, *Western Washington University*

Stephen Haber, Armando Razo, and Noel Maurer provide a detailed, theoretically rich examination of the interplay between political chaos and economic growth in Mexico. Why did Mexico's economy continue to grow through a series of civil wars, coups, and outside interventions? More generally, what conditions lead to robust growth under political instability? Political scientists, economists, and others have searched for, and failed to find, a consistent link between political instability and economic failure. These authors develop an argument on the conditions under which instability and robust economic growth go hand in hand, using the years before, during, and after Mexico's chaotic revolution as their case study.

The state's propensity to trample on property rights lies at the center of the discussion: If economic actors live in constant fear that their assets will be seized, they will not invest, they will not do business, and a country's economy will not grow. Investors prefer a stable democratic system with rules and institutions offering credible property-rights protections in the long run. Absent stable democracy, investors will settle for a stationary bandit, a dictator with a long-term stake in the economic success of the country. The bandit's actions are restrained by a desire to reap maximum rents through taxation, remuneration the dictator will reduce if he or she seizes assets to the point that they deter investment, and thus taxable revenue. We would expect instability, a series of short-term dictators punctuated by chaos, to produce inferior results. These frequent changes in rulers and rules *should* cause capital flight and mattress stuffing, leading to slow economic growth or even economic contraction. Haber, Razo, and Maurer point out that such instability often fails to strike fear into the hearts of economic actors, who befuddle social scientists by driving economic growth through decades of on-and-off political disorder. The key to sorting out this puzzle, and the main focus of this book, is to illuminate the conditions under which property rights remain secure over multiple political regimes of varying terms and characters.

The book's first chapter provides a focused, enlightening discussion of existing arguments on the interplay of regime type, political stability, property rights, and growth. The second chapter turns to the theoretical arguments. The authors point out that states need not provide property-rights security across the board but can achieve positive economic growth by guaranteeing these rights selectively. "Vertical political integration" (VPI) (p. 29) is crucial to the provision of these rights across changing governments, and thus to economic progress under instability. VPI involves a coalition of rent-seeking asset holders, a weak government that extracts rents from these asset holders, and a third-party enforcer that receives a payoff for protecting asset holders from the state. VPI's positive effect on property-rights protection is enhanced by three conditions: 1) Asset holders' technology or markets cannot be easily expropriated; 2) revenues from these asset holders are crucial to the survival of the government; and 3) asset holders are organized enough to shut down collectively, cutting off revenue to the government. The central argument of the book thus boils down to this: Property rights will be selectively provided over unstable periods when a government–private sector coalition receives property-rights guarantees from a stable third party and where a succession of weak, dependent governments cannot successfully expropriate assets.

The bulk of the book applies these theoretical arguments to a variety of economic sectors. Each chapter evaluates the degree to which VPI in Mexico allowed robust growth through periods of stable dictatorship, chaotic revolutionary war, and a disorganized series of autocratic

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regimes. The chapter on finance highlights how weak post-revolutionary governments ensured the property rights of banks and investors by allowing them to "write the rules governing their activities" (p. 122). Government commitment not to expropriate assets was ensured by a stream of revenue to key politicians, and also by the integration of these politicians into the banking sector. These relationships were durable, reemerging after a revolutionary-era slowdown and helping to restore economic growth. Subsequent chapters address industry, petroleum, mining, and agriculture, providing detailed accounts of the economic and political ups and downs of these sectors. Third parties are often key to supporting the private sector's property rights, such as U.S. government advocacy for its oil companies and labor unions' using their leverage within government to protect private industry from government predation. The conclusion chapter summarizes the relevance of the study along disciplinary lines, with sections for history, political science, and economics.

This work provides a rich story of how property rights remained secure for much of Mexico's key economic actors over widely shifting political terrain. The central insight is its highlighting of the types of state–market relationships that avoid predation under instability. Such an alignment of forces may even provide better property-rights protection than more stable arrangements. For instance, the authors point out that the introduction of political stability in the late 1930s opened the door for the expropriation of the oil industry, a predatory act that weaker states in unstable times were unable to pull off. Such insights are welcome, as is the well-documented journey through the political economy of Mexico.

The Politics of Property Rights has a few shortcomings. The authors have gone to great lengths to provide quantitative data and other evidence to tell their story, but many will wonder why they did not advance their argument on a much more recent case where such limitations would be rarer, one that might yield more lessons for contemporary policy. The authors suggest that their framework is generalizable, but they could go further in offering examples of other cases that displayed VPI and grew, or cases that do not display VPI and did not grow. Overall, however, the authors have provided a readable and well-documented work that helps to unpack the difficult instability-growth puzzle.

Towards Juristocracy: The Origins and Consequences of the New Constitutionalism. By Ran Hirschl. Cambridge, MA: Harvard University Press, 2004. 294p. $49.95.

— Stephen Levine, *Victoria University of Wellington*

To the various systems of government known to political science we may now add another—"juristocracy." Although not rigorously defined in this book, the term refers to a process of judicial empowerment in which judiciaries take on powers and responsibilities previously exercised by representative institutions. While there is little new in recognizing that courts often make decisions with profound political consequences, this study considers recent transfers of power to judiciaries to be unprecedented in their breadth and scope, features of what the author describes as "the new constitutionalism."

While many scholars, commentators, politicians, and members of the public tend to look favorably on the development of new opportunities for judicial intervention, this study adopts a more skeptical view. It is, of course, an essential element of academic research for received truths to be examined in the light of new information. In this case, Ran Hirschl has chosen to use comparative methods to examine whether constitutions, bills of rights, and judicial review achieve the goals and purposes so often ascribed to them.

The broad theme of *Towards Juristocracy,* that governing elites initiate and support delegations of power to the judiciary in order to preserve their own hegemony, is tested through an examination of the origins and consequences of recent constitutional developments in Israel, Canada, New Zealand, and South Africa. In each case, though admittedly in vastly different circumstances, public and parliamentary doubts about the value of bills of rights and judicial review were ultimately overcome. This study emphasises common patterns of causality and consequence in each case, reviewing each country's recent constitutional history, the content and character of its charter of rights, and the broad tendencies to be found in its constitutional jurisprudence.

Although the outlook is broadly comparative and the language often theoretical, the principal propositions derive largely from the author's understanding of the origins and consequences of judicial empowerment in the State of Israel. The point of departure for this study, therefore, lies with a critical assessment of the Supreme Court of Israel, an institution emboldened to issue often far-reaching judgments on a whole range of matters. As Hirschl shows, many of these decisions coincide closely with the interests and ideological preferences of elite groups whose political power had otherwise begun sharply to decline.

In another sense, therefore, the methodology involves a series of case studies—Canada, New Zealand, and South Africa—designed to test whether the conclusions drawn with respect to Israel and its Supreme Court have a wider application. The stakes would appear to be high, for notwithstanding the apparently high regard in which judges and judiciaries are commonly held, this book argues that juristocracy represents "a new political order," one that "has been rapidly establishing itself through the world" (p. 222) and has less to do with the establishment of a more just and progressive society than with the consolidation of power by elites desperate to preserve their dominance. On this reading, moves to develop justiciable bills of rights, and to take courts to topics ("political questions") that they would have been reluctant to address in the past, has little if anything to do with a devotion to human rights, with the author instead seeing efforts to entrench neoliberal, small-state policies and perspectives by placing them outside the reach of newly emerging majorities.

There is, perhaps, something of a puzzle about recent tendencies toward the development of more robust systems of judicial review. It is less clear, however, that these developments reflect the ability of "elites" to disguise their own self-interested agendas by associating themselves with movements to enshrine human rights and constitutional review mechanisms into national and international law and jurisprudence.

While Hirschl says that it might require several lifetimes to test some of his propositions, a few are more quickly dispatched. There is no mystery in his discovery that the New Zealand judiciary has made little use of the country's Bill of Rights in order to bring about a progressive redistribution of resources, since, as he has already noted, the document was deliberately designed to exclude enforcement by the courts of economic or social rights. While it is useful to see whether recent Israeli experience with judicial review has analogues in other parliamentary democracies, excessive reliance on the "elite hegemony" thesis introduces distortions, with unrelated developments linked up in at times inappropriate ways. New Zealand's "economic elite," for instance, is credited with engineering New Zealand's "joining multilateral economic groups such as . . . the South Pacific Forum" (p. 83), misrepresenting both the Pacific Islands Forum (as it is now known) and the circumstances behind its establishment in 1971. The author confuses "the libertarian New Zealand Party" (p. 85), which effectively contested only one election, 1984, and which never won a single parliamentary seat, with the populist New Zealand First Party, which was founded in 1993 and won the third-highest number of seats in Parliament at the elections of 1996 and 2002.

It is surprising to see pivotal Treaty of Waitangi decisions by the Court of Appeal in 1987 and 1989 overlooked, as it was these that brought the treaty to the forefront of the country's jurisprudence and placed real limits on parliamentary sovereignty. This is somewhat unfortunate for the author's argument, as these decisions preceded the adoption of the Bill of Rights Act. It is difficult to accept that that legislation was responsible for making the New Zealand Court of Appeal "a central forum" (p. 193) in dealing with Maori grievances when the act has, in fact, had little to do with the emergence of a more treaty-centred polity. An analysis linking the drafting of a Bill of Rights for New Zealand with "the very same politicians who introduced comprehensive neoliberal economic reform" (p. 87), and with an "oligarchy of wealth and political power seeking to preserve [its] hegemony" (p. 89), simply lacks credibility.

Although often repetitious—its core propositions are restated at regular intervals—the author has performed a useful service by encouraging commentators to look more carefully at the actual outcomes of measures portrayed by their proponents as enhancing constitutional democracy. It is legitimate to enquire whether constitutions and charters to define and defend a people's rights really do work that way in practice, or whether by accident or design they come to serve other motives and purposes.

Hirschl would not be the first to write a book evincing a distrust of the legal profession. His agenda, however, is concerned with the skill with which unrepresentative judicial elites, advancing their own interests, ideologies, and policy programs, have managed to accumulate new powers, infiltrating governance structures at national and supranational levels. His most sweeping recommendation, that proponents of social, political, and economic change need to look elsewhere than to the courts for genuinely progressive and redistributive transformations, warrants serious consideration.

Authoritarian Legacies and Democracy in Latin America and Southern Europe. Edited by Katherine Hite and Paola Cesarini. Notre Dame: University of Notre Dame Press, 2004. 360p. $60.00 cloth, $30.00 paper.

— James Mahoney, *Brown University*

In recent years, scholars have turned their attention to the role of antecedent authoritarian regimes to understand the many new democracies that have emerged around the world. This volume seeks to contribute to this discussion by introducing the concept of "authoritarian legacies" as a new analytic lens. The goal of the volume is specifically to show how a focus on authoritarian legacies offers fresh insights into the workings of contemporary democracies in Southern Europe and Latin America.

While the idea of authoritarian legacies is quite promising as a tool for structuring analysis, the concept also presents some important challenges. One challenge concerns its definition. In their introductory chapter, editors Katherine Hite and Paola Cesarini opt for an encompassing definition: "Authoritarian legacies are those rules, procedures, norms, patterns, practices, dispositions, relationships, and memories originating in well-defined authoritarian experiences of the past that, as a result of specific historical configurations and/or political struggles, survive democratic transition and intervene in the quality and practice of postauthoritarian democracies" (p. 4). This definition has the merit of including a broad range of phenomena that may carry over from the past, including phenomena that are altered in the process of the transition to democracy. However, one might legitimately ask if the definition is too broad. Does the definition include nearly everything from the past that negatively shapes the quality of democracy? What are examples of important legacies that are not "authoritarian legacies"?

Another key challenge concerns the role of authoritarian legacies in explanation. Because they are by definition remnants from the past that affect democracy, these legacies

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cannot be used to *explain* levels of democracy without risking tautology. Rather, authors must draw on authoritarian legacies as hypothesized causes of outcomes that are analytically separate from democracy (e.g., market reforms) or as outcomes to be explained (i.e., as dependent variables).

The substantive chapters roughly follow the editors' useful distinction of three types of authoritarian legacies: (1) formal rules, (2) specific actors, and (3) cultural practices. Much of the literature on old regime legacies has focused on the first category of formal rules, such as antecedent constitutional provisions, party regulations, and electoral guidelines that carry over to the new democratic era. Although some of the chapters touch on these kinds of formal rules, they are not the main emphasis of the book.

By contrast, authoritarian legacies in the form of concrete actors—especially the military and the police—are the focus of several chapters. Felipe Agüero argues that the strength of the military at the moment of democratic transition shapes long-run military–civilian relationships. For example, compared to Southern Europe, South America has not been successful at imposing democratic standards on the military. Agüero traces this difference and other variations within and across the two regions to the extent to which the military was able to control the transition process. Anthony W. Pereira and Mark Unger's nicely presented analysis of policing and the persistence of *mano dura* (iron fist) in Brazil and the Southern Cone argues that many problematic police practices developed after the transition to democracy, and thus that they are *not* authoritarian legacies. For instance, police atrocities such as corruption, brutality, and discrimination are often *post*-authoritarian developments. Better candidates for authoritarian legacies are long-standing institutional problems, such as the imperfect separation of the police and the military in much of Latin America.

Several of the chapters also focus on authoritarian legacies in the form of cultural practices, lived experiences, and psychological dispositions. These chapters often explore the degree to which cultural schemas are open to reconstruction by political leaders. For example, Cesarini emphasizes the role of agency when analyzing the first democratic leaders in post–World War II Italy and post-1983 Argentina. She argues that these leaders had considerable discretion in creating particular "myths of refounding," and that these cultural myths in turn greatly influenced social memories, citizen trust in government, and the quality of democracy. By contrast, Consuelo Cruz suggests that Latin American democracy is impeded by a more deeply ingrained elite bias against political freedom. She argues "that the project of individual rights was postponed and marginalized in Latin America by historical rhetorical practices and discursive formations that privileged organic identities and paternal authority" (p. 316). This kind of authoritarian legacy, Cruz argues, can only be overcome through a basic transformation of Latin American culture.

In an excellent chapter, Frances Hagopian brings together a broad range of authoritarian legacies to explain the extent and timing of market reforms in South America. She puzzles over such questions as why Brazil was a laggard in economic reform when compared to Argentina, and why neoliberal reforms were not reversed in democratic Chile. Her answers stress the ways in which multiple political and economic authoritarian legacies shape the relative power and negotiating strategies of key actors, including both political elites and actors from civil society. One major finding is a suggestive negative correlation between the strength of the labor movement and the extent to which market-oriented reforms are pursued and maintained.

Overall, the volume succeeds at using comparisons to systematically describe how the past intervenes on the present. Indeed, all of the chapters are comparative, and most of them engage in broad and stimulating cross-continental comparisons. As a shortcoming, however, the volume is perhaps less successful at using the small-n comparisons to tease out causal patterns and actually explain major outcomes. This limitation sometimes manifests itself in the "kitchen sink" problem—that is, the inclusion of an unwieldy range of explanatory factors that are regarded as important. For example, Hite and Leonardo Morlino's otherwise interesting chapter on "good democracies" examines the effect of several contextual dimensions on authoritarian legacies, but ultimately concludes "that there is no necessary consistency between contextual dimensions and legacies themselves" (p. 73). Paloma Aguilar and Hite's thoughtful analysis of the sources of political justice in Spain and Chile arrives at a similarly open-ended conclusion, that "there does not seem to be one approach with sufficient explanatory efficacy that allows us to recommend it here" (p. 222). Even Hagopian notes that her analysis could be criticized for embodying a path of causation that "is too long and blurry" (p. 100). In this sense, the strength of *Authoritarian Legacies and Democracy in Latin America and Southern Europe* rests more with its comparatively informed and richly contextualized historical discussions than with its formulation of parsimonious explanations.

Fujimori's Coup and the Breakdown of Democracy in Latin America. By Charles D. Kenney. Notre Dame: University of Notre Dame Press, 2004. 379p. $60.00 cloth, $30.00 paper.

— Carol Wise, *University of Southern California*

In this book on Peruvian politics, Charles Kenney leaves few stones unturned in analyzing the various factors that led to the suspension of the country's constitution and the disbanding of the legislature by President Alberto Fujimori in April 1992. While I am not entirely convinced that this particular episode in Peruvian politics warrants the book-length treatment that it has received here, Kenney has done a commendable job of locating Peru's "autogolpe" (or civilian-inflicted coup) within the broader literature on the breakdown of democratic regimes in Latin America.

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Given that the book sticks closely to this literature, rather than deepening our understanding of the determinants of democratic breakdown in this region, there is nothing new or compelling here. Instead, Kenney's main contribution is that of a thoughtful, if telescopic, analysis of one developing country's struggle to construct and defend a democratic regime under highly adverse domestic conditions. In remaining true to the literature from which he has drawn, the author focuses on the design and nature of the country's political institutions and, in particular, on the difficulties that Peru's presidential system and volatile multiparty organizational structures posed for Fujimori when he took office in 1990.

The gist of Kenney's argument is that within this dicey political context, Fujimori's lack of a loyal majority coalition in the legislature proved fatal for the survival of democracy as Peruvians had known it since the transition from military rule in 1980. As he argues, "certain institutional features of democratic presidential regimes are associated with their survival and demise. In Peru, minority presidencies were an important risk factor for the survival of democratic and semi-democratic regimes during the twentieth century" (p. 262). Conversely, democracy has done better when elected executives within a presidential system like Peru's are able to forge and control a majority support coalition in the legislature, such as under the two separate administrations that were elected there during the 1980s.

The author advances this thesis via a three-part discussion of the collapse of Peru's multiparty political system and the quixotic rise of Fujimori to the presidency in 1990; those institutional rules and political coalitions that underpinned the executive office and the relationship of the executive vis-à-vis the Peruvian congress; and a detailed analysis and interpretation of the autogolpe itself. A final chapter offers a dozen or so brief country sketches that basically describe the conditions under which democratic regimes collapsed in Latin America in the time span between 1960 and 1997. Ostensibly meant to lend concluding insights with a comparative angle, this final chapter is ill advised in that it accomplishes neither of these goals.

With respect to these three main themes, let me highlight some of the book's stronger points. Kenney's analysis of the complex cluster of factors that contributed to the unraveling of Peru's political parties in the late 1980s is both sophisticated and convincing. On this count, he skillfully shows how the dark-horse victory of Fujimori in the 1990 presidential race was intricately linked to the failures of the traditional political establishment. Moreover, Kenney aptly observes that although "Fujimori was initially the product of the collapse of support for the 1980s parties, once in office, he did much to undermine public support for parties, and the complete collapse of the party system came only as a consequence of his success as an antiparty president" (p. 77).

As for the complicated contingencies that both preceded and provoked the 1992 autogolpe, Kenney offers a synthetic portrait of the factors that led to the breakdown of democracy in Peru at this time. In doing so, he avoids a simplistic cause-and-effect explanation and instead tackles the coup's origins in their entirety. Although the analysis hinges on the ineffective design of political institutions and the growing stalemate in executive/legislative relations, he incorporates other important contributing variables into his analysis. For example, the incipient signs of economic recovery in early 1992, the still-raging guerrilla insurgency that had plagued the country since 1980, and Fujimori's growing preference for military advisors are all factored into Kenney's explanation.

In approaching the actual outbreak of the coup, the author sharpens his explanatory focus on the tensions between Fujimori and the legislature, which he portrays as erupting in a series of stages between July 1990 and April 1992. During this period, the following dynamics took a cumulative toll on Peru's fledgling democratic regime: "Elected to the presidency in a highly polarizing election, with a minimum of organized party support in a fragmented legislature, Fujimori faced what some analysts considered an almost impossible game" (p. 248). In other words, Fujimori's autogolpe was both preemptive and proactive in that his failure to rein the situation in could have led to his own downfall.

Overall, although Kenney has readily accomplished the goals he sets forth at the outset, his microlevel treatment of Peruvian politics makes it tough to wade through some parts of the book. Moreover, in his effort to thoroughly situate these themes within the ongoing academic debates, Kenney does too much equivocating and weighing of the explanatory options. When he does take a firm stand, the book's own detail towers over the main punch line: "minority presidencies are associated with higher levels of democratic instability than are majority presidencies, both when the economy grows and, especially, when the economy fails" (p. 268).

For those interested in the reform options and political path forward for Peru and other countries similarly plagued with suboptimal political institutions, these larger debates are not taken up here. Neither does the author offer a concrete sense of those institutional contingencies or combination of rules and norms that might qualify a developing country's political regime as a "success." In the end, I suspect that *Fujimori's Coup and the Breakdown of Democracy in Latin America* will appeal mainly to those area specialists and policy analysts with a keen interest in Peru.

Race and Regionalism in the Politics of Taxation in Brazil and South Africa. By Evan S. Lieberman. New York: Cambridge University Press, 2003. 344p. $75.00 cloth, $24.99 paper.

— Patrick Heller, *Brown University*

Evan Lieberman has produced a first-rate work of comparative political economy. Just as importantly, he has done so by going boldly (and engagingly) where so few have gone

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before—into the tax state. Given how critical the capacity of a state to tax economic elites is to the provision of public goods, redistribution, and the promotion of development in general, it is indeed shocking to realize just how little attention this question has received from political scientists (there being as always some notable exceptions). Lieberman sets out to correct this gap not only by carefully and meticulously defining and measuring different tax states but also by providing a rich historical and comparative account of the rise and consolidation of the two very different tax states of South Africa and Brazil.

As the author shows, South Africa has what he calls a *cooperative tax state,* one that is capable of securing significant tax revenues from a comparatively compliant dominant class. Brazil, on the other hand, is saddled with an *adversarial tax state,* in which dominant interests assiduously resist taxation. The result is that the tax structure in South Africa is efficient and progressive (with the largest portion of taxes coming from income and property), whereas the Brazilian tax state is inefficient and regressive (with the largest share coming from consumption taxes and social security contributions). Locating his analysis squarely in the Joel Migdal-Atul Kohli-Vivienne Shue state-in-society paradigm (*State Power and Social Forces,* 1994) he in effect paints a vivid contrast between a highly synergistic state–society relation (South Africa) and a highly disarticulated state–society relation (Brazil).

Of course, Brazil and South Africa are very different cases, but Lieberman makes a sound case for the comparison, noting that both countries have similar colonial histories and parallel trajectories of economic development through import-substitution industrialization. In explaining the divergent patterns of tax state building, he methodically dispatches a range of contending explanations (tax culture, relation to global economy, basic state capacity, regime type, etc.) and instead traces the origins of the tax state to constitutional junctures at the turn of the century. The critical junctures are important because they in effect give institutional form (and great durability) to different national political communities (NPCs)—the "historically rooted institutions which give political salience to certain group identities and not others" (p. 10). In the case of South Africa, class cohesion and a resulting willingness to tax and be taxed is cemented by white solidarity and exclusion of the black majority. In Brazil, the powerful forces of regionalism and the fact that race never becomes a source of political mobilization produce a fragmented polity in which dominant interests actively resist the federal state's many attempts to impose higher rates of income and property taxes. The argument is effectively bolstered by brief but insightful analyses of other cooperative and adversarial tax states, including a nicely constructed statistical model.

If the argument is developed with exemplary rigor, one cannot help but wonder if the concept of a national political community is not just old wine in a new bottle. The actual *mechanism* that drives state–society relations in Lieberman's argument is intra- and interclass cohesion. If the dominant class is cohesive and confident and recognizes the basic interdependency of class interests, then it is willing to make short-term sacrifices (pay taxes) for long-term gains (public goods, order, labor quiescence). This is the essence of what Antonio Gramsci called hegemony, a form of class politics in which dominant classes concretely coordinate their interests with subordinate classes. Similar arguments have been made about the encompassing interests of the working class as the basis of high-capacity social democratic states (e.g., Adam Przeworski, *Capitalism and Social Democracy,* 1985). Lieberman undoubtedly knows this but wants to argue that the NPC concept is novel because its takes identity and institutions seriously. But then have not all serious scholars of the history of class formation (the process of going from the defense of narrow to encompassing interests) taken institutional context and identity seriously, beginning with Gramsci's focus on ideology (political construction) and, more recently, social history perspectives on class (e.g., E. P. Thompson, Ira Katznelson, and many others)?

The point here is not, moreover, just about theoretical lineage. Highlighting how the dynamics of class formation shapes political economies might also have helped correct the two weak points of the book. First, in his determination to make race and regionalism matter (and they certainly do matter), Lieberman loses sight of the equally determinant effect of the socioeconomic origins of capitalism in South Africa and Brazil. South Africa is the story of mining capital in search of cheap labor. The solution to the labor problem was the creation of a centralized and coercive state that could uproot the African peasantry and mobilize wage labor. Race became the key organizational construct. In Brazil, a plantation economy produced a far more paternalistic and clientelistic regime of labor control, one that relied on the local equations of power and dependency, including race relations that were politically obfuscated even as they were socially deployed. The relative significance of race and regionalism, and the resulting patterns of dominant class formation, thus have their roots in the colonial economy and its particular configurations of social power. Second, as Lieberman extends his analytic insights into the present phase of democratic transformations, he makes a strong case that a regressive and inefficient state tax remains a significant obstacle to change in Brazil, but is on much shakier grounds when he argues that the cooperative tax state should serve South Africa well. If it is indeed the case that the "post-apartheid state has better-than-average capacities to ameliorate income and wealth disparities" (p. 278), the posttransition class configuration may very well have trumped this advantage. To many observers, the ruling African National Congress increasingly represents a new alliance of emerging black elites and the white business community, marked most

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notably by the ANC's abandonment of its historical commitment to using the state as an instrument of redistributive justice and its embrace of neoliberal economic policies (including fiscal austerity and the commodification of public services). If race is on the decline, class is on the rise, and patterns of income inequality and social exclusion have actually increased over the past decade.

Be this as it may, *Race and Regionalism* is an invaluable contribution not only to the much neglected topic of the tax state but more broadly to an understanding of the complex political and institutional patterns that shape the possibilities of development in the semiperiphery. This book is essential reading to all students of comparative political economy.

Democracy Delayed: The Case of Castro's Cuba.
By Juan J. López. Baltimore: Johns Hopkins University Press, 2002. 272p. $42.50.

— William M. LeoGrande, *American University*

When the Berlin wall came down, many Cuban-Americans eagerly anticipated the imminent fall of Fidel Castro's regime in Cuba. "Next Christmas in Havana," read the bumper stickers in Miami. But most scholars studying Cuba doubted that Castro would be so easily dislodged. They cited manifest differences between Cuban and European communism: Cuba had an authentic revolution that began with broad support, whereas communism arrived in most of Eastern Europe in the rucksack of the Red Army. Cuban nationalism bolstered the legitimacy of a government in conflict with the United States, whereas European nationalism corroded the legitimacy of regimes beholden to Soviet Russia. The standard of living in communist Europe paled in comparison to that of the West, whereas Cuban conditions compared favorably to much of Latin America and the Caribbean. European communist regimes were led by colorless bureaucrats who had long since lost faith in their own ideology, whereas Cuba was still led by the charismatic Fidel Castro and the generation that made the revolution.

Juan J. López also wants to explain why there was no transition in Cuba by comparing it to communist Europe, but he sets himself the task of proving these scholars wrong. Castro's survival was not the result of these oft-cited conditions, he argues. Castro could have been overthrown in the early 1990s and could still be overthrown today. Conditions prevailing in Cuba are not fundamentally different from those prevailing in communist Europe in 1989. The critical difference, according to López, is that Washington has not given adequate support to Cuban dissidents.

This is a hard case to make, since the theories that foresaw Castro's survival have predictive validity on their side. López tries to refute them by showing that each factor they cite as distinguishing Cuba from Eastern Europe was actually present to some degree in at least one of the European cases. From this he concludes that none of these factors can logically explain why Castro survived. But he examines each factor in isolation, rather than seeing them as a constellation of contributing causes, which is how most scholars view them. This mechanistic approach misses the possibility that the Cuban case might be explained by the interaction of several contributing causes not replicated in any one European case. The single conventional factor López thinks is valid in explaining Castro's survival is regime repression, yet this is the one dimension on which Cuba is most similar to communist Europe and, by López's logic, is therefore the least useful explanatory variable.

Why did Europeans suddenly rise up despite state repression, whereas Cubans did not? López argues that Cubans lacked a sense of political efficacy, a belief that action could effect change. Social movement theorists studying the European transitions have found that a growing sense of efficacy catalyzed the expansion of dissident activity from a few hundred people to hundreds of thousands. López may be right; perhaps all the necessary conditions are in place for a Cuban transition from below, all except this one missing piece. But while efficacy is a necessary condition for mass mobilization, it is not sufficient, and so the absence of mobilization is not evidence of a lack of efficacy. It is equally plausible that Cubans have not mobilized against the regime because they prefer Fidel Castro's socialist welfare system to the likely alternatives; or because they fear that a transition might be violent; or because they fear the return of the largely white Cuban upper class that fled to Miami in 1959; or because they fear the reimposition of U.S. hegemony; or simply because the disaffected prefer exit to voice; or all of the above.

The only way to ascertain the subjective reasons that Cubans have not mobilized against their government would be to ask them, and this we cannot easily do. As an alternative, López relies heavily on a University of Florida survey of exiles and a survey of dissident organizations in Cuba. While acknowledging the limitations of this information, he argues that we have to make due with it because it is all we have. In fact, it is not all we have. A number of polls have been taken in Cuba over the past decade, an independent one by CID-Gallup for the *Miami Herald* and several by Cuban scholars. They have their limitations, too, of course, but at least they are representative samples. López relies uncritically on the dissident polls as an accurate gauge of Cuban opinion in general. As the CIA learned to its dismay at the Bay of Pigs, exiles are poor judges of public opinion in the homeland they fled.

Having concluded that only a lack of political efficacy stands between the Cuban people and democracy, López argues that only a lack of resources prevents the dissident movement from boosting the population's sense of efficacy. He attributes this lack of resources to the unwillingness of the United States—President Bill Clinton in particular—to expand Radio and TV Martí broadcasts or to provide the dissidents with significant aid. "The Clinton administration actually sought to maintain the status quo in Cuba

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rather than promote a transition to democracy," he argues (p. xxviii). This is both unfair and untrue. U.S. officials did say, as López recounts, that a violent transition producing a migration crisis would not be in the interests of the United States. But that is a far cry from trying to preserve the status quo. Some Clinton officials sought to engage Cuba in hopes of promoting liberalization. Others favored continued economic pressure in hopes of hastening Castro's collapse. But all would have welcomed a Czech-style, nonviolent transition to democracy.

Unfortunately for the reader, López appears to have finished writing *Democracy Delayed* in 2000 because he does not mention either the Varela Project, whereby dissidents were able to mobilize more than 10,000 Cubans to sign petitions calling for a referendum on democracy, or the 2003 arrest and imprisonment of 75 dissidents on charges of having accepted aid from the United States (precisely the sort of aid López advocates). Varela's success indicated a degree of strength in the dissident movement that few observers expected, but the arrests demonstrated the regime's continuing ability to repress dissent without sparking broader mobilization.

López's contention that Washington has been soft on Castro and is therefore to blame for his survival may soon be put to the test. In May 2004, President George W. Bush announced that he intends to do exactly what López recommends—boost the signal strength of both Radio and TV Martí and expand U.S. support for Cuban dissidents. If that policy is carried out, we will see whether López's theory of transition in Cuba has as much predictive validity as those he criticizes.

The Transformation of Central Asia: States and Societies from Soviet Rule to Independence. Edited by Pauline Jones Luong. Ithaca, NY: Cornell University Press, 2004. 352p. $49.95 cloth, $22.95 paper.

— Terry D. Clark, *Creighton University*

The closed nature of the Soviet system meant that little was known about the republics on the periphery, particularly those of Central Asia. Most scholars viewed the region as a cotton colony of the Soviet Union dominated by a largely traditional, feudal society impervious to Western understanding. Despite greater access to the region and its peoples in the wake of the collapse of communism, the perception that the region remained under Moscow's shadow continued to suppress interest among nonspecialists. More recently, however, renewed interest in the Muslim world brought on by the war on terror has changed all of that. Specialists in international relations, security studies, and comparative politics, in particular, are intrigued by any number of questions, not the least of which are the persistence of authoritarianism and the inability of radical Islam to gain traction in Central Asia.

In an attempt to further lift the shroud, Pauline Jones Luong has assembled 10 chapters addressing state–society relations in postcommunist Central Asia. Her espoused purpose is to add to both our theoretical and empirical knowledge of the region. While she is more successful in achieving the latter goal, readers will find that the introductory and concluding chapters make a decent effort to address the theoretical implications of the volume's eight chapters. This is particularly so for the concluding chapter in which Luong addresses debates in the literature on the developing world that revolve around the unitary state versus the pluralist state, strong societies and weak states, and the role of the international community in developing states. The discussion is enlightening, but this reader was left wondering why she did not engage the literature on state and society in the Islamic world more directly and explicitly. Comparisons would be of particular interest to those concerned with the rise of radical Islam. Insofar as Central Asia appears to have substantially avoided the problem, what comparative differences in the region's state–society relations might have contributed to its having done so?

The introductory chapter engages in a careful critique of the dominant views of Central Asia during the Soviet era. The discussion is important to the volume's thesis. Eschewing a behavioralist/culturalist position, *The Transformation of Central Asia* essentially contends from a historical legacy perspective that the Soviet state transformed the region more fundamentally than previously thought, as a consequence of which the Soviet legacy is a much stronger causal factor than Islam in explaining most transformation processes in the postcommunist era. In so doing, it gave new meanings to existing symbols and imbued the population with secularist values. These values inform the relationship between state and society in postcommunist Central Asia.

Subsequent chapters address particular aspects of this thesis. While most focus on the effect of Soviet-era institutions, some highlight the effect of Soviet-era values on state–society relations. The general argument is that despite the new states' use of Islam to form a national identity centered on traditional values, the region remains largely secular and deeply suspicious of radical Islam. In this vein, Chapter 1 argues that women continue to look to the state for maternal support; and Chapter 2 argues that such traditional practices as bride kidnapping, which appear to be reemerging, in actuality persisted during the Soviet era as well. However, rather than being an expression of religious belief or traditions, both then and now, the practice has been adapted to social and economic realities.

The contributions focusing on institutional effects generally argue that Central Asia's elites have adapted Soviet institutions and practices in an effort to legitimate their rule. These efforts have not been terribly successful. The reasons offered do not point to a single cause. Some contributors argue that state capacity is too weak (Erika

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Weinthal's thesis in Chapter 8 in discussing problems associated with grappling with environmental protection). Others contend that elites are too divided to effectively use the state's capacity, as has been evidenced in the debate over language policy in Kazakhstan (Chapter 4). Most of the volume's contributors, however, point to the very design of the institutions themselves, arguing that they induce conflict. Laura Adams (Chapter 3) argues that cultural workers' attempts to serve central elite interests by producing works intended to legitimate the newly emerging state have been impeded by the inter- and intrainstitutional competition for resources that marked these same institutions during the Soviet era. Chapters 5 and 6 argue in a similar manner that republican elites attempting to centralize have been met with the resistance of local and regional elites using their institutional bases of power.

The issue of authoritarianism is touched upon in virtually every chapter. Indeed, those studying state–society relations generally do so operating from the premise that democracies require strong societies capable of holding ruling elites accountable. Kelly McMann's contribution (Chapter 7) does not suggest that such societies are likely to emerge in Central Asia anytime in the near future. She argues that civic organizations continue to look to the state for support. Such dependency on the state is rooted in both the relationship of society to the state during the Soviet era and the fact that the state has retained much of the wealth and resource base of the former Soviet state. Under conditions of economic underdevelopment, this makes it virtually the only source of funding within the country. The implications for democracy of a weak society are spelled out most clearly in the volume's opening chapter. Decentralization, whether it has occurred as a consequence of elite competition of the type addressed in Chapters 5 and 6 or as the intended result of state policy, such as the devolution of social responsibilities to local councils (*mahalla*), has not led to either greater public accountability or more efficient government. The elite struggle for power has taken place within the context of a weak society. This has meant a lack of competitive elections and inadequate information flows to assure transparency. The result has been the institutionalization of authoritarianism at the local and national levels.

European Integration and Political Conflict. Edited by Gary Marks and Marco R. Steenbergen. New York: Cambridge University Press, 2004. 294p. $75.00 cloth, $26.99 paper.

— Beate Sissenich, *Indiana University*

In the study of European Union politics, it is commonplace to deplore the absence of electoral mobilization across national boundaries. One of the reasons the EU suffers from a democratic deficit, the argument goes, is that elections to the European Parliament serve as a projection screen for fundamentally domestic debates. The purpose of Gary Marks's and Marco Steenbergen's volume is to subject this common assumption to systematic empirical scrutiny. The central question the editors pose concerns what features shape the contestation over European integration. Specifically, can such contestation be captured by a small number of dimensions? And how do these dimensions relate to the domestic left/right cleavage over the role of the state in the economy?

The editors propose four theoretical models to answer these questions. 1) The international relations model (derived, for instance, from producer-group theories) posits a single dimension of contestation over European integration in which the left/right division is entirely irrelevant. 2) The Hix-Lord model (based on *Political Parties in the European Union* by Simon Hix and Christopher Lord, 1997) sees political conflict structured along two unrelated dimensions, support for versus rejection of European integration and left versus right politics. 3) The regulation model hypothesizes that conflict over integration can be reduced to the domestic left/right cleavage, with the Left calling for more European regulation and the Right for less. 4) The Hooghe-Marks model (developed in *Multi-Level Governance and European Integration* by Liesbet Hooghe and Gary Marks, 2001) claims that the pro/anti-integration dimension can neither be collapsed into the left/right dimension (as hypothesized by the regulation model) nor seen as entirely independent of left/right politics (as posited by the Hix-Lord model). Rather, specific aspects of integration can be mapped onto the left/right divide, producing two likely constellations: Left/pro-integration stands for "regulated capitalism" (p. 9) and right/anti-integration for neoliberalism.

The editors do not address the problem of conflicting theoretical interpretations of the main thrust of integration. Thus, the models assume specific interpretations of integration that are themselves subject to dispute. The international relations model sees integration primarily as a neoliberal undertaking that benefits exporters and hurts import-competing industries. By contrast, the other three models view integration as an attempt to regulate cross-border trade at the European level to substitute for protective and distributive measures traditionally provided by national governments. What integration actually means depends very much on specific policy issues and on the prevailing national status quo, a qualification that is at least considered in some of the contributions.

Still, it speaks to the intellectual cohesion of the volume that most of the empirical chapters directly address these four models. Four chapters each are devoted to individual-level attitudes and party competition, respectively, while two chapters deal with intermediary groups. Carefully triangulating methods and data, the chapters combine evidence from a wide array of sources, including Eurobarometer surveys of mass public opinion, European Election Study data, expert surveys of party positions, elite interviews, party manifestos, roll-call data, and event counts. Thus, the volume offers 10 empirically rich and theoretically ambitious chapters, each of which provides an original approach to

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the same overarching questions. The chapters do not, however, produce consistent findings. Rather than offer a definitive analysis of political conflict over European integration, the volume charts a bold intellectual agenda for further empirical analysis and replication of results.

The limited consensus that does emerge from (some of) the chapters can be summarized as follows: 1) Functional (left/right) and territorial (pro/anti-integration) competition are indeed largely independent, as hypothesized by the Hix-Lord model. 2) At the party level, functional and territorial competition produce an inverted U-curve, with extremist parties on both ends of the left/right spectrum opposing integration and mainstream parties supporting it. 3) A third dimension of "new politics" further complicates the pattern of contestation (see in particular the chapters by Hooghe et al., Jacques J. A. Thomassen et al., and Bernhard Wessels). Whereas the "traditional/authoritarian/nationalist" pole (p. 121) is strongly correlated with hostility to integration, its opposite, the "green/alternative/libertarian" pole (p. 121), varies across issues in its placement on the integration dimension. There is much room for follow-up studies that test the effect of this third dimension on specific policy areas.

All the empirical chapters are densely packed with data analysis deserving detailed comment. But a few stand out because they go beyond the four theoretical models that frame the volume. Both Leonard Ray and Adam Brinegar et al. map individual attitudes toward integration by taking into account the national status quo and resulting perceptions of relative losses and gains. Brinegar and colleagues test the effect of political-economic institutions on voters' attitudes toward integration. They argue that in universalist welfare states such as the Nordic countries, leftist voters tend to be Euroskeptic, whereas in residual welfare states, they expect to gain from more integration. Though their nested model of contextual effects and individual-level variables accounts for only a small percentage of the variation, this is a potentially productive line of analysis that may shed light on the apparent disconnect between the functional and territorial dimensions of contestation. Wessels examines the intriguing but separate question of whether European-level interest groups tend to form in anticipation of or reaction to EU institutional expansion. His answer, according to which new European interest groups tend to *follow* institutional reform, could be made more persuasive by reporting statistical significance levels (Table 9.1, p. 203). By focusing specifically on Eurogroups, Wessels brackets the territorial dimension of conflict in favor of exposing the new politics dimension alongside traditional left/right issues.

Despite the lack of consistent findings, *European Integration and Political Conflict* is required reading for students of contestation in an emerging polity. At least three lines of investigation invite follow-up studies: 1) the effects of national institutional variation on contestation, 2) issue-specific dynamics of contestation over integration, and 3) the role of new politics in structuring contestation in interaction with left/right and territorial politics.

Caught in the Crossfire: Revolutions, Repression, and the Rational Peasant. By T. David Mason. Lanham, MD: Rowman and Littlefield Publishers, 2004. 328p. $78.00 cloth, $30.95 paper.

— Cynthia McClintock, *George Washington University*

Among citizens and scholars in many parts of the world, concern about political violence has been intense since the United States declared a war on terror after 9/11. In this volume, T. David Mason reminds us that, tragically, insurgency was common in the last half of the twentieth century, and he explores a question that remains fundamental: Why, when rebel victory is uncertain at best and the risks large, do people rebel? He probes this question primarily with respect to peasants in Latin America, but his answers are more broadly relevant as well.

In the second chapter of the book, Mason provides an overview of theories of revolution and, in the following four chapters, describes in greater detail conditions provoking political violence. These conditions include dependent development, social and economic dislocation in the countryside, and the concomitant erosion of the stability afforded by patron–client ties; the capacity of social movements to provide selective incentives to peasants who join the rebellion; and weak, even "protection racket" states that are unable to respond effectively to a guerrilla challenge. In an argument with important policy implications, the author emphasizes that state repression, in particular repression of nonviolent political action, is likely to lose peasants' hearts and minds to the insurgency. He highlights "the counterinsurgency dilemma": Soldiers, seeking to survive against a movement about which intelligence is limited, "engage in overkill" and "drive otherwise neutral peasants into the arms of the rebels" (pp. 155–56).

Mason's analysis in these chapters is valuable. He writes clearly and assumes little prior knowledge of the topic; his work is accessible to students. Influenced by rational choice perspectives, the author focuses on what is logical and what is illogical, and resolutely asks the key question: Why? He also frequently highlights the negative impact of dramatic population growth in Third World countries, a factor that to date has often been neglected.

Mason's analysis is not particularly ambitious, however. He is synthesizing traditional scholarly theories, rather than advancing a new theory. Although he shows that socioeconomic deprivation, the political opportunity structure, and an ineffective state are all important to the rise of insurgencies, he does not build a model explicitly delineating the roles and relationships of these factors. Although he acknowledges that "international forces" are "central" (p. 55), the United States is rarely mentioned as an actor in Third World conflicts. Mason does not incorporate recent theoretical explanations by such scholars as Timothy Wickham-Crowley

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(*Guerrillas and Revolution in Latin America,* 1992) or Cynthia McClintock (*Revolutionary Movements in Latin America,* 1998), which do highlight the interplay among these factors. Nor does he consider sufficiently the role of revolutionary organization and ideology; he does not discuss the argument that peasants may find moral and emotional incentives for participation in an insurgency, cogently advanced by Elisabeth Jean Wood (*Insurgent Collective Action and Civil War in El Salvador,* 2003).

Chapters 8 and 9 focus on the insurgencies in El Salvador and Peru, respectively. Mason describes the harsh conditions that provoked the Faribundo Martí National Liberation Front (FMLN) in El Salvador and Sendero Luminoso (Shining Path) in Peru. For El Salvador, he highlights the rise of export agriculture and the concomitant displacement of peasants; the emergence of a "protection racket" state; the mobilization of the poor by progressive Catholic Church groups; and, in particular, the escalating state repression against these and other nonviolent groups, which gradually pushed them toward armed rebellion. For Peru, the author describes effectively the country's rural inequality, pointing out the differences between the export agriculture in the enterprises on Peru's coast and the subsistence farming in the indigenous communities in the country's highlands. He also emphasizes the counterproductive response of the Peruvian state to the Shining Path; during 1983–84, Peru's security forces killed thousands of innocent people and fanned the flames of insurgency.

In various respects, however, Mason's pair of case studies is problematical. He does not clarify why he selected the two cases, or to what extent they are representative of the universe of insurgencies in the Third World. He emphasizes similarities between the rise of the FMLN in El Salvador and the Shining Path in Peru, despite critical differences between the two. Indeed, some scholars of the Shining Path—see, for example, David Scott Palmer, "Rebellion in Rural Peru: The Origins and Evolution of Sendero Luminoso," *Comparative Politics* 18 (January 1986)—have deemed sui generis this unusually savage and fanatical movement, launched from a highlands province against a nominally democratic state. One of the similarities highlighted by Mason is that the movements occurred despite land reforms in both countries. However, the differences between these two reforms—the Salvadoran was initiated after the emergence of the FMLN and limited by the politics of the war, while the Peruvian was initiated before the emergence of the Shining Path and limited by a paucity of good land—were great.

Most problematically, although I agree with Mason that state repression of an opposition social movement has often been a catalyst for rebellion, and certainly was in El Salvador, he exaggerates this factor in the Peruvian case. Although the Peruvian government's indiscriminate repression during 1983–84 was tragic and counterproductive, it did not provoke an opposition movement to violence; Sendero Luminoso was savage and strong in highlands Peru prior to 1983–84 (see Steve J. Stern, ed., *Shining and Other Paths,* 1998).

The question asked in *Caught in the Crossfire* is among the most important of the contemporary era, and the answers provided are thoughtful. The mix of traditional theories of revolution with rational choice perspectives yields interesting insights throughout the book. Its conclusion assesses concisely and carefully the reasons why revolutionary violence might diminish in the twenty-first century—and reasons why it might not. However, Mason is not highly rigorous; his theoretical framework is not innovative, and his research design is inchoate. His research experience in both El Salvador and Peru appears limited, and complexities about the trajectory of the Shining Path in particular are slighted.

Women's Access to Political Power in Post-Communist Europe. Edited by Richard E. Matland and Kathleen A. Montgomery. New York: Oxford University Press, 2003. 400p. $85.00 cloth, $35.00 paper.

— Valerie Sperling, *Clark University*

It was something of a truism that within the political institutions of the Soviet bloc states, women and power were found in inverse proportion to each other. While women occupied roughly 30% of the seats within the faux-parliamentary bodies of the communist region, true power was never located in those institutions. Instead, political power was found at the Communist Party's zenith, where women were seen rarely, if at all.

The collapse of Communist Party–run dictatorships across Eastern Europe presented scholars with an opportunity to test various political science theories derived largely from the study of Western democracies. The editors of this volume are concerned primarily with testing theories of representation. In particular, they seek to explain the causes of women's low representation in postcommunist parliaments. The 18 contributors hail from across Europe and the United States, and present case studies on Germany, Lithuania, Hungary, Ukraine, Russia (including a chapter on regional elections), Macedonia, Poland, the Czech Republic, Slovenia, Croatia, and Bulgaria.

While the authors do not deny that the postcommunist region features several "legacy" characteristics that could depress women's representation (such as seeing women in parliament as Communist Party–sponsored tokens), they largely agree that the paucity of women in parliaments there stems from institutional sources. These include electoral rules, such as the use of proportional representation (PR) versus a majoritarian system, and party rules, such as the use of quotas. However, the authors do not suggest that institutional rules are the only thing promoting or preventing women's equal representation. They portray women's representation as being determined by supply and demand:

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Supply is governed by the number of women wishing to run for office, and demand is determined by the voters, on the one hand, and by party "gatekeepers," on the other. Meanwhile, many sociocultural and economic calculations and preconditions factor into the choices made by potential female candidates, party gatekeepers, and voters. Suffice it to say that women's underrepresentation is overdetermined (and not only in postcommunist Europe).

Several findings hold across most of the countries included in this study. First, institutional rules matter for women's level of representation. PR benefits women when the number of seats contested in a given district is high. Parties can then win larger numbers of seats, allowing in candidates beyond the few top slots on the lists. PR thus leads to the presence of more women in parliament than does a single-mandate district system (as is typically true in Western democracies). There are some exceptions to this rule (notably Russia and Hungary). The Russian mixed-system case highlights the fact that PR is not sufficient to promote women's representation. There, party fragmentation works against women's successful election to PR seats, as does the way that party lists are composed (through patronage), whereby women are often placed in "unwinnable spots" (p. 162). In Russia's 1993 election, across all the party lists, "90 percent [of women] were below thirtieth place on the lists" (p. 163).

Another finding consistent across several countries is that women's organizing is crucial in order to improve female representation on party lists. As Frederick Douglass said, "Power concedes nothing without a demand." In the Polish case, a main cause of the increase in the number of women elected between 1997 and 2001 was that women across parties during the election campaign in 2001 supported a "Pre-Electoral Coalition of Women," the purpose of which was to increase the number of female parliamentarians, and which sported slogans like "Enough adoration—we want representation" (p. 232). As a result of such public pressure, combined with efforts by women within the political parties, some parties instituted sex-based quotas.

Sociocultural factors also influence women's representation. The authors of one chapter cite cross-country poll data showing that majorities of men and women in the region believe that men make better political leaders than do women. "Demand" for women in power is thus low. However, as the editors point out, some of the countries there have increased women's representation, despite such attitudes. The data in the case study chapters show that, to some extent, such increases are traceable to the introduction of PR systems, and to the change over time in party predominance, with leftist parties in particular coming to power, bringing more women into parliament with them. The most interesting case of shifting party fortunes giving rise to more women in parliament comes from Bulgaria, where, in 2001, the former king hitched his electoral efforts to a little-known women's party (of necessity, since his own party was refused registration) and won, thereby bringing the percentage of women in the parliament up to 26.7% from a mere 10.4% in the previous election (p. 316).

Other evidence reveals the underlying sexism that leads party gatekeepers to erect high barriers to female candidates' inclusion on party lists. Several chapters note the fact that women must be "hyperqualified" (p. 207) in order to receive a party's nomination; they are often better educated than male members of parliament, and also must "prove" their legitimacy more extensively than men do. In Poland, for example, in 1993, 73% of the women members of parliament had been members of Solidarity in 1980, whereas this was true of only 49% of male MPs (p. 219).

On the whole, this is a highly readable book that tells the reader an impressive amount about politics and women's representation in postcommunist Europe. The first two chapters set out the research questions and theoretical framework. All of the chapters are written in an accessible style, and the project sensibly combines qualitative and quantitative data in order to best make sense of the diverse levels of women's representation in the chosen countries. The case study chapters are densely packed with empirical data. They feature rich detail and complex explanations, exploring the factors laid out in the theoretical framework, such as public opinion, electoral institutions, party rules, and the change over time in the dominance of political parties. *Women's Access to Political Power in Post-Communist Europe* has a high level of coherence, since each of the cases is explored within the same theoretical framework. The chapters are punctuated by useful at-a-glance tables comparing electoral rules, levels of female representation, and so on. Any study of elections and politics in these countries from here on should take these valuable contributions into account. Gender can no longer be ignored in studies of elections in the postcommunist world.

Framing Europe: Attitudes to European Integration in Germany, Spain, and the United Kingdom. By Juan Diez Medrano. Princeton: Princeton University Press, 2003. 344p. $39.95.

— Andreas Sobisch, *John Carroll University*

During the 1990s, a number of scholarly books and articles appeared in both Europe and North America to help explain the preferences and motivations of European mass publics concerning the European integration process. These scholarly efforts were spawned in large part by the vast amount of survey data generated by the Commission of the European Union in its efforts to monitor public opinion on issues pertaining to the European Union.

This research has taught us much about how Europeans feel about the ongoing efforts to create an "Ever Closer Union" of European states. Three major findings stand out: First, European mass publics, by and large, are neither particularly interested in, nor particularly well informed about, the affairs of the EU; second, utilitarian motivations far outweigh cognitive traits, ideology, or partisanship as explanations for public support; and, finally, the considerable

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cross-national differences in levels of support have remained relatively constant over the past 30 years. For example, the United Kingdom and Denmark have from the beginning been much more critical of the integration process, while the Benelux countries, and to a lesser extent France and Germany, have consistently been much more favorable.

Yet the precise origins of these cross-national differences have largely remained a mystery. The comparative politics literature, by employing in its explanations statistical models that specify individual characteristics, has tended to focus on factors that highlight the *similarities* across EU member states. Cross-national *differences* in levels of support were largely treated as "black boxes" to be accounted for by "dummy variables" inserted into the models.

To explore in detail the sources of these attitudinal differences is precisely the purpose of Juan Diez Medrano's study. He asserts that in order to understand the attitudes of Europeans on these issues, one must thoroughly examine, in each nation, the *historical context* in which these attitudes have developed. He does so by combining in-depth interviews of European citizens with detailed analyses of the processes through which their attitudes and beliefs have become "framed" within the respective national contexts. Drawing on comparative sociology literature, "frames" are defined as the perceptual lenses through which the European Union, and in particular the integration process, are interpreted by the public. Some of these "lenses" are shared across most or all countries (e.g., the EU as a large market), while others are dependent on sociodemographic or political factors, such as occupation or ideology. Others again are closely connected to the specific national cultures, and it is these that constitute the author's main theoretical interest.

The book focuses on three countries, Germany, Spain, and the United Kingdom, which together contain almost half of the EU's population. These countries represent a mix of similarities and differences along a number of variables that are important to the understanding of the observed attitudinal patterns. The research thus combined the "most similar" with the "most different" systems designs.

The intensive interviews conducted with 160 respondents in six cities (two from each country) reveal that over 80% of respondents spontaneously mentioned the benefits of the single market as a positive aspect of integration. Most Europeans thus share at least one basic conceptualization of the EU. However, the central puzzle of Medrano's investigation concerns difference: Why is British support for integration so much lower than that of Germany and Spain, even after all individual-level variables have been accounted for? In short, what is inside the "black box?"

Medrano finds the answer to this conundrum in the pattern of differences observed in the interviews: While some themes articulated by the respondents delineated the adherents of the different models of integration regardless of country (e.g., the notion that states have become "too small"), others more clearly distinguished the different nationalities. Although several opponents of integration expressed concern over the loss of sovereignty and identity that it might entail, it was among the British respondents that this line of reasoning truly predominated, suggesting that these anxieties were a major reason for the overall more Euroskeptical attitudes prevailing in that country. Spanish respondents, by contrast, cited the need to end the country's isolation and to push forward with its modernization as their reasons for support, while West Germans expressed the hope that their country's engagement in the EU would help it overcome the legacy of National Socialism (East Germans rarely mentioned this point, a finding that the author explains in great detail). Thus, whereas the dominant frames tended to enhance support for European integration in Germany and especially in Spain, in the U.K. it was just the opposite.

In order to validate the argument that these frames are culturally constructed, the author undertook in each country an in-depth analysis of editorial opinion, prizewinning novels, history textbooks, and head-of-state speeches covering the entire postwar period. The aim was to correlate the frames identified during the interviews with the themes articulated in these sources. It is impossible in this short review to do justice to the richness of the descriptions and the wealth of evidence presented in support of the argument. Briefly stated, the British reluctance to commit itself fully to the European integration project can be traced to the existence of an alternative set of identities (e.g., Empire) whose persistence resulted in a strong sense of being different from Europe, thus precluding a strong identification with the integration project. While both Spain and Germany had attempted to construct such pan-national identities prior to World War II, these attempts were thoroughly discredited, thus leaving "Spaniards and Germans . . . more receptive to efforts toward European integration" (p. 255).

Medrano's study constitutes a significant contribution to the literatures on European integration, political culture, and nationalism. It tells a compelling story of the construction of collective identities and of the myths upon which they are frequently based. Critics of the political culture approach will find in this study all the usual shortcomings and logical fallacies, yet these cannot take away from the force of the argument presented in it and from the mountain of evidence in support of it. All but one of the true defects are in fact minor: The interview chapters are too long and the narrative is often convoluted; some of the tables are poorly labeled and explained; and the appendixes provide insufficient information on the methodology, particularly of the sampling process. The major systematic flaw I could detect concerned the sample itself: Since it contains very few hard-core opponents of the EU, even from the U.K., the interview data does not really allow conclusions to be drawn about opposition to European integration. But since the real strength of *Framing Europe* lies in its contextual analysis, it constitutes without question a significant achievement.

Defining Russian Federalism. By Elizabeth Pascal. Westport, CT: Praeger, 2003. 224p. $74.95.

— Gerald M. Easter, *Boston College*

The rise of Russia's regions became one of the big stories that followed the collapse of Soviet communism. After decades of submission in a highly centralized and authoritarian system, resurgent regions emerged as autonomous actors, threatening the territorial integrity and political stability of the newly independent Russian state. With the notable exception of Chechnya, Russia's regions eventually were reintegrated with the center, but on the basis of an asymmetrical federal system. Elizabeth Pascal's book examines the process that led to this uneven territorial distribution of power in post-Soviet Russia.

Pascal's analytical focus is the use of bilateral negotiations to redefine the rights and responsibilities between the center and individual regions. More specifically, she argues that the asymmetry in the Russian federation was determined by the variant economic and political resources that regional leaders brought to bear in the bilateral negotiation process. To demonstrate the argument, the study compares three regions that ended up in very different positions concerning economic rights in the new federation. The time period is confined to the Yeltsin presidency. In a conclusion, Pascal places the Russian experience in a comparative perspective, bringing in Spain and Canada as other examples of asymmetrical federal systems.

The real strength of the book is the rich detail found in the three regional case studies—Samara, Bryansk, and Vologda. These cases are constructed from primary sources, based largely on interviews with local officials. While the source base is a noteworthy contribution of the book, the author might have been a bit more critical in its use. The cases provide an interesting insiders' account and bottom-up perspective on the process that led to asymmetrical federalism. They show why some regions were more interested in negotiating over "pocketbook" issues—maintaining access to federal subsidies—rather than in "rulebook" issues—gaining autonomy over the regional economy.

Samara, located along the Middle Volga region, benefited from a diversified and well-developed regional economy. Samara was ruled by a powerful and tactful governor in cooperation with a unified regional elite. Regional politicians were able to concentrate their political capital on bilateral negotiations. As a result, Samara managed to wrest significant economic rights from the center. Bryansk, located in the poor western region near the Belarussian and Ukrainian borders, suffered economically from its dependence on military industry and agriculture. The Bryansk political elite were marred by official corruption, internal division, and leadership turnover. With such obstacles, Bryansk remained a recipient of federal handouts and under the tight fiscal control of the center. Vologda, located north of Moscow in the central industrial region, fits in between these two cases in terms of economic potential and political clout. Not surprisingly, Vologda's negotiations with the center led to an outcome of shared fiscal responsibilities and rights.

Through its empirical findings, the book provides a revealing glimpse into the practice of asymmetrical federalism in Russia. While the description of each case is well done, the study is weak in demonstrating the larger analytical claims. The main question concerning the relationship between the bilateral approach to center–regional relations and the rise of asymmetric federal institutions is left unanswered. In the setup to the study, Pascal argues that Russia's asymmetrical federalism was the result of three influences: preexisting institutions, policy choices, and the intersection of national and subnational interests (p. 3). But the study does not develop this scheme in a systematic and comparative way. Related to this, the study is hindered by a tendency to introduce analytical concepts—path dependency, short-time horizons, presidential vertical, Rikerian bargain (pp. 11, 67, 182)—but not explicitly to follow up on them. Instead, they appear almost as afterthoughts and are not coherently integrated into the analysis. Meanwhile, in the case study chapters, the analysis implicitly suggests that regional economic endowments and political skills are the main determining factors in redefining relations with the center.

The problems with the analysis are compounded by style. The research findings are presented in a narrative style, with a compilation of anecdotes and quotations. There certainly is nothing wrong with this approach, except that there is little effort to place the particular findings into a comparative analytical context. The book would have benefited from a tight conceptual scheme that explicitly and consistently highlighted the key distinctions among the cases and elaborated on their larger implications. As it is, the reader can be frustrated by overly long paragraphs, tedious repetition, and undeveloped analytical statements. The limitations of this approach are exposed in the conclusion, in which the some of the author's key assumptions about bilateralism and asymmetrical federalism seem to have been undermined by the Putin presidency.

The book fits in well with recent scholarship on the reconfiguration of center–regional relations in post-Soviet Russia. With 89 constituent regions to cover in in-depth case studies, *Defining Russian Federalism* provides a needed and valued contribution to scholars. The book will have a more limited appeal to comparative studies of federal systems.

Contention and Democracy in Europe, 1650–2000. By Charles Tilly. New York: Cambridge University Press, 2004. 320p. $60.00 cloth, $22.00 paper.

— Nancy Bermeo, *Princeton University*

Charles Tilly's study of democracy and contention is itself highly contentious and deservedly so. After reading and writing social and political history for over five decades, its author is well positioned to judge which social scientific understandings of democracy hold up to historical scrutiny and which do not. In his search for the "mechanisms and processes that promote, inhibit or reverse democratization" (p. ix), Tilly identifies a number of false leads. One of his most contentious points is that the quest for democracy's "necessary and sufficient conditions" is futile (p. 39). Democracy, he insists, "does not have a single history . . . repeated in more or less the same conditions and sequences by each democratizing country" (p. 35). Searching for either uniform conditions or repeated sequences is, thus, a waste of time. In an equally contentious mode, he cautions "culturalists, phenomenologists, behaviorists and methodological individualists" not to "treat individual dispositions as the fundamental causes of social processes." Democratization and de-democratization cannot be understood through the "reconstruction" and "aggregation" of individual dispositions just before their point of action (p. xi). Nor, he argues, can democratization be understood as a "product of age-old character traits or of short-term constitutional innovations" (p. 9).

Tilly draws an unmistakable line in the sand and leaves hordes of political scientists on the other side, but he never steps across it to engage in sustained individual attacks. Instead, he uses massive blocks of historical evidence to construct an edifice of argument so broad and sturdy that few will dare assault it. The complexity of his argument is an affront to parsimony, but this is precisely what the author intends. He asserts that democratization emerges not from "regular relationships among variables" but from "robust, recurrent causal mechanisms that combine differently with different aggregate outcomes in different settings" (p. 9). Three multifaceted mechanisms drive the democratization process: mechanisms that segregate public politics from inequalities based on social category, mechanisms that integrate trust networks into public politics, and most importantly, mechanisms that increase the breadth, equality, enforcement and security of mutual obligations between citizens and agents of government. Tilly specifies what these (and other) abstractions mean in a series of highly detailed tables. The first mechanisms include activities ranging from the confiscation of church property to the formation of associations that embrace unequal groups. The second mechanisms include activities ranging from government absorption of previously autonomous patron-client networks to the creation of government-backed disaster insurance. The third mechanisms range from the co-optation of regional strongmen through the containment of private military forces, through the formation of coalitions between select regime elites and excluded groups. "For democratization to ensue," these last mechanisms, involving the expansion of political participation, the enhancement of collective control, and the reduction of arbitrary power, "must occur" and they "must combine" with major changes in the relations between public politics and either trust networks or categorical inequalities (p. 22).

The moving force behind these mechanisms and mandatory changes is "popular contention," which occurs when "politically constituted actors" make "public, collective claims on other actors, including agents of government." When popular contention shifts from parochial, particular, and bifurcated interactions based on embedded identities to cosmopolitan and multifaceted interactions based on detached identities, the process of democratization advances (p. 8).

The complexity of Tilly's argument is more than matched by the weight of evidence he brings to its defense. The book contains two chapters detailing the connection between contention and regime types in Europe since 1650; a chapter examining the Low Countries, Iberia, Russia, and the Balkans since 1815; three detailed case studies of France, Switzerland, and the British Isles; and a concluding chapter that draws parallels between the histories of democratization in Europe and elsewhere. Tilly's title does not mislead us. This book truly does embrace 350 years of political history.

Readers who brave the run through the author's dense forest of terms and facts will find themselves breathless but invigorated. Contentious actions do seem to drive the designated mechanisms, and these do seem to propel democratization forward (and backward). This book should encourage scholars who have used a narrow set of variables to understand democratization in the past to now see the process through a wide-angle lens, panning in first on popular contention, and then on its effects.

The points Tilly makes here are not all new. As stated clearly in the introduction, the text sometimes draws on earlier work. But it also draws on a range of evidence he has never assembled before, and it is this that makes the argument stick. Of course, not all its component parts are equally adhesive. One wishes, for example, for more discussion of how the mechanism involving "the expansion of political participation, the enhancement of collective control and the reduction of arbitrary power" differs from democratization itself (p. 22). Tilly is wise enough to address this issue directly and argues that these processes "produce changes in relations among actors *outside* of government before they exert their impact on relations between citizens and governmental agents" (p. 23). This reasoning helps us separate cause and effect, but whether political scientists will succeed in tracing the before-and-after sequence systematically remains uncertain.

At least two of the conclusions in *Contention and Democracy in Europe, 1650–2000* are certain to affect our research agendas immediately. The first is that both religious and secular private trust networks actually hamper democratization if not outweighed by public trust networks. This conclusion leads Tilly to argue that "recruiting people into voluntary associations with the hope of building 'civil society' " will, in the absence of other changes, "do more harm than good" (p. 257). The second agenda-setting conclusion is that "revolution, confrontation, colonization and conquest repeatedly accelerated and activated democracy-promoting processes" in Europe's past (p. 259). At a time when democratization through conquest looms so large on the U.S. political agenda, the study of past acceleration processes merits careful attention. But so, too, does Tilly's insistence that even "crisis [induced] adoption of democratic forms does not suffice to produce stable democracy in the absence of necessary changes in categorical inequality, trust networks and public politics" (p. 24).

The Strategic Use of Referendums: Power, Legitimacy, and Democracy. By Mark Clarence Walker. New York: Palgrave Macmillan, 2003. 166p. $49.95.

— Priscilla L. Southwell, *University of Oregon*

This book portrays the referendum as a consequence of elite bargaining. Mark Walker describes this electoral device as one that arises from an executive–legislative struggle where competing groups attempt to gain political legitimacy from the masses, even in nondemocratic states. Political elites thus use the referendum to settle disputes that appear irresolvable in the traditional chambers of the power, or, as E. E. Schattschneider indicated in *The Semi-Sovereign People* (1960), to expand the scope of the conflict to enhance one's political advantage. The referendum process is thus subject to manipulation—by the choice of wording, the timing of the vote, the subject matter, and even the interpretation of the results.

Walker's inquiry first centers on an analysis of France under de Gaulle (1958–1969). He also examines five Chilean referenda—one in 1925, and four under Augusto Pinochet's regime in 1978, 1980, 1988, and 1989. (Salvador Allende was to have announced a sixth in September 1973.) Most of the Chilean referenda were constitutional in nature and passed by large margins, although Pinochet's attempt to extend his presidency indefinitely failed decisively in 1988. Walker views these French and Chilean referenda as thinly disguised attempts at executive aggrandizement. When de Gaulle and Pinochet failed in their efforts, it was simply because they misperceived public sentiments at the time.

It is curious why the author did not also address more contemporary French referenda, such as the 1992 referendum on the Maastricht Treaty. This referendum, which narrowly passed during François Mitterrand's tenure in office (with 51.05% approval), more clearly fits Walker's hypothesis about elites seeking legitimacy. The examples from the de Gaulle era seem only to confirm the megalomaniac desire of de Gaulle to threaten to resign (if a referendum failed) until French voters had finally had enough of such tactics. This example also dovetails closely with the issue of the ambiguity of referendum results, which the author emphasizes later in his Soviet and Russian examples.

The reminder of *The Strategic Use of Referendums* is devoted to Walker's examination of the myriad referenda, and counterreferenda from the republics, under Mikhail Gorbachev's rule in the Soviet Union in 1991, and the continued use of this device in Russia under Boris Yeltsin. The Soviet Union was a case in which the conflict was not between an executive and a legislature but between institutions of different federal and regional structures, where the main issue was the degree of regional autonomy. However, in Russia in 1993, the referendum again was used as a weapon in the traditional executive–legislative struggle, as Yeltsin battled the Congress of People's Deputies.

This excellent comparative work is very thorough, but it does tend to emphasize those cases in which the hypothesized relationships are confirmed. Walker is convinced that the referendum process is one that is essentially manipulative of the masses, either by the elites or the media that do their bidding: "[T]his study argues that the kind of manipulation possible in referendum campaigns strains the very democratic nature of the process itself" (p. 2). His description of the manipulative aspects of the referendum process does not seem all that different from what we have come to expect from candidate-centered elections, or even public opinion polling. Yet in making this argument, he joins the chorus of those who make similar claims about the dominance of moneyed interests in initiative and referendum processes in the United States (David Broder, *Democracy Derailed: Initiative Campaigns and the Power of Money,* 2000). This analysis is quite credible, and Walker has built a very convincing case with his examples from these four countries.

Such electoral appeals to the public are "all over the map" with regard to motivations, intentions, and results, and yet these alternative scenarios are given short shrift in this book. Sometimes the referendum is a simple absolution of responsibility on controversial issues, particularly involving taxes. Legislatures may feel pressed to reform the status quo but are unwilling to exhaust the political capital necessary to legislate such changes, and so they instead abdicate responsibility. At other times and places, the referendum can be a grassroots appeal to address a grievance that simply is not salient to certain political leaders, such as campaign financing reform or extension of the franchise. Here, governors or other executives may indeed turn to the public to gather support for their policy preferences, but it is not simply a battle against one's enemies as much as a desire for policy reform. Mark Walker has done an excellent job in thoroughly describing the strategic aspect of referenda in four different contexts; it is to his credit that he raises many other issues in the process of this extensive and provocative analysis.

From Mao to Market: Rent Seeking, Local Protectionism, and Marketization in China. By Andrew H. Wedeman. New York: Cambridge University Press, 2003. 292p. $60.00.

— Scott Wilson, *Sewanee: The University of the South*

In the debate on postsocialist transitions, China has proved a contentious anomaly for proponents of the "big bang" and "gradualism" to explain. In particular, scholars have

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contended that China's combination of capitalist markets with socialist purchasing and marketing organs provided opportunities to gain rents by taking advantage of discrepancies between the two price mechanisms. Officials gain from such a pattern of rent seeking, which tends to stall reform efforts. Somewhat paradoxically, Andrew Wedeman argues that economic rents propelled China toward a free market economy, a novel contribution to the debate on postsocialist transitions.

The book offers a detailed account of Chinese local protectionism, primarily focusing on the second stage of China's economic reforms and attempts at retrenchment, 1985–92, a period rife with rent seeking. Protectionism took two distinct forms: (1) export protectionism, raw material and agricultural producers failing to surrender their resources to state procurement agents at fixed prices that fell below market-clearing prices, and (2) import protectionism, provinces or subprovincial units illegally blocking outside goods from entering their territories. Officials and producers engaged in export protectionism because the price scissors created by the state price mechanism favored industrial goods and urban consumers over raw materials and their mainly rural producers. Local officials sought to boost sales on the black market at premium prices and, in the process, capture rents. Import protectionism resulted from supply of a given commodity exceeding demand, thus pitting producers against one another, rather than producers against the state.

The book's most controversial argument is that local protectionism and rent seeking contributed to the development of open markets in China. Wedeman claims that protectionism and rent seeking both raised the prices of goods toward market-clearing levels and undermined state monopsonies. High black market prices increased the supply of goods, which ultimately brought the market-clearing prices down toward the state-set prices. As the price discrepancies narrowed, rents declined, and when supply exceeded demand, the state could remove its price and marketing controls without inducing hyperinflation as occurred in postsocialist economies that followed the big bang model.

Wedeman's analysis of export protectionism is insightful and compelling. He has combed Chinese language materials to provide a rich account of why export protectionism arose and how it forced the state to raise prices toward market-clearing prices. The explanation for export protectionism is less convincing, in part because it relies more on theoretical supposition than on detailed evidence like that provided in the chapter on export protectionism. Essentially, Wedeman offers a prisoner's dilemma in which localities may choose to cooperate (engage in free trade) or not to cooperate (engage in import protectionism). In the late 1980s, excess supply of goods caused localities to move toward a position of mutual noncooperation. The author contends that the central government played a crucial role in moving localities back to a position of free trade (in their "rational self-interest"), not so much with coercion as by convincing localities that it would serve as an "honest broker" (p. 211). As evidence of the commitment to playing such a role, the center cracked down on "triangular debts" and "debt chains," which were series of debts that producers from one locale refused to pay to producers in other regions. As long as debt chains persisted, localities could not be sure that producers in other localities would treat local producers fairly. By Wedeman's own admission, the central government tried but was unable to eliminate the debt chain problem (p. 212). It is difficult to discern if the central government's well-intentioned actions assuaged local officials' fear of moving to a sucker's position by lowering import walls while other localities maintained their own barriers. Moreover, the presumed harmony of interests in open trade should be rooted in specialized production of different products, but in the cases described by Wedeman beer-producing regions tried to keep out beer from other regions and refrigerator producers tried to block imports from other refrigerator factories.

A second potential weakness of the book's analysis is the scant appearance of international trade factors. Foreign investors in China sought to open up the country's market and pressured central officials to alter domestic trade relations. Too, foreign trade likely affected trade scissors in China for tobacco, silk, cotton, and wool, the key commodities analyzed in the chapter on export protectionism. The rapid rise of China's textile trade gave strong encouragement to local officials and local producers to operate outside of the state's monopsony. Thomas G. Moore's (2002) *China in the World Economy* reveals how the quantity and unit prices of Chinese textile exports to the United States, for example, climbed during the late 1980s and early 1990s (pp. 83–93). China faced strong foreign pressure through the Multi-fiber Arrangement to rein in exports of cotton products and silk products (after 1992), which might explain the state's persistent pursuit of monopsonies on textile inputs. Imports of foreign cigarettes gave rise to the marketing of many Chinese cigarettes under false foreign labels. The premium prices paid for foreign cigarettes widened the price scissors for tobacco, creating greater incentives for noncompliance with state demands to sell tobacco to its agents.

The book's conclusion points out how the Maoist legacy of China's fragmented and decentralized economy, in contrast to the Former Soviet Union's more centralized economic structure, contributed to China's market transition. Wedeman contends that post-Mao China could not keep central control over rents in the same way that Russia's economy, which was rife with monopolies, did. Hence, the rent seeking of the late 1980s helped China to get prices to market-clearing levels and to undermine the already fractured state monopolies and monopsonies. *From Mao to Market* offers a rich theoretical discussion and detailed analysis of China's price reforms, focusing on a pivotal period, 1985–92, and undoubtedly it will spur debate among scholars of Chinese political economy and postsocialist transitions.

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INTERNATIONAL RELATIONS

The Size of Nations. By Alberto Alesina and Enrico Spolaore. Cambridge, MA: MIT Press, 2003. 272p. $35.00.

— Solomon W. Polachek, *State University of New York at Binghamton*

When the Soviet Union fell, why did the region break up into smaller independent states, rather than remain one nation? In contrast, at almost the same time, what caused 15 European nations to band together to create a supranational institution with a common currency, rather than maintain their completely separate sovereignty? Are small countries more viable than large countries? Are dictatorships bigger than democracies? And within countries, why are capital cities usually centrally located?

One would think these questions too diverse to be answered with one simple paradigm. But in an extraordinarily innovative and provocative book, Alberto Alesina and Enrico Spolaore provide a brilliant yet parsimonious model, capable of cogently addressing all these questions. In a nutshell, they simply examine what factors influence the size of nations. But in answering that one question about nation size, the authors are able to answer each of the above questions, and many more about the viability of nations. They do so in 13 chapters. The first two provide the theory, the next seven (Chapters 3-9) apply the theory to various situations, the next several (Chapters 10-12) test implications of the theory, and the final chapter (Chapter 13) recapitulates, carefully pointing out qualifications and needed future work.

Of the chapters applying the theory, Chapter 3 considers democratic countries, Chapter 4 analyzes tax and transfer payment policies, Chapter 5 examines dictatorships, Chapter 6 introduces trade among nations, Chapters 7 and 8 explore international conflict, and Chapter 9 takes on decentralization of government services. Of the chapters testing the theory, Chapter 10 adopts statistical methods, Chapter 11 utilizes a historical approach, and Chapter 12 adopts a case study concerning current European integration. All chapters are well written and easy to follow. Many have mathematical models, but the mathematics is separated, making it easy for the less technically inclined reader to skip without any real loss in understanding.

The authors' basic approach is simple. They define nations as powerful entities capable of taking legal actions within their own borders to ensure well-being for their citizens and leaders. One legal action is taxation, which raises revenues for such public goods as defense, highways, and schools, all of which a nation provides its citizens. Larger countries permit economies of scale, and so per citizen costs for public goods diminish (much as the per mile costs of passenger air travel diminishes as airplane size increases). But by becoming large, a nation grows more heterogeneous, making the country more difficult to manage. These bigger populations imply diversity; however, diversity complicates how leaders allocate tax dollars because a wide-ranging citizenry often has conflicting interests. The trade-off between these two, that is, scale and heterogeneity, determines any particular nation's size. Nations gravitate toward an optimal size by being big enough to take advantage of scale, but small enough not to have too much diversity. Any factor that alters this trade-off influences a nation's size. In this context, the book examines myriad factors, for example, form of government (Chapters 3 and 5), international trade (Chapter 6), and conflict (Chapters 7 and 8), that affect this trade-off. The book also explores how this trade-off evolved throughout history (Chapter 11).

Some might think it irrelevant to model a nation's optimal size since nations do not change size very quickly. However, even though nations do not change size annually, they can and do change size over longer time periods. Alesina and Spolaore give numerous historical examples and point out that in 1945, there were 74 independent countries with an average landmass of 201 million square kilometers, whereas today we have 193 countries with an average size of 77 million square kilometers.

Begin with the basics. Given earth's constant landmass, the larger the number of nations, the smaller the size of each nation. Thus, a world divided into a greater number of nations means that on average, each nation is smaller in size. Accordingly, size of nations and the number of nations are intertwined. For this reason, the authors derive theorems regarding number, but these theorems have implications concerning size.

If one assumes that citizens benefit from being close to the seat of government, then their benefits from public goods diminishes the farther they reside from the capital. (This is why the authors argue that capital cities tend to be centrally located.) Given their assumption of an evenly distributed population, a larger population necessarily implies a greater number of citizens farther from the capital. These citizens gain less from government. As such, they have a greater tendency to secede to form a new nation. Citizens in democratic regimes have more voice than citizens in dictatorships. Thus, all else being constant, democratic freedoms empower peripheral citizens to form new nations. In turn, more nations imply smaller sized countries. From this logic, the authors predict democracies to be smaller than dictatorships, which is consistent with the formation of 15 countries following the Soviet Union's fall. This logic also explains the expansion of the Ottoman Empire in the fifteenth and sixteenth centuries, the growth of France in the sixteenth and seventeenth centuries, and the conquests of Germany after World War I.

World trade also affects country size. Consider a world with no trade. This means all nations must be self-sufficient.

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But to be self-sufficient, a country needs to be diverse enough to supply all citizen needs. Small nations are at a disadvantage because they simply do not have the scale to provide everything. To overcome this inadequacy, they must grow. By the same token, free trade enables countries to value heterogeneity over scale, since through trade they can purchase what they cannot produce domestically. As such, nations will be smaller and exploit comparative advantages to obtain goods not produced at home. Thus, free trade eras should lead to smaller countries than periods of trade restrictions. According to Alesina and Spolaore, this principle explains why "the city-states of Italy and Northern Europe [between the fourteenth and seventeenth centuries] prospered because of sea trade" (p. 219). Similarly, they argue that "the trend towards economic integration and political separatism over the last fifty years has resulted in the number of independent countries almost tripling" (p. 219).

The authors' approach reinforces the current literature on trade and conflict. In that literature, trade leads to a diminution of conflict. They argue that increases in trade leads to smaller countries. But since smaller nations spend less on defense, conflict is likely to fall. Worldwide belligerence also affects country size. As they state, "the size of countries is influenced by the need for governments to protect [their] citizens in an unfriendly world" (p. 95). But we already know that economies of scale mean that bigger countries are more efficient at providing public goods. Given that defense is one such public good, large countries are at an advantage in a bellicose world. Conversely, the authors argue that when world tensions eased, such as following the Cold War, separatism exploded, leading to the creation of new states.

I wish I could find some substantial fault with the book, but except for a few minor typos and perhaps a cursory empirical analysis, I cannot. *The Size of Nations* is a brilliantly crafted piece of work. Although some might find the implications regarding how polity, trade, and worldwide belligerence affect nation size obvious, I do not because each of the implications is derived from a powerful overriding theory. This is an especially crucial read for political scientists since it illustrates how a very parsimonious model can lead to strong predictions, reasonably upheld by data. The book ranks among the best I have read in my career. I recommend it highly.

The Behavioral Origins of War. By D. Scott Bennett and Allan C. Stam. Ann Arbor: University of Michigan Press, 2004. 300p. $59.50 cloth, $24.95 paper.

— Philip A. Schrodt, *University of Kansas*

An aphorism in the natural sciences states that one should either write the first article on a subject or the last one. The statistical study of war began in the 1930s and 1940s with the work of Lewis Richardson and Quincy Wright, then expanded massively in the 1960s with the Correlates of War project based at the University of Michigan. Those were the first articles. This book is potentially the last important one.

D. Scott Bennett and Allan C. Stam, in a massive synoptic data analysis, have assembled a comprehensive set of variables—packaged in the marvelously user-friendly *EUGene* software package [http://www.eugenesoftware.org/]— and applied a uniform methodology to test most of the major theories that have suggested empirical predictors for interstate war. This industrial-strength system for the slaughter of sacred cows will doubtlessly be greeted by howls of protest from die-hard advocates of the various theories, but to this observer—who has no stake in any single theory—the authors have gone to great lengths to be fair and impartial. The theories are not straw men, but instead have elicited thousands of journal citations. If they were as good as their advocates claim, they should have stood out in these tests. With three exceptions—strong effects for geographical contiguity, mutual democracy, and a systemic concentration of power—they do not.

It is clear that this is not the outcome that the authors expected or desired. But if their objective had been merely to delegitimate the quantitative study of war, there were far easier ways to do so than with a 12-year effort involving data collection, software development, and statistical analysis. While one can quibble with any of the various analytical choices made in the exercise, the overall package is solid, and it seems quite unlikely that minor modifications of their methods would lead to major modifications in their results. The authors, in fact, try some alternative formulations—including one requiring 60 days of computation—and find that their overall conclusions hold.

One of Bennett and Stam's more intriguing innovations is adopting the standards of medical research to assess whether variables really make a difference. The 1-in-14,000 risk of war per dyad-year is similar to the risk of some diseases that have invoked policy responses, notably lung cancer. Most of the proposed causes of war show nowhere near the level of risk that is associated with policy changes in the domain of public health, and instead are at levels that epidemiologists have learned will usually not survive subsequent tests.

Bennett and Stam's results are also consistent with two other lines of evidence. First, the past 40 years of extensive statistical work has found consistently strong results only for contiguity and the democratic peace (the strong effect of systemic power concentration is the novelty in this study). The other proposed causes have proven to be "oat bran theories" that shine briefly in one set of articles, only to fade upon additional examination. The closest parallel exercise in the qualitative literature—Geoffrey Blainey's (1988) *The Causes of War*—comes to the same general conclusion: Very few proposed theories about war can survive a broad historical examination.

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In view of this rather staggering assessment, where does one go next? Bennett and Stam deal with this question extensively in their first and final chapters. The obvious alternatives would be to revert to either Leopold von Ranke's historicist approach that emphasizes the uniqueness of events or, in the contemporary world, to a postmodern nihlism that denies most notions of objective evidence. Both approaches are rejected by the authors with detailed arguments grounded in the assertions that quantitative statistical methods are the best means of integrating large amounts of empirical evidence, and that most historical and case study approaches by political scientists do, in fact, view history as having repeated patterns instantiated in an objective reality.

Bennett and Stam suggest a number of alternative statistical agendas. One potentially productive avenue—pursued in some initial tests in the book itself—is to subset the cases on time, space, and other characteristics. This is particularly credible since the time period in the comprehensive test—1816 to 1992—encompasses changes in warfare greater than any seen before in human history. Changes in international politics, notably the expansion and demise of global colonial empires, were almost as great. A 177-year period in medieval Europe, imperial China, the Mayan city-state system, or the steppes of Central Asia would most certainly show far greater behavioral regularity.

Unfortunately, good data for these periods is difficult to acquire, and there is an additional implicit assumption in most contemporary international relations research that one should seek to develop theories relevant to the modern system, not for Philip II of France or Genghis Khan. But with a bit of rereading of the original theories (and possibly some interpolation as to what their authors *meant* to say based on the choice of case studies), and more importantly, giving up the exceedingly ambitious objective of having a single "law" that covers an extraordinarily heterogeneous sample—black powder muskets and smart bombs; nineteenth century Central American dictatorships and the European Union—one may yet find strong statistical regularities.

Subsetting is effectively what research on the democratic peace has done with great success. By restricting the applicability of the theory to democratic dyads, one effectively imposes a set of controls on space, time, and other circumstances implicitly required of the theory. When the cases are conditioned on these restrictions, a very nice empirical result emerges. Subsetting, while not used in most statistical studies of war to date, is consistent with a behavioralist approach, and hardly a retreat to either historical uniqueness or postmodern nihilism.

The Behavioral Origins of War is a tremendously important work, but it brings a message—there is no there here—that many well-established research traditions will not want to hear. Time will tell whether it is treated as the sympathetic wake-up call its authors intend, or die a death by a thousand critical cuts from entrenched interests.

Genes, Trade and Regulation: The Seeds of Conflict in Food Biotechnology. By Thomas Bernauer. Princeton: Princeton University Press, 2003. 224p. $39.50.

— Alasdair R. Young, *University of Glasgow*

There is more heat than light in the discussion of the differences between the United States and the European Union over the regulation of genetically engineered (GE) crops. Thomas Bernauer's sober, thorough, and accessible book sheds welcome light on a complex issue, while also taking some of the heat out of the discussion.

Bernauer is motivated by concern that the "deep crisis" affecting this technology will deny the world, especially the developing world, potentially useful crops. He contends that this crisis stems from profound differences in how polities regulate the technology and the attending trade conflicts between them. He focuses on the United States and the EU as they are the two "regulatory poles"—the United States has the world's most biotechnology-friendly regulations and the EU its least—and have the most impact on other countries. Chapter 2 provides an excellent account of the emergence of and challenges confronting agricultural biotechnology. The meat of the book explains the differences in approach between the EU and United States (Chapter 3), analyzes why these differences emerged (Chapters 4 and 5), and examines the prevailing attempts to resolve these differences (Chapter 6). Having concluded that these efforts do not address the reasons underpinning the differences, Bernauer advances his own suggestions for "coping with diversity" (Chapter 7).

The crux of the author's argument is that the different approaches to regulating agricultural biotechnology in the United States and EU reflect the interaction within each polity of interest group competition (a "bottom-up" process) and the dynamics of regulatory federalism (rather confusingly called a "top-down" process). He argues that the two processes push in the same direction within each polity, but in the opposite direction from those in the other polity. In other words, the anti–GE technology interest groups in Europe have been more influential than their counterparts in the United States *and* the greater regulatory autonomy of the EU's member states (relative to the U.S. states) has enabled those most hostile to the technology to "ratchet up" the EU's rules.

Central to the influence of anti-GE groups in the EU has been the "public outrage" stemming from a distrust of agricultural biotechnology and a lack of trust in regulators. Bernauer argues provocatively that anti-GE nongovernmental organizations (NGOs) in the EU did not create this distrust but capitalized on it as a means of mobilizing members and financial resources (p. 69). Unfortunately, this claim is rather poorly developed. As the author admits (p. 75), the survey evidence suggesting that negative public attitudes toward agricultural biotechnology preceded NGO activism is "sketchy." Other means of getting at the issue—such as interviewing NGO representatives or contrasting

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NGO activity in member states with different levels of public outrage—do not appear to have been tried. An important unanswered puzzle is, if the motive was self-interested, why have European consumer groups not been as hostile to agricultural biotechnology as European environmental groups?

Bernauer's introduction to regulatory federalism is excellent, but its application to the specific case is problematic. The central problem is that his discussion slips between the creation/reform of the regulatory framework and the approval of specific products. It is only really in the context of shaping the approval process and labeling rules, however, that the interaction among different jurisdictions matters. When it comes to individual approvals, the preferences of the member governments matter, not their regulatory autonomy. That the member governments' preferences regarding agricultural biotechnology are reported but not explained is disappointing. None of this invalidates the overall thrust of his analysis, but it does mean that it is not as compelling as it might have been.

Having examined the deep roots of the two regulatory systems, Bernauer analyzes the implications of their profound differences. He identifies strong pressures for the United States to seek to end the negative effects of the EU's regime on U.S. exports. He is, however, pessimistic about the prospects of resolving the problem through negotiated approximation or mutual recognition, the unilateral adoption of a more stringent approval process by the United States, or the payment of compensation by the EU. Rather, he anticipated that the issue would end up before the World Trade Organization, which it did, just as he was completing the manuscript. Consequently, much of Chapter 6 examines the relevant WTO rules. Bernauer seems to have been caught out by the launching of the WTO complaint, which is included only in general terms and sits rather uncomfortably within the text. In addition, his interpretation of the relevant WTO rules could have been better substantiated. Nonetheless, he comes to the quite possibly correct prediction that the EU may well prevail. Further, he contends, again probably correctly but on the basis of a rather crude (and now dated) extrapolation from the EU's problems complying with the WTO judgment challenging its ban on hormone-treated beef, that even if the EU were to lose the complaint, it would be unlikely to comply, or at least not in a way that would greatly improve market access.

In the light of this, Bernauer turns in Chapter 7 to coping with diversity. The first of his three complementary proposals is to strengthen national and supranational regulatory authorities in the EU in order to address low consumer confidence. The second is to encourage market-driven product differentiation, which is already under way, but which requires expensive identity preservation systems and labeling schemes if it is to be truly effective. The third is to support developing countries by funding research on agricultural biotechnology applications that would benefit them and by providing aid for developing their regulatory systems. With the exception of the market-driven scheme, which would likely become much more attractive if the United States loses its WTO complaint, these proposals do not seem obviously more likely to occur than the noncoercive measures Bernauer dismissed in Chapter 6 on the same grounds.

Despite its shortcomings, *Genes, Trade and Regulation* is essential reading for anybody who is interested in agricultural biotechnology or wants to understand the transatlantic dispute. The weaknesses in some aspects of the analysis, however, reduce its attractiveness for those more interested in the interaction between domestic (regulatory) and international (trade) policies more broadly.

Terrorism and the UN: Before and After September 11. Edited by Jane Boulden and Thomas G. Weiss. Bloomington: Indiana University Press, 2004. 248p. $60.00 cloth, $24.95 paper.

— Donna M. Schlagheck, *Wright State University*

One year after the attacks of 9/11, a workshop was held in New York City to explore two questions: How does the United Nations respond to terrorism, and how has the international environment been changed by terrorism? This well-edited selection of chapters, first presented at the workshop sponsored by CUNY's Ralphe Bunche Institute and the University of Oxford's Centre for International Studies, offers a thorough examination of the UN post-9/11, and provides an inventory of the questions now facing scholars of international conflict and international organization.

Editors Jane Boulden and ThomasWeiss provide an analytical framework in their introduction, emphasizing that the work focuses on the United Nations, not terrorism. The two, of course, are inextricably linked. The paradigm shifts examined in UN organizational functions and in the issue area of security are driven heavily by the phenomenon of terrorism. The editors and contributing authors all address the core conceptual problem surrounding the definition of terrorism and the numerous difficulties that stem from the lack of consensus on a definition. Neither the editors nor authors address the fundamental contradiction built into the UN's charter and values, that is, that sovereign states have a right to security (including self-defense when threatened) *and* that peoples have a legitimate right to struggle for their national self-determination. The inevitable clash of these two rights is captured in the dilemma terrorism poses, and it merits greater exploration in this work. Editors Boulden and Weiss also claim correctly that the existing literatures on terrorism and U.S. foreign policy have generally neglected the role of the United Nations; Bruce Hoffmann's *Inside Terrorism* (1998) is the exception, and he uses the "definition" chapter to explore why the United Nations has been stymied in its efforts to define and act effectively against terrorism. The more recent and abundant studies of terrorism, such as Yonah Alexander's *Combating Terrorism* (2002), looks at the counterterror strategies

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of 10 countries, and Walter Laqueur's *The New Terrorism* (1999) notes few links between managing terrorism and the United Nations.

The strength and significant contribution of this work is its dual commitment to studying the transforming and interactive effect of terrorism on the UN and the international security environment. To that end, it includes works on international law and the inherent difficulties of interinstitutional cooperation, as well as in-depth looks at the organizational dynamics of the Security Council and the General Assembly. The post-9/11 approach gives *Terrorism and the UN* a distinct advantage; two leading UN works, *Multilateral Diplomacy and the United Nations Today* by James P. Muldoon et al.(1999), and the second edition of *The United Nations in the Post–Cold War Era* by Karen A. Mingst and Margaret P. Karns (2000), must be revised in light of the shattering effects of the 9/11 terrorist attacks.

This work will be of interest to scholars of American foreign policy and will be quite useful as a companion reader in foreign policy, international organization, and international conflict seminars. Of particular interest will be the discussion on "The Role of the Security Council" by Chantal de Jonge Oudraat; "The Root Causes of Terrorism and Conflict Prevention" by Rama Mani; and "The US, Counterterrorism, and the Prospects for a Multilateral Alternative" by Edwin C. Luck. All three bridge the multidisciplinary divide posed by the study of terrorism and the UN.

Oudraat's contribution is in her identification of the five trends (pp. 151–52) that drive terrorism to higher levels of salience in Washington, D.C., and the Security Council: an increase in the proportion of terrorist attacks aimed at the United States, the rising number of casualties in attacks, the mounting evidence of a global terrorist network, growing concern over terrorist use of weapons of mass destruction, and state support for terrorism. Coupled with her discussion of post-9/11 trends in militarizing and globalizing international responses to terrorism, Oudraat enriches significantly the discussion on unilateral versus multilateral policy options in the U.S. policy arsenal.

Rama Mani touches on another linkage worth exploring: that international conflict scholars and those focusing on international organizations share a growing interest in the root causes of conflict, especially scholars focusing on conflict prevention. The "root causes" debate has been politically charged, but Rama cogently explores (pp. 225–37) it along three causal axes: poverty/despair, failed states, and "clash of civilizations."

Finally, Luck, whose recent *Mixed Messages: American Politics and International Organization, 1919–1999* (1999) explored long-term trends in U.S. unilateralism, multilateralism, and exceptionalism in international affairs, makes yet another compelling argument for a multidisciplinary approach to the study of conflict and organizations that fight it. He assesses the Bush administration's efforts to invoke multilateral support for the war on terrorism as having been undermined by "a series of self-inflicted disabilities" and "garbled public diplomacy" (p. 75), but it is interesting to note that he rejects outright "complaints about excessive unilateralism in the US response to the terrorist attacks of September 11" (p. 92). Foreign policy and international organization scholars will find this counterintuitive and provocative grist.

Overall, the 10 chapters in this collected work are well edited and coherently organized, and they benefit from a unifying analytical framework. The implicit premise of the work, that greater multidisciplinary research bridging both international organization and international conflict will expedite our understanding of the impacts of 9/11, is compellingly well argued. This book clearly and usefully fills a void in the literature and promises to stimulate continued cross-disciplinary work on the new security environment.

Pivotal Deterrence: Third-Party Statecraft and the Pursuit of Peace. By Timothy W. Crawford. Ithaca, NY: Cornell University Press, 2003. 304p. $39.95.

— Vesna Danilovic, *Texas A&M University*

Pivotal deterrence highlights the problem of dual deterrence in which one state (pivot) attempts to preserve the status quo and prevent conflict between two revisionist adversaries, while each depends on the pivot's support and/or neutrality for its ultimate success. Timothy Crawford's book is an important and, in several ways, original statement on a topic unduly ignored in past studies. In the first two chapters, the author carefully lays out his theoretical expectations concerning the conditions that make pivotal deterrence 1) possible, 2) probable, and 3) likely to succeed. A set of necessary conditions expected to make pivotal deterrence possible are clearly outlined: The two adversaries must see each other as more threatening than the pivot, whereas the pivot must be at least equal in power to them, prefer the status quo between them, and believe that both adversaries are revisionist and willing to go to war if assured of its support.

The author then tackles the next issue of what makes pivotal deterrence attempts probable. Here, Crawford provides an interesting angle and novel use of an old problem concerning the interplay between the balance of power and interests. Depending on whether the pivot's interests in the adversaries are vital or secondary and its power preponderant or equal to them, there are four possible combinations, though only two are expected to give rise to pivotal deterrence. The author perceptively resorts to the logic of selection bias to rule out the situation of a pivot's equal power and secondary interests as unlikely to trigger such attempts, because the pivot should reasonably anticipate almost a sure failure and, thus, unlikely select itself into the crisis. Similarly, the reverse situation of a pivot's vital interests and preponderant power predisposes it to success, which, in turn, should make general deterrence work. We are then left with two conditions that are likely to trigger attempts at immediate pivotal deterrence—the pivot's equal power/vital interests or its preponderant power/secondary interests. The cases are selected to represent each of these two contingencies, as both are necessary for attempts to occur, but neither is sufficient to determine their eventual success or failure.

The book's main concern is with the outcomes of pivotal deterrence, which are principally attributed to varying alignment options available to adversaries. Crawford rests his compelling explanation on the premise that the pivot's bargaining leverage declines or strengthens in proportion to the number of alternative "pivotal" allies (if any). Moreover, there is a detailed analysis of specific causal mechanisms leading to success or failure under varying alignment options. Overall, minimal ("scarce") alignment options should lead to success in preventing war, but need also be accompanied by the "uncertainty effect" by which the pivot keeps the adversaries unclear about its ultimate allegiance in case of failure. If uncertain about the pivot's commitments, both adversaries would exercise caution and/or compromise to avoid alienating the pivot. On the other hand, alternative ("abundant") alignment options work against success by creating avenues through which the pivot's bargaining leverage is undercut. The careful discussion of several peace- and failure-inducing mechanisms presents one of the most interesting points in the book.

The case selections are guided by a concise and excellent treatment of relevant methodological points, and the empirical analysis is expertly done. Crawford shows a mastery of the methods of causal analysis in historical case studies, making this a primer in qualitative research design. The tests show that the pivot succeeds if the adversaries cannot substitute their dependence on its support with alternative alignment options, as analyzed in the cases of Bismarck's attempts to preserve the status quo between Austria-Hungary and Russia in the Eastern Crisis 1875–78 (Chap. 3) and U.S. efforts to prevent war between Greece and Turkey in the Cyprus Crises of 1964 and 1967 (Chap. 5). The study insightfully dissects alternating Greek and Turkish approaches to Moscow in their intent to force the United States to compete with Moscow, and how America's consistent pursuit of pivotal deterrence policy minimized the destabilizing effects of such attempts at alignment diversification. Although each case presented different variants of the pivot's interests and power (vital/equal in the former case and secondary/preponderant in the latter), the absence of effective alternative alignment options directed adversaries toward restraint.

I was a bit intrigued by the 1870s Eastern Crisis analysis that places Germany as the pivot deterring Austria and Russia. A more conventional approach attributes the eventual Russian concessions to Britain's overt threats as a protector of its interests in Turkey. That is, the same episode can alternatively be interpreted with Britain as the key deterrent figure and, perhaps more important, as a classic form of extended immediate, rather than pivotal, deterrence. The crisis was too complex, however, to sustain only one analytical angle, and the author made a valid case for seeing it through pivotal deterrence lenses as well.

Conversely, alternative alignment options led to pivotal deterrence failure in both critical combinations of the pivot's interests and power. The British failed to preserve the status quo and deter France and Germany from escalating in the 1914 July Crisis (Chap. 4), and U.S. efforts proved unsuccessful in preventing war between India and Pakistan in the Kashmir Conflict of 1962–65 (Chap. 6). The examination of South Asian crises is particularly successful, showing how the Sino-Indian conflict drew China to Pakistan, which then predictably drove India closer to Moscow, all of which hampered U.S. bargaining leverage as a pivot. As for the 1914 July Crisis, given the differences between many schools of thought that are far from being resolved, it is only fair to say that Crawford's is yet another interesting take, perhaps more informative for international relations theory than World War I historiography, on this perennial historical controversy.

The book concludes with a discussion of how the theory sheds light on the recent U.S. successes and failures in preventing wars among third parties. For example, the analysis of the Kosovo crisis was refreshing for its strictly strategic context, and is one of the most penetrating studies of this crisis from a strategic vantage point.

There could have been a more thorough examination of deterrence theory, however, recognizing several distinct approaches. Identifying the entire corpus of deterrence theory with the premise that "the best way to achieve deterrence is to make your threats and promises early, clearly, and publicly" (p. 12) is obviously too narrow. There is a longstanding argument advocating strategic unclarity in many forms of deterrence, but the author seems to identify it primarily with pivotal deterrence.

Relatedly, the book takes a somewhat restrictive conceptual understanding of extended deterrence, juxtaposing it to pivotal deterrence. One could argue, however, that some scenarios of pivotal deterrence can be interpreted as modified extended immediate deterrence (EID). This is most notable under the "straddle strategy" scenario in which the pivot (i.e., defender in EID) believes that one side would not go to war without its support, while the other would if the pivot stays neutral. It is not surprising, then, that some of the cases identified with pivotal deterrence have been analyzed elsewhere as extended immediate deterrence (e.g., 1914 July Crisis or the U.S.-Taiwan-China triangle). Had the author recognized the much closer affinity between these two forms of deterrence, he could have acknowledged his possible contributions to extended deterrence research as well. This is particularly true for its potential toward solving the complexities of the moral-hazard problem between defender and protégé, an issue inexplicably neglected by many analysts.

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That said, *Pivotal Deterrence* is a fascinating read, profuse with engaging arguments, sure-handed historical research and elegant narratives, and ingenuities in drawing policy implications for current U.S. foreign policy. Crawford makes an important contribution to diverse areas of conflict research, including deterrence, alignments, defensive realism, mediation as coercive diplomacy, and overall war prevention. His book should undoubtedly have strong appeal for both international relations scholars and policy practitioners.

Unwanted Company: Foreign Investment in American Industries. By Jonathan Crystal. Ithaca, NY: Cornell University Press, 2003. 256p. $35.00.

— Andrew P. Cortell, *Lewis & Clark College*

Jonathan Crystal's primary theoretical objective in this book is to refine our understanding of "how societal actors translate economic interests into policy preferences" (p. 2). In this respect, he is not concerned with explaining why societal actors achieve their preferences, but instead with why societal actors seek one policy rather than another to further their interests. To do so, the author examines U.S. firms' reactions to the increase in foreign direct investment in the United States during the 1980s and 1990s.

Crystal rightly notes that in the international political economy literature, the dominant way to understand societal preferences is by reference to societal actors' economic position and the relative economic returns they receive from different policy choices. Chapter 2 provides a useful overview of three such economic interest explanations—a production profile approach, a global industry perspective, and a strategic trade approach—and their expectations for a firm's likely preferences for the regulation of trade and inward foreign direct investment (IFDI). Crystal concludes that deducing expectations from a firm's economic situation is likely to provide only a partial understanding of its IFDI policy preferences since "[o]ften a number of policies (not all of them mutually compatible) would serve producer interests" (p. 2), and it is "unclear which motives or incentives will dominate a firm's calculation of its policy preferences" (p. 8). Which policy ultimately is demanded is a function of political factors: "[F]irms will seek only policies for which domestic institutions exist to channel their demands and supply the policy output; moreover, the policies must accord with widely held beliefs concerning the appropriate role of the state" (p. 155). This is the book's central analytical premise. It should be noted that the author does not seek to link specific institutional configurations or norms to demands for more or less restrictive IFDI strategies; instead, the existence or absence of a norm or institution simply affects firms' decisions "to pursue or not to pursue a particular strategy" (p. 25) to maximize their profits. He determines this effect empirically, maintaining that institutions and norms reduce the relative costs of alternative actions by providing information about their comparative feasibility and legitimacy. In this way, domestic institutions and norms are identified as "telling us how economic interests are translated into different types of policy preferences" (p. 7).

Crystal writes (p. 162) that "[t]he political approach laid out in this book represents the beginnings of a synthesis of a deductive, parsimonious, rational-choice orientation and a more accurate, context-sensitive, historical institutionalist approach. The main ideas behind such a synthesis are neither terribly complicated nor surprising." Here are the primary strength and weakness of the book. It provides a straightforward way to make more accurate some of the more theoretically spare and useful insights regarding preference formation. However, the book's intuitive suggestions to strengthen these theories remain contextual and less precisely specified than they might have been. For example, firms are expected to demand a policy for which the prevailing institutional context promises the most favorable outcome. Yet it is not completely clear how one identifies a priori the specific institutional traits posited to affect a firm's probable success: those that channel firms' demands and supply desired policies. In addition, greater specification of these institutions and their impact for the comparative feasibility of various policies is needed since societal actors typically face a complex institutional landscape; multiple institutions resulting in different policies may serve as resources for firms to achieve their interests.

The four empirical chapters apply the argument to eight sectors that faced increasing foreign competition in the U.S. market and, by consequence, a reason to consider demanding policies dealing with IFDI. Crystal investigates the steel, consumer electronic, semiconductor, automobile, machine tool, antifriction bearing, airline, and telecommunication services industries. The breadth of empirical coverage is impressive as is the array of details regarding a litany of policy demands for each sector. In each chapter, the author derives expectations from the three economic interest approaches for the sector's preferences and explains whether and how the two political factors shed light on the IFDI policy preferences the firms advanced. To Crystal's credit, he is careful to note when the political factors do not offer significant additional insight (e.g., Chapter 3). Nonetheless, several of the other chapters, particularly those covering the semiconductor, automobile, and machine tool industries, indicate the difficulties of using only the economic approaches to make sense of the firms' demands.

One goal of the book is to explain why U.S. firms spent more time and resources demanding trade restrictions than foreign direct investment regulations. Crystal suggests that this contrast can be understood by reference to institutional and normative differences in the two arenas of U.S. foreign economic policy. The empirical chapters might have explored this comparison in more depth with respect to each sector's strategies, especially in light of the emphasis on the role of economic interests in the conclusion

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(pp. 145–50). There is nevertheless much of interest in the sectors' varying strategies toward IFDI, and Crystal helps to clarify their individual policy preferences. These chapters often indicate that government officials have their own ideas about how to deal with IFDI. Since the author tends to associate institutions with rules and regulations, it is unclear how state actors fit into the conception of institutions on offer. Given that policymakers enforce regulations, moreover, how do individual policymakers' preferences affect societal actors' calculations of a policy's feasibility or legitimacy? In this respect, is it the presence or absence of institutions per se or the presence or absence of sympathetic officials in these institutions?

In sum, *Unwanted Company* usefully draws our attention to the impact of political factors on firms' decisions to translate their desire for greater profit into preferences for supportive government action. Crystal's use of domestic institutions and norms to understand these decisions offers a promising avenue for future research, and his detailed discussions of the sectors' policy preferences regarding the regulation of inward foreign direct investment provide a valuable resource for students of U.S. foreign economic policy.

US Hegemony and International Organizations: The United States and Multilateral Institutions. Edited by Rosemary Foot, S. Neil MacFarlane, and Michael Mastanduno. New York: Oxford University Press, 2003. 312p. $74.00 cloth, $24.95 paper.

— Andrew R. Willard, *Yale University*

This important and timely work has the potential to disturb those who feel they already understand the conditions, character, and consequences of U.S. hegemony with respect to the way in which this position of power shapes U.S. efforts at establishing, maintaining, transforming, and terminating multilateral institutions. For people who view the United States as a champion or villain in its relations with international organizations, this work is likely to be irritating and upsetting; and for those who are less certain or dogmatic, this collection of studies is an excellent introduction to the subject because the work includes detailed, empirical case studies on a wide range of issues. Regardless of one's alignment or identification with these groups, this finely edited volume is illuminating, and its high quality makes it relevant to scholars, decision makers, and interested members of the public. Support for this recommendation is found in the careful scholarship that runs throughout and in the firsthand experience of many of the authors with the topics and controversies they analyze and present. Given the international audience to which this study is directed, the work's overall credibility is probably enhanced by the fact that many of the authors are not Americans.

The impetus for researching and writing *U.S. Hegemony and International Organizations* developed from an interest by the editors and authors in clarifying the nature of the interplay between the United States and multilateral institutions, especially since the collapse of the Soviet Union. I suspect that they were also motivated to focus on this critical issue because of the passion it can provoke. Through a series of case studies and a systematic set of research issues, which each author deals with in a way that is tailored to the particular case, the work as a whole provides a coherent, lucid, thorough, and nuanced description and analysis of the phenomena of concern. The charged emotional climate that tends to pervade discussions of U.S. relations with multilateral institutions is not present, nor is it studied.

The book is organized in five sections. In an introductory essay, the editors describe the primary research issues each author addresses. These subjects include case-specific examinations of U.S. behavior toward multilateral organizations or institutions. The specific organizations and institutions vary, of course, from case to case. Most importantly, the emphasis here is on explaining U.S. behavior, rather than on evaluating or appraising it. Factors that shape U.S. conduct are considered in terms of the internal or domestic context and the external or international environment. Authors also identify and analyze the impact of the United States on multilateral organizations and institutions. The editors note that the United States can have significant impact even when it is not a formal member of an organization or institution since formal members will shape, in part, their own policies and conduct in light of their understanding of how the United States will respond to different positions and behavior.

Ten case studies grouped into three categories comprise the heart of the book. The first category, "Perspectives on the U.S. and Multilateral International Organizations," includes two case studies: "American Exceptionalism and International Organization: Lessons from the 1990s," by Edward C. Luck, and "State Power and the Institutional Bargain: America's Ambivalent Economic and Security Multilateralism," by G. John Ikenberry. The second, "The U.S. and Global Organizations," consists of four chapters: "U.S.–UN Relations in the UN Security Council in the Post–Cold War Era," by David M. Malone; "The United States and the International Financial Institutions: Power and Influence Within the World Bank and the IMF," by Ngaire Woods; "The United States and the GATT/WTO System," by Gautam Sen; and "Looking Beyond the 'K-Word': Embedded Multilateralism in American Foreign Environmental Policy," by Stephen Hopgood. The third group of case studies, "The U.S. and Regional Organizations," includes four chapters: "Making Africa Safe for Capitalism: U.S. Policy and Multilateralism in Africa," by Philip Nel; "U.S. Approaches to Multilateral Security and Economic Organizations in the Asia-Pacific," by Ralph A. Cossa; "Trouble in Pax Atlantica? The United States, Europe, and the Future of Multilateralsim," by David G. Haglund; and "Power Multiplied or Power Restrained? The United

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States and Multilateral Institutions in the Americas," by Hal Klepak. As would be expected, some of the studies are more insightful and compelling than others, but they are a rich source of information on particular topics.

In a concluding essay, "Instrumental Multilateralism in U.S. Foreign Policy," the editors summarize and integrate the findings of the case studies: In general, "there is no clear pattern or trend that signals a growing U.S. rejection of multilateral organizations as venues for the promotion of U.S. foreign policy interests. The United States picks and chooses from a range of possible approaches, depending on the issue, its interests, and changing international and domestic conditions. America can afford to be discriminating in this way. U.S. hegemony affords it broad discretion to use unilateral, bilateral, or multilateral means to obtain its objectives. Hegemony provides it with the privilege of instrumental multilateralism" (pp. 265–66). The editors continue: "These examples suggest that grand generalizations about America's hostility towards multilateral institutions are overdrawn, especially when one looks broadly across issue areas and regions, and over time" (p. 266). With regard to multilateral entities, they are equally blunt: "As for multilateral institutions themselves, they will continue to operate within the direct and indirect constraints that U.S. instrumentalism imposes" (p. 272).

As valuable as this study is, I have two suggestions that, if addressed, would improve its scientific or scholarly value, as well as its potential to inform policy. The first falls within the original terms of reference of the project that culminated in this book, and the second goes beyond the project's initial parameters. With respect to the former, I always find it puzzling when scholars do not use the ideas of other scholars whose work they know and which bear importantly on their subject of inquiry. In this case, the work of W. Michael Reisman could be used to deepen the description and analysis of U.S. behavior toward multilateral organizations and institutions. In the book's opening chapter, the editors cite Reisman's article "The United States and International Institutions" (*Survival* 41 [no. 4, Winter 1999–2000]: 62–80), but they do not comment on his insight that the United States performs a number of different, sometimes conflicting, roles in its relations to multilateral organizations and institutions. Included are what Reisman identifies as a prophetic and reformist role, an infra-organizational role, a custodial role, and a domestic-pressure reactive role. Because much of the scholarly and political interest in and passion about U.S. behavior toward multilateral organizations and institutions arises—whether knowingly or not—from the different roles the United States plays in differing contexts and in conflicting evaluations of the consequences of how the United States carries out these roles, each of the ten case studies would have benefited greatly, in my view, if the authors had clarified which role(s) the United States was performing in the context of their study. Since all of the issues continue to be important, Reisman's concepts could be incorporated in future work by these editors and authors. Of course, if they did not find Reisman's analysis thoughtful or compelling, explaining why they reached this conclusion would be helpful.

My second suggestion focuses even more directly on the possibility that this work will be used to shape policy. Because it has this potential, I would encourage the contributors to use their knowledge explicitly in pertinent policy discussions. I realize that some of them may be doing so in private, but it would be a wonderful addition to the academic literature if they clarified in print their policy preferences for U.S. involvement in those multilateral organizations and institutions they know best. When such preferences indicate a change in policy and/or behavior, contributors could explain the rationale for both the proposed changes and the strategies designed to put them into effect. By taking up these two suggestions, the editors and authors can build constructively on what they have already accomplished.

What Moves Man: The Realist Theory of International Relations and Its Judgment of Human Nature. By Annette Freyberg-Inan. Albany: State University of New York Press, 2004. 266p. $59.50 cloth, $19.95 paper.

— Daniel Nexon, *Georgetown University*

Human nature is the subject of Annette Freyberg-Inan's sustained attack on political realism. She argues that all realist theory shares a common set of assumptions about human motivations. In short, realists believe that humans are, and always will be, up to little good. We are fearful, self-interested, power-hungry, and "by and large, rational" (p. 94). Realists apply these psychological characteristics to nation-states, modifying them only to the extent that states and their leaders are likely to be more rational—in the sense of being strategic and utility maximizing—than average people. Such understandings of human nature are, however, simplistic and misleading.

What troubles Freyberg-Inan is not merely that realists get their facts wrong, but that realism's "prophecy" is self-fulfilling. The more the policies of states are influenced by realism, the more international actors will behave as fearful, self-interested, power-hungry rationalists. If we want to avoid creating the very world realists describe, we must recognize the plurality of motives and means in politics. In her conclusion, she classifies major current theories of international relations according to the basic motives they stress, their "motivational complex," and the main foreign policy goals they isolate. For realists, these are fear, power, and security; for liberals: profit/self-interest, achievement, and prosperity/rights; for constructivists: honor/recognition, affiliation, and identity/membership (p. 163). She calls for an integrative approach, one that stresses "problem solving and the relevance of our research efforts" (p. 171).

If this all seems like familiar terrain, that would be because it is. There is little new in Freyberg-Inan's volume. Its main

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contribution to ongoing debates about realism is that it is a thorough, well written example of synthetic exegesis. The author collects, and provides an often lucid examination of, many of the major arguments about realist microfoundations, their descriptive accuracy, and the possibility that realism might create the very world it claims to describe.

The volume is least persuasive when Freyberg-Inan turns from "classical" to contemporary realism. For the last few decades, realists have eschewed psychological reductionism; they rely on claims about social and political dynamics that are, from the perspective of conceptualizations of human nature, multiply realizable. To assume, for example, that anarchy imposes particular constraints on political communities and their leaders does not require a view of human beings as inherently "selfish schemers, usually wickedly rational, at times dangerously irrational; they are asocial, untrusting, as well as untrustworthy" (p. 95).

Freyberg-Inan argues that the difference between earlier, human-nature realism and contemporary realism is simply a matter of levels of analysis: The psychological dispositions realists once attributed to individuals they now attribute to states. She also suggests that, even if they deny it, contemporary realists still assume that human beings are pretty nasty by nature.

The problems with this sort of argument become very clear if one looks closely at Freyberg-Inan's treatment of Kenneth Waltz. She twice quotes (p. 10 and 73) Waltz as arguing, in the *Man, the State, and War* (1959), that "the root of all evil is man, and thus he himself is the root of the specific evil, war." *This is not Waltz's argument.* He is, in context, describing the tenants of first-image pessimism. Freyberg-Inan does admit that Waltz believes "human nature is too indeterminate to be considered the primary cause of war" (p. 73). I have trouble understanding, therefore, on what grounds she justifies elevating Waltz's discussion of *views he does not share* into an "implicit first-image foundation for systemic international relations theory" (p. 10).

In her treatment of his seminal *Theory of International Politics* (1979), Freyberg-Inan notes Waltz's emphatic rejection of psychological accounts of systemic dynamics as reductionist, yet still maintains that his theory depends upon a putatively realist account of human nature. To this end, she discusses how Waltz sees units as "rational 'unitary actors who, at a minimum, seek their own preservation and, at a maximum, drive for universal domination' " (p. 74). If one skips to the bottom of the same page in *Theory* that Freyberg-Inan quotes, however, one finds Waltz rejecting the assumption of *innate* rationality, pointing out that some states may even choose to amalgamate with other states, and explaining that "the possibility that force may be used by some states to weaken or destroy others does, however, makes it difficult for them to break out of the competitive system" (*Theory,* pp. 118–19). This is an account of structural constraints and the functional requirements of systems, one that is consistent with a fairly broad range of claims about human nature.

Similarly, in Freyberg-Inan's account of the debate between offensive and defensive realism, the relevant disputes seem to have more to do with the imperatives of systemic structure than with essentialist claims about the perniciousness of human nature. Indeed, her major complaint in this context is the "rational actor" assumption frequently deployed in realism. Whatever the psychological merits of rational decision making, it is not, in any way, intrinsically connected to realism. If anything, the assumption of strong rationality is much more closely associated with the liberal tradition, as Freyberg-Inan's own discussion about Machiavelli and Hobbes, and her references to Hirschman, make clear. Regardless, the utility of arguments derived from rational choice analytics is a separate and much broader issue than the one raised by realist conceptions of human nature.

Thus, it would seem that a commitment to realism does not require strongly pessimist ideas about what moves man. In this light, perhaps the more important question raised by Freyberg-Inan is whether claims of human psychology *ought* to be at the very core of international relations theory. Whether or not *What Moves Man* adequately answers that question, it certainly provides a good starting point for addressing it.

Taming the Sovereigns: Institutional Change in International Politics. By K. J. Holsti. New York: Cambridge University Press, 2004. 372p. $70.00 cloth, $25.99 paper.

— John L. Campbell, *Dartmouth College*

For those who want to understand how international political institutions have developed and changed over the last 300 years, this is the book for you. K. J. Holsti begins from the premise that we actually know very little about how these institutions have changed and especially about the degree to which they have done so since the Second World War.

To address the issue, Holsti begins by identifying different types of institutional change, ranging from minor to fundamental, in international politics and by explaining how researchers can better distinguish conceptually and methodologically among them. He argues that all international political institutions consist of three dimensions: patterned practices, ideas and beliefs, and norms. In order to determine the degree to which an institution changes, he argues, we need to track the degree to which each of these dimensions changes over a given period of time. He uses this approach to determine how much change has occurred in four institutions that provide the basic foundation for international politics (i.e., states, sovereignty, territoriality, international law) and four so-called process institutions that underlie and regulate interactions and transactions between international political actors (i.e., diplomacy, international trade, colonialism, war). Each institution receives a chapter-length treatment that describes the essential developments of each dimension from its origins to the present.

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Additionally, Holsti assesses for each institution the various factors that are most responsible for precipitating change, such as depressions, wars, globalization, technological changes, intellectual breakthroughs, or the collapse of empires, including the Soviet Union.

The author finds that no single explanation or description of change fits all eight areas that he examines. Trade has recently become increasingly globalized; territoriality has been transformed (e.g., the European Union); colonialism has become obsolete; and war has reverted in part to pre-Westphalian practices and principles (e.g., terrorism). Nevertheless, he concludes that insofar as trends can be identified, many international institutions have become increasingly complex. That is, the essential practices, ideas, and norms have remained basically the same for a long time even though the activities and actors associated with them have expanded in numbers and tasks, rules have become more elaborate, and the functional scope of these institutions has often increased dramatically. For instance, he argues that the essential elements of the typical Westphalian state have remained in place since the late seventeenth century, although such states have grown in terms of their resources, personnel, military capabilities, and areas of responsibility. As such, he argues that it is a mistake to claim, as many do, that states today are becoming obsolete, withering away, or otherwise being transformed in ways that change their fundamental institutional character. This is not to deny that some states have become weaker or that some states have collapsed. But for the most part, according to Holsti, the basic institutional elements of most states and the international political institutions within which they are embedded have remained remarkably resilient in the face of phenomena that, others have argued, create tremendous pressures for change.

One aim of *Taming the Sovereigns* is to refute those who have argued that late-twentieth-century globalization is fundamentally transforming the institutional basis of international politics. This is an impressive rebuttal, given the wide range of institutions that Holsti tackles. There are no new data here. But the conceptual and methodological apparatus he uses to organize his presentation of the secondary literature brings new sophistication and analytic rigor to the debate—especially insofar as he systematically tracks the same institutional dimensions (practices, ideas, norms) over time in each case to determine what types of change, if any, have occurred.

There are also other reasons why this book is important. In contrast to most realist theories of international relations, Holsti maintains that ideas, such as economic theories and taken-for-granted norms regarding the behavior of states, to mention just two, figure prominently in the development and change of international political institutions. He acknowledges that the material interests of states and the concerns of states with raw political power matter, too. But the book seeks to show that interests and ideas are intimately connected in ways that affect change. Hence, the argument here has implications for the recent literature on "ideas" in politics. What these implications are, however, are not clearly developed. And this illustrates a shortcoming of the book. Let me explain.

I found it frustrating that Holsti failed to offer a coherent *theoretical* view of change in international politics. In fact, the final chapter explicitly refrains from drawing broad theoretical conclusions. On the one hand, I understand that he wants to be cautious and not overgeneralize from his findings, especially because he finds some important differences across cases. But, on the other hand, because he acknowledges throughout the volume the debates over ideas and interests and the debates over globalization, I was expecting him to adjudicate these debates and draw theoretical lessons from his analysis. I hoped, at a minimum, that tracing developments in eight institutional areas over the last three centuries would yield at least some theoretical hunches upon which others might focus later. Instead, we are left with a descriptive, albeit useful, taxonomy of change, rather than a coherent theory of change. Moreover, if in fact the important story that emerges from his analysis is that international political institutions are resilient over time and tend to increase in complexity, then the implications for theories of path-dependent political and institutional change could have been developed. Theoretically informed arguments about path dependence abound in political science (and sociology and economics) these days, but none of them are addressed in the book. In fairness, however, we should remember that Holsti's stated purpose is not so much to engage theoretical debates as it is to determine empirically how much change has occurred or not.

Overall, given its substantive breadth and historical scope, this book would be an excellent text in courses on international politics or globalization. It certainly provides a wonderful introduction to the history of state building and international political institutions. It also shows how international institutions have tamed the behavior of sovereign states. And it provides food for thought for those who might want to consider a range of underlying theoretical implications later.

Governance in a Global Economy: Political Authority in Transition. Edited by Miles Kahler and David A. Lake. Princeton: Princeton University Press, 2003. 472p. $70.00 cloth, $24.95 paper.

— Alexander Thompson, *Ohio State University*

The social scientific study of "globalization" has not been especially productive. Varying and expansive definitions of the phenomenon are used, cause and effect tend to be confused, and positive and normative claims are sometimes mixed. In this volume, Miles Kahler and David A. Lake offer a tractable and fruitful approach to the study of globalization by defining the phenomenon narrowly—in terms of economic integration made possible by reduced barriers

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to exchange and capital mobility—and by adopting a "second image reversed" perspective. Globalization is the independent variable used to explain changes in governance policies and institutions at the national level. This coherent focus allows the individual contributions to speak to each other while building on landmark works, such as Ronald Rogowski's (1989) *Commerce and Coalitions* and Robert Keohane and Helen Milner's (1996) *Internationalization and Domestic Politics*.

Kahler and Lake point to three aspects of governance potentially affected by globalization. First, economic integration may produce changes in the location of governance authority, devolved down to substate actors, delegated up to international institutions, or shifted to private actors. Second, it may engender convergence or divergence of policies and institutions across countries. Third, the degree of accountability of governing agents may change with globalization. These phenomena deserve serious scrutiny, as strong claims are made regarding all three in the globalization literature. As an alternative to functionalist explanations, the editors call on analysts to focus on actors, their strategic interactions, and their institutional environments—in other words, to adopt a more *political* approach.

The volume's contributors adhere to the editors' framework sufficiently to produce interesting comparisons and cumulative findings. Part I asks whether governance authority has shifted away from nation-states as a result of globalization. Analyzing a wide variety of substantive issues, the authors find that globalization does not lead in a straightforward way to either the decentralization or centralization of governance. Outcomes depend on a variety of contextual and intervening variables, including the relevant policy issue, the ways in which specific groups are affected by globalization, and the constraints imposed by existing institutions and path dependence. Chapters by Michael Hiscox, Geoffrey Garrett and Jonathan Rodden, and Pieter van Houten provide an especially coherent dialogue on the conditions under which policymaking is decentralized to regions or remains in the hands of central governments. While authority is shifted to international organizations (IOs) in some situations, Benjamin Cohen and Barry Eichengreen show that supranationalization of governance is often obstructed by political interests and entrenched institutions at the national level. And while globalization shifts some authority to private actors (see the chapters by Virginia Haufler and Walter Mattli), this has not necessarily come at the expense of public authority. Private actors are often constrained by governments and may even strengthen national bureaucracies that interface with them, as Mattli argues.

Part II considers whether the competitive pressures of globalization generate homogeneity in policies and institutions across countries. Ronald Rogowski argues strongly that governments are not converging toward capital-friendly policies; his model overwhelmingly predicts divergence. Peter Gourevitch points to various factors, including prevailing ideas regarding state–market relations, that lead countries facing similar market pressures to exhibit divergent governance outcomes. Even in the highly integrated European context—what should be a likely case for the "race to the bottom" argument—welfare programs remain intact and national differences remain, according to Kathleen MacNamara.

A fairly sanguine portrayal of accountability is offered in Part III. While the European Union may not be as democratic as many would prefer, James Caporaso argues, EU institutions are moving in the direction of more accountability. Robert Keohane and Joseph Nye argue that electoral accountability is not the most appropriate model for assessing global governance; various legal, market-based, reputational, and principal-agent mechanisms help promote IO responsiveness to citizens' needs.

Taken together, the contributions to this volume reveal just how complicated the effects of globalization are on governance. Many intervening variables and the "stickiness" of existing institutions and practices make it difficult to generalize about globalization's impact. This suggests a crucial corrective to the globalization literature: Facile declarations regarding the demise of the nation-state, global homogenization, and supranational bureaucracies run amuck must be tempered, examined in context, and qualified.

The volume also suggests that answers to questions about globalization and governance depend on how—in space and time—individual analysts choose to focus their analytical microscopes. Within a given polity, governance may be centralized for some issues and decentralized for others, as Garrett and Rodden (pp. 87–88) point out. Even within a given issue area, we may find significant centralization overall, only to find decentralization when the issue is disaggregated (see the chapter by Lisa Martin). Similarly, we may see convergence from a macroperspective but divergence as we examine policy choices more closely, as McNamara shows. Time is also a crucial factor. A look at the 1980s reveals primarily intergovernmental efforts to guide corporate responsibility in the area of the environment, while more recently, corporations themselves have taken the lead, according to Haufler. Possibly arbitrary choices about levels of aggregation and time periods may therefore drive our conclusions regarding governance. Reliable generalizations about the location and convergence of governance can only be made if we think carefully about the comparability of findings.

Governance in a Global Economy benefits from ignoring the distinction between international relations and comparative politics. The greatest shortcoming of the "two-level games" research program has been the tendency by individual scholars to focus on one direction of the causal story, ignoring the reciprocal and even simultaneous nature of interactions between levels. Kahler and Lake note that economic integration depends partly on political decisions and institutions (p. 4). They thus understand that their dependent variable, governance, is also a key component of their

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independent variable, globalization. The contributors do not systematically address the endogenous effects of governance changes on globalization (partial exceptions are Hiscox and Beth Simmons and Zachary Elkins). Nevertheless, along with other recent works (e.g., Daniel Drezner, ed., *Locating the Proper Authorities,* 2003), the framework offers the possibility of completing the two-level loop in the study of globalization.

A Theory of Global Capitalism: Production, Class, and State in a Transnational World. By William I. Robinson. Baltimore: Johns Hopkins University Press, 2004. 224p. $46.95 cloth, $18.95 paper.

— Himadeep Muppidi, *Vassar College*

This book intervenes in the contemporary debates on globalization by asking what it means to conceptualize the global economy. Demonstrating the inadequacy of nation-state-based or international-centric responses to this question, it makes a case for conceptualizing globalization as a historically novel form of transnational capitalism. World capitalism, William Robinson argues, has undergone an "epochal change" involving not just a quantitative intensification but also a qualitative reconfiguration of economic, political, and social processes that were hitherto largely international. Taking advantage of technological developments and organizational innovations, capital has liberated itself from the social and political constraints imposed on it by nation-states and reorganized—fragmented and decentralized—the production process on a transnational basis. This reorganization of the productive base has gone hand in hand with the emergence of a transnational capitalist class (TCC), a transnational state apparatus, and a transnational ideological project.

Elaborating on this thesis, Robinson discusses quite insightfully, in Chapters 2 and 3, the analytical boundaries and empirical indicators of the emerging transnational capitalist class and the transnational state apparatus. He conceptualizes the transnational bourgeoisie as the class that owns "the major productive resources of the world" (p. 47) and is "involved in globalized production and manage-[ment] [of the] globalized circuits of accumulation" (p. 100). Though comprised of capitalists from different parts of the world, the TCC is presented as increasingly detached from nation-state-oriented political and social projects. But the TCC's detachment from national political and social projects does not necessarily imply a lack of access to the authority of the state form itself. The TCC asserts its political authority globally through a historically new transnational state form—a "multilayered" and "multicentered" network of transformed national states, supranational institutions, and global economic and political forums. The ideological agenda that the TCC seeks to institutionalize through the transnational state apparatus involves, among other things, the spread of neoliberal economies and polyarchic polities. It is these that allow the world to be both made "available to capital" and also "safe for capital" (p. 81).

This book fulfills its major goal of offering a succinct and accessible theory of globalization based on a distinctive "global capitalism" approach. Starting from the centrality of the organization of the social relations of production, Robinson cogently develops and describes the social and political implications that flow from a historic change in these relations. Drawing upon, but also carefully differentiating his theory from, other closely related ones—most prominently those of world-systems theory and neo-Gramscian international relations—he consistently foregrounds the transnational or the global dimension of globalization. This allows him to connect insightfully the different changes in the global economy to changes in global society and global governance.

Notwithstanding these strengths, the capacity of *A Theory of Global Capitalism* to force the reader to rigorously theorize the global is weakest on the dimension of the cultural. Robinson's theory is nuanced enough to acknowledge the importance of culture (e.g., pp. 83–84). But his conceptualization of the cultural does not approach it in its historically and geographically global dimension: as the culture of capitalist and colonial modernity. In seeing culture primarily as a "component of the transnational project," Robinson reduces it to an agglomeration of "consumerism, individualism and competition" (p. 84). This is a puzzling limitation in a book that is in conversation with some of the foremost historical and contemporary theorists of modernity (Max Weber, Karl Marx, David Harvey, and John Tomlinson).

A theory of globalization that neglects the theme of capitalist and colonial modernity cannot account adequately for many crucial features of the contemporary global economy. I will limit myself to two that are of immediate relevance: the rapid spread of outsourcing and the consensual dimension of hegemony.

Explaining the change from Fordist to flexible regimes of accumulation, Robinson traces it to capital's application of new technologies and organizational innovations such as outsourcing (pp. 16–22). What facilitates this application, according to the author, is the emergence of a new relationship between capital and labor. But the "historical analysis" that he offers does not really explain what makes this new relationship possible: What is the source of capital's increased social power over labor? Ruling out any "technological determinism" to explain this critical change, Robinson focuses on the competitive drive and class struggle embedded in capital itself. It is this drive that forces capital to constantly look for new ways to increase profits by lowering its costs. But this resolution only substitutes the "determinism of capital" for that of technology. Moreover, we still do not know why outsourcing takes the specific transnational form that it does. That is, how is it that one of the most advanced sectors in the global economy is able to find its most skilled

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and technologically competent (not just inexpensive) workers from the poorer and "developing" social formations in the world? The theory of globalization that Robinson offers cannot grasp the dynamics of colonial and capitalist modernity that lead to the mass production of skilled software engineers in Third World countries in quality and quantity sufficient to make capital's relative power over national labor possible. It is in this realm that an attention to the cultural and thus necessarily to the heterogeneity and difference of the Other can be analytically productive in explaining globalization.

Additionally, and more briefly, an adequate conceptualization of the cultural would enrich the relatively underspecified dimension of ideological hegemony in the author's analysis. Although Robinson recognizes that hegemony involves a coercive and consensual element, he relies on asserting rather than demonstrating the "hegemonic" nature of the TCC. He is convincing when he argues that hegemony is in the interests of the TCC. But he is not very compelling in showing how these particular interests of the TCC are made universal and persuasive to various subordinate others. A richer conceptualization of the cultural as the modern, in its multifarious forms, would make for a stronger theory of globalization.

The Ties That Divide: Ethnic Politics, Foreign Policy and International Conflict. By Stephen M. Saideman. New York: Columbia University Press, 2001. 348p. $70.00 cloth, $23.00 paper.

The Geography of Ethnic Violence: Identity, Interests, and the Indivisibility of Territory. By Monica Duffy Toft. Princeton: Princeton University Press, 2003. 256p. $37.50.

— Stuart J. Kaufman, *University of Delaware*

These two useful books represent important contributions to the literature on violent ethnic conflicts. Monica Duffy Toft proposes a useful theory and adduces convincing evidence on some of the key determinants of severe ethnic violence. Stephen Saideman provides an even stronger theory and strongly suggestive evidence for explaining states' decisions to intervene in ethnic conflicts over secession. Both books provide a mix of quantitative analysis and case studies to support their claims more or less convincingly. Both conclusions deserve prominent attention in the literature.

As Toft summarizes her central theme in *The Geography of Ethnic Violence,* different actors view the same territory in different ways. The outbreak of ethnic war, in her view, is explained by the theory of indivisible territory: If both sides in a conflict see control over a disputed territory as indivisible, they are likely to fight over it (pp. 1–2). More specifically, ethnic groups are most likely to demand sovereignty over their territory if they are a concentrated majority, representing the majority of the population in what they see as their homeland. States are most likely to resist such demands if, as is usually the case, they are multiethnic and fear setting a precedent for other potentially secessionist groups.

Toft's reasoning is sensible: Concentrated majority groups, because they are the majority, have a claim to democratic legitimacy for their demands, and at the same time are most likely to have the capability to rebel. Her statistical data provide some eye-catching support for these contentions. In particular, she finds that concentrated minority groups, representing 48% of all minorities at risk, account for 78% of ethnic rebellions, and that 63% of concentrated majorities engage in at least some ethnic violence. Most of the rest of the ethnic rebellions are accounted for by concentrated minorities.

While Toft's theory makes sense, the value of the book is weakened by the straw-man nature of the alternative arguments she considers: the materialist explanation (a simplistic conflictual modernization model); the nonmaterialist explanation (a simplistic security dilemma model); and the elite manipulation explanation. She would have done better to test more sophisticated versions of both of the first two approaches that are put forward in different parts of Donald Horowitz's 1985 classic *Ethnic Groups in Conflict*: the psychological dynamic model he proposes early in the book, and his advanced-backward distinction among groups and regions. This would have added nuance both to the theory testing and to Toft's use of her own theory.

The strength of Toft's theoretical approach is that it joins constructivist logic about the definition of group identity with rationalist analysis of the interaction of the consequent group preferences. However, virtually all of the analytical leverage comes from the constructivist logic: If both sides in a dispute inflexibly demand full control over all of a disputed piece of territory, it hardly needs a formal model to show that they will come into conflict. This fact turns up the theoretical weakness of the work: The theory gives too little weight to identity construction, leading Toft to overemphasize materialist-rationalist factors, especially demography, in her case analyses, while the evidence points to the importance of identity construction.

In the case of Georgia's conflict with secessionist Abkhazia, for example, Toft argues that because they were a local minority, the Abkhazians did not at first define their demands as indivisible, and that this changed only after the war began. Her own evidence, however, shows that Abkhazia was inflexibly demanding complete independence before the war. The constructivist part of her theory explains this behavior perfectly: The Abkhazians' notion of homeland driven largely by fears of Georgian domination motivated them to make this demand not in spite of but because of their demographic weakness. But this is not the argument Toft makes. Similarly, in the Tatarstan case, she overemphasizes the role played by the Tatars' not-quite-majority demographic status (48.5% in Tatarstan), while underrating the fact that nearly half of all families in Tatarstan were ethnically mixed. Here was the real demographic factor for ethnic moderation in Tatarstan (driven, in turn, by the high degree of

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ethnic toleration that allowed for such high rates of exogamy in the first place).

Toft's statistical case is actually in some ways stronger than she makes it. Being a concentrated group is, as she notes, very nearly a necessary condition for an ethnic rebellion. The only exceptions are Lebanon's Sunnis and Rwanda's Tutsis, however, these exceptions prove the rule, as Lebanon's Sunnis were dragged into a war started by others, and Rwanda's Tutsis started with a base of support in neighboring Uganda. What Toft neglects to clarify is that the demographic factor is not a sufficient condition for violence: Only 25% of concentrated majorities rebel. The rest of the variance is accounted for by other variables.

The strengths of *The Ties That Divide* are the converse of Toft's work, as the book is characterized by excellent theoretical argumentation and case studies but weaker statistical data. Saideman's theoretical argument is simple but sound: State leaders tend to back foreign ethnic groups with which their own constituents sympathize, especially when the leaders feel their position is threatened (and so feel a need to bolster their base of support).

The alternative theories Saideman considers are the prominent ones in the field, and they perform surprisingly badly. He drives another nail in the coffin of the old conventional wisdom already convincingly attacked by Alexis Heraclides' claiming that states that are vulnerable to secessionists tend not to support other's secessionists. He also convincingly debunks a realism-based model suggesting that states might tend to support secessionists in states that threaten them.

Rather, his case studies show that quite consistently, states choose their policies on ethnic or racial grounds. In the Congo-Katanga crisis of the early 1960s, for example, black African states backed the African nationalist government of Congo, while minority white regimes in Africa, along with colonial power Belgium, supported the pro-white secessionist Katangan leadership. In the case of the Nigeria-Biafra conflict, the main line of cleavage was religious, and so Nigeria's government, dominated by the Muslim Hausa-Fulani group, garnered support from Cameroon (with a strong Fulani constituency), Niger (which was 46% Hausa), and the Muslims of the Arab Middle East.

Eschewing a simpleminded clash-of-civilizations argument, however, Saideman also shows that some cases that might appear to violate ethnic logic actually do not do so. Thus, in the Yugoslavia case, Orthodox Greece opposed Orthodox Macedonia because Greek nationalist ideals objected to the Macedonians' appropriation of the name Macedonia. Some of his arguments are a bit strained: Christian Ethiopia's backing of Muslim Nigeria to promote civic nationalism, for example, sounds more like the vulnerability argument than the ethnic one. Still, all in all, his argument holds up far better than the alternatives.

There are two objections, significant but not decisive, that could be raised about the research design. First, the Congo case is not as crucial a case as Saideman claims. Because the key cleavage was racial, it was less precedent-setting than it might have been; white racial domination was already a cause in terminal worldwide decline. Still, if not a pivotal case, it is still an important one, not least because proponents of other theories claim it as a source of supporting evidence.

More important is the design of the quantitative part of the book. The first portion of the quantitative analysis simply sums up all of the observations from the three case chapters: useful, but not, as Saideman concedes, anything like a representative sample. The second portion, while using the large Minorities at Risk (MAR) data set, focuses on the wrong unit of analysis. Although Saideman's logic, as he concedes, is dyadic, the statistical analysis asks what factors lead groups to gain support in general, or states to offer it in general, rather than looking for ethnic links between patrons and clients. He pleads overload, as there are tens of thousands of potential state-group dyads derivable from the MAR data set. This is a fair point, but a smaller data set consisting of a sample of politically relevant dyads would still have allowed him to generate a more convincing set of tests. As it is, the value of his statistical tests is (as he concedes) mostly to provide more evidence to undercut the rival hypotheses, rather than to support his own.

These caveats aside, both books are valuable and make important contributions to the literature. Both authors sustain their main cases. Toft is clearly right that demographic patterns have an important effect on the likelihood of ethnic violence, as does the way nationalist ideology defines a group's relationship to its homeland. Saideman is clearly right that ethnic ties strongly influence states' policies toward ethnic separatism. Anyone interested in the causes of, or international relations of, ethnic civil wars should read both books.

Both also have notable policy implications, but primarily negative ones. Toft's offers further evidence for why ethnic civil wars are so intractable. Saideman's points out that while prescriptive arguments about international intervention tend to assume that the international community can and will act in concert, states rarely do so because their domestic politics often drives them to back different sides. These cautionary notes are already well known to policymakers, but they are important for any theorists who might want to venture into the realm of policy prescription.

A New World Order. By Anne-Marie Slaughter. Princeton: Princeton University Press. 2004. 368p. $29.95.

— Nazli Choucri, *MIT*

This book puts forth a bold vision for global governance, based on a simple but powerful argument. The argument is this: "Networks of government officials . . . are a key feature of world order in the twenty-first century, but they are underappreciated, undersupported, and underused . . ." (p. 1). Central to this vision are the concepts of network and governance. Anne Marie Slaughter is careful to define each of these concepts and, in so doing, calls attention to the fundamental theoretical, empirical, and pragmatic challenges inherent in transforming this vision into some semblance of reality.

The book introduces the argument with a brief discussion of "the globalization paradox," by which the author refers to the expansion of government structures and functions, nationally and internationally, on the one hand, and the attendant fears and concerns that invariably accompany this expansion, on the other. This duality is one of the many paradoxes that are generated by the sustained power of the state in world politics and the persistence of invasive globalization processes. The argument is that the tensions created by this paradox can be reduced, if not resolved, by greater understanding, appreciation, and development of government networks operating both within and across national boundaries. The author selects three types of network functions—those pertaining to information, enforcement, and harmonization—and examines in considerable detail the networks of regulators, the world of courts and court systems, and the networks of legislators worldwide.

The first chapters are devoted to a careful description of each of these networks, noting their characteristic features, core functions, and operational manifestations. By distinguishing between vertical and horizontal networks, the author reinforces the overall strategy toward the globalization paradox by highlighting the architecture for this vision of the new world order in terms of breadth (across states) as well as depth (within states). This is an important exercise in that it provides a rather inclusive view of existing governmental networks across domains of governance and jurisdictions. Presented as a description of the world as it is today, this exercise underscores the author's view of the state and of the state system in disaggregated, rather than the more conventional, unitary terms. This disaggregation however, is defined almost exclusively in terms of governmental structures and functions with special focus on the purposive networks that emerge as a result. While essential to the author's vision, if taken too literally this disaggregation itself may harbor some serious theoretical, empirical, and normative challenges that, if unheeded, may undermine the validity of the overall vision. To be fair, there is every indication that the author appreciates these challenges and, to some extent, is deft in recognizing their implications and addressing them head on.

The notion of networks is applied across internal, external, transnational, and organizational boundaries, creating a vision of a world that is dense in networks of governance. This density is seldom appreciated by scholars or by practitioners in international relations. In many ways this analysis amounts to something of a census of official and quasi-official global networks, with formal structures and functions, and relatively well defined parameters of purposive behaviors. Excluded from this census are informal, emergent, or self-organizing networks that may be devoid of formal structures and functions but characterized by common, shared and purposive actions. Noting this exclusion is not meant as a criticism of the vision or the argument, rather it highlights clear boundaries in the author's terms of reference.

A New World Order is based on the view that the "state is not disappearing; it is disaggregating." However, only in the world of formal international relations is the state seen as a unitary entity. Few scholars, analysts, or practitioners of international relations would seriously support the unitary vision. But, since the state is the only entity enfranchised to speak on behalf of individuals in the international context, the unitary perspective remains robust for purposes of governance at any level of analysis—national, international, or global.

What is important and distinctive about the view of the state in the book is the organizing principle—or the meta-fault line—of this dissaggregation. The subject is governance (structures, functions, purposes, and performance) and, by definition, formally recognized as such by all relevant entities in question. Again, the terms of reference are clear: these do not focus on informal networks, organizations, or mechanisms, "underground" or competing governance structures, or any institutional and networking principles that may be rooted in principles other than those recognized in conventional modern, Western, political discourse.

In this connection, however, one of the significant contributions of the book is that it provides the foundation for framing basic and applied research to address issues at the frontier of this bold vision anchored in governance and in networks. Among the interesting research questions in the governance domain, for example, are the following: To what extent do the advances in information technology that are deployed in virtual domains (emerging cyberspaces) influence the operations of established networks of governance? Is the practice and promise of e-governance similar to that of "real" or material or physical governance? Does the diffusion of e-capabilities facilitate or impede the development of trajectories toward this vision of a new world order? What meta-principles of governance can provide the "best" guidance for mechanisms to reduce disconnects in the design and organization of basic information relevant to governance at different levels of socio-political or economic aggregation?

In the domain of networks, some issues on the theory side are particularly compelling, and must be addressed in order to gain a deeper understanding of the full implications of this book. For example, given the ubiquity of networks and the wide range of network theory (or theories) put forth in different disciplines, what particular theory of networking might be most relevant to the international order? Given the diversity of intellectual perspectives, what are the most promising approaches for research on governance networks to help support a new world order? Even more fundamental, of course, is how a more fully articulated network theory of governance might facilitate the transition from framing a bold vision to addressing the complex challenges of operation and implementation. Further research aside, however, *A New World Order* is an important volume.

Europe's Foreign and Security Policy: The Institutionalization of Cooperation. By Michael E. Smith. New York: Cambridge University Press, 2004. 308p. $80.00 cloth, $28.99 paper.

— Roy H. Ginsberg, *Skidmore College*

Michael Smith asks and answers a question about the foreign policy of the European Union: How do we explain its surprising growth and development over the past 30 years despite the many obstacles? The question is timely for scholars and practitioners. For practitioners, the EU is beginning to matter more in international politics as it finally begins to operationalize the European Security and Defense Policy (ESDP) in the Balkans and Africa. For scholars, a theory of European foreign policy has not enjoyed the attention paid by theorists to internal economic integration (neofunctionalism), interstate bargains struck at intergovernmental conferences (realism, liberal intergovernmentalism), and the impact of ideas, preferences, identities, and interests (constructivism) that influence institutions.

Dissatisfied with mainstream theories of international cooperation that he argues are too narrow or simplistic to capture the impact of institutions on cooperation, Smith applies a theory of institutions to explain the evolution of European foreign policy cooperation among the EU member states. He is intrigued by the cumulative relationship between institutions and cooperation. European foreign policy cooperation began in 1970 as a loose, informal, non–treaty-based foreign policy consultative mechanism designed to keep international political issues from dividing the member states or harming the European Community. Over the next 23 years, European political cooperation became increasingly institutionalized and grew into the Common Foreign and Security Policy (CFSP) established in 1993 when the Treaty of Maastricht created the European Union. CFSP is a formal, high-profile, treaty-based process by which the EU and its member states seek to develop foreign policy actions and positions in order to play a proactive role in world affairs and to contribute to international peace and security. The evolution from reactive to active to proactive foreign policy cooperation is of keen interest to Michael Smith and other students of the role of the EU in the world.

For Smith, institutions matter. He is interested in understanding why, over time, European foreign policy became institutionalized and why it has developed its own internal momentum. He is equally interested in how institutionalization promoted European foreign policy cooperation; how the EU developed a sustained track record of foreign policy cooperation; and how CFSP has grown to become greater than the sum of its parts. He prefers a theory of institutionalism to other theories because institutionalization is a nonstatic process by which norms, or shared standards of behavior, are created and developed over time. Institutionalization involves the reciprocal, circular, and dynamic links between institutional development and the propensity of states to cooperate to achieve joint gains. Institutionalization, Smith writes, encourages actors to build institutions that foster cooperative outcomes that later influence the process of institution building.

The thesis of the book is that European foreign policy cooperation benefits those for whom it was originally intended: the EU member states. European integration is an ongoing discourse about how institutions translate common values or aspirations into specific collective policies or behaviors through application of norms and rules. CFSP has advanced, he argues, in a cycle involving crisis or opportunity, small-scale innovation, and institutional codification until the sequence repeated itself and eventually took foreign policy cooperation into new directions. Institutions filter which internal/external demands are focused on the EU; they make collective behavior stable over time and help condition state interests. As institutions develop, they generally make it easier for states to reach decisions and make judgments about the scope, demands, ends, duration, effectiveness, and desirability of cooperation. Smith argues that institutionalization and cooperation are related dynamic processes. State preferences are altered by institutionalized interactions with other states, meaning that domestic and international politics are linked in complex ways.

The author operationalizes his theory of institutions by examining the historical development of European foreign policy in stages over the past 30 years. These stages include information sharing, norms creation and codification, and actual governance—the authority to make, implement, and enforce rules. Relations among EU states progressed from narrow instrumental rationality characterized by intergovernmentalism to a more collective rationality characterized by legitimate procedures of governance and corresponding changes in domestic politics. He opines that since the EU is the most densely institutionalized network of states ever devised in world politics, a theory of institutions is appropriate in explaining how cooperation and institutions are related to one another. Smith concludes that there is a future for CFSP: He predicts reproduction and incremental adaptation, rather than rollback.

The author sets out to achieve a better understanding of the interplay between the EU's higher-profile foreign policies and its more significant institutional elements. Does he succeed? He does. He provides a fluid read that avoids excessive jargon and is accessible to both specialist and generalist. However, no book is flawless. Smith could have more extensively weaved into the text the results of the elite interviews of an impressive 60 EU officials and documented these primary sources with more specificity, given their importance to the volume and its arguments. The text is more an historical study than a contemporary one. Since there was a lag in time between writing and publishing, the

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events of 9/11, the war in Afghanistan, the intergovernmental conference for a new EU constitution, and the fast-breaking developments in the operationalization of ESDP are not given the full attention needed. Although *Europe's Foreign and Security Policy* was primarily designed to examine the process of institutionalization, the author might have drawn more extensively from other works that have focused on assessing the outcomes of European foreign policy actions, given how critical outcomes are as feedback for further institutionalization.

Quibbles aside, Smith's important work deserves to be included in the canon of European foreign policy theoretical texts. It nicely rounds out our theoretical knowledge of the role of institutions in European foreign policy cooperation and is strongly recommended to those, like Smith, who are intrigued by the unprecedented degree of foreign policy cooperation among the 25 members of the European Union.

The Impact of Public Opinion on U.S. Foreign Policy Since Vietnam. By Richard Sobel. New York: Oxford University Press, 2001. 288p. $24.95.

International Public Opinion and the Bosnia Crisis. Edited by Richard Sobel and Eric Shiraev. Lanham, MD: Lexington Books, 2003. 344p. $80.00 cloth, $25.95 paper.

— Doris A. Graber, *University of Illinois, Chicago*

In 2001, Richard Sobel published four case studies chronicling the impact of public opinion on U.S. foreign policy since the start of the Vietnam War. He used public opinion surveys and interviews with senior policymakers, including three secretaries of state and four secretaries of defense, to document how opinions fluctuated throughout each crisis and how various movers and shakers felt about the weight that they should assign to public opinion in their deliberations. The crises—the Vietnam War, the Nicaraguan Contra-funding controversy, the Persian Gulf War, and the Bosnia crisis—demonstrate that public opinion did constrain policy options, but did not determine the specific policies that were chosen. This finding confirms V. O. Key's theory, first expressed in his pathbreaking study *Public Opinion and American Democracy* (1961) that public opinion operates like a system of dikes. These dikes limit how far policymakers can go in committing the country to actions in the policy sphere.

Looking at public opinion data through the eyes of the very people who determine how much weight they will give to it in framing and defending their policy decisions provides a highly useful insider perspective on policymaking considerations. Nonetheless, one may question whether public statements and retrospective interviews truly capture the thoughts of publicity-conscious public figures. That concern is especially troubling in this case because many of the interviews were conducted in public settings during prominent news programs, rather than by the author in a more private, reflection-inspiring environment. Examining the actual performance of leaders, which Sobel does as well, may be a far better guide to what actually happened.

In addition to providing an interesting record about policy formulation, Sobel also uses the cases to test prevailing theories about the role of public opinion. As Ben Franklin said centuries ago in *Poor Richard's Almanac,* "There's many a slip twixt cup and lip." Theories are one thing, practice is another, especially when it comes to democratic theories that postulate that in a democracy, vox populi, vox Dei—the voice of the people is the voice of God. Accordingly, the theory that emerges from the findings of Sobel's 2001 book, reinforced by its 2003 companion volume, is designed to be realistic. Sobel and Eric Shiraev, his later coauthor, label it as a "multi-axial" approach to indicate that complex political phenomena, like the impact of various public opinions on a variety of policies, must be examined from multiple perspectives. Each of these perspectives is composed of many testable variables that must be scrutinized in light of the specific socioeconomic, political, cultural, and psychological conditions prevailing at the time and place.

Sobel and Shiraev replicate the case-study-based approach to public-opinion impact research in their edited volume, *International Public Opinion and the Bosnia Crisis.* That book is unique because it tracks the public-opinion aspects of the crisis in eight countries: the United States and Canada, Britain, France, Germany, the Netherlands and Italy, and Russia. Each country chapter is written by experts familiar with the particular country's politics. The broad sweep through the political cultures of eight diverse countries allows for genuine comparisons. It also sheds fresh light on the validity of various prevailing theories, and permits the development of a complex theoretical framework that the editors explain in their coauthored introductory and concluding chapters in the volume.

For example, Sobel and Shiraev present a framework for assessing policy climate and testing the impact of climate factors on the interplay between public opinion and public policy. The main factors that create the policy climate are 1) the nature and functions of each country's political institutions and 2) the political status quo as it relates to a particular foreign policy. That includes the distribution of policy preferences among the public, as well as the bargaining strategies of national decision makers. As a third factor, the editors mention 3) the dominant value structures that shape the policy climate. For instance, Canadians, deeply concerned about humanitarian values, favored intervention in Bosnia from the start. By contrast, their Russian counterparts were initially neutral but then became aggressively negative, largely because of increasingly anti-American and anti-NATO sentiments. The fourth climate factor relates to 4) the contemporaneous framing of the foreign policy issue, which depends heavily on the nature of mass media coverage. Differences in these four factors explain differences in public opinion climates.

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To elaborate a bit more on just one of these factors, when it comes to political institutions, public opinion has more impact in two-party than in multiparty parliamentary systems, probably because one can rarely talk about genuine majority opinions in multiparty systems. The respect for public opinion shown by political parties and interest groups also determines its influence, as does the degree of consensus among elites and between elites and the public. Unstable political parties are unlikely to recommend forceful foreign policy actions. Opinion guidance is strongest in presidential systems, especially when presidents happen to be popular.

As in Sobel's earlier book, he and Shiraev conclude from the comparisons of policy developments in multiple nations that there is substantial correspondence between policies and public opinions and a reluctance by policymakers to defy an overwhelming public consensus. Public opinion appears to be relatively stable everywhere, and people are universally reluctant to risk casualties. The precise impact of public opinion on policy depends on the context and on mediating variables, such as the effectiveness of networking among elites, elite awareness of public opinion, and the nature of the policy proposals. Military elites are far more likely to oppose military ventures than is true of civilian elites.

What major benefits flow from the comparative findings? One is the important realization that the term "public opinion" is interpreted in different ways, depending on each country's ideological and political context. Pollsters vary cross-culturally in the kinds of questions they ask about public policies and in the way they assess the strength of feelings and beliefs that support particular opinions. These differences have to be kept in mind when comparing reports about opinions held in different countries and at different times.

Similarly, policymakers have different motivations in heeding or ignoring public opinion. They may base their actions on ideological concerns, including the belief that democratic norms mandate attention to public opinion, or they may act on practical considerations that require evaluating many factors unrelated to the public's views. If practical considerations are paramount, policymakers who feel insulated from the wrath of the public may feel free to totally ignore it. If the public seems disinterested, which is often the case when foreign policy issues are at stake, governments everywhere are quite free in choosing the thrust of policies.

While most of Sobel and Shiraev's findings accord with familiar knowledge about public opinion, their contention that policy is more likely to change in response to a change in public opinion than vice versa is controversial. As an example, they point to the fact that public sentiment in various countries favored intervention in Bosnia long before their reluctant governments changed their minds. But that fails to acknowledge the well-documented "fait accompli" effect that suggests that publics will approve a policy they rejected earlier, once that policy has been adopted. The reversal helps people avoid the mental discomfort of having to live with a disliked policy. The crucial factors that explain whether publics or policymakers are more willing to change their views relate to the strength of the respective convictions and to the political circumstances at the time when decisions must be made.

Overall, the greatest benefit of this edited book for scholars, practitioners, and attentive publics is its usefulness for making well-considered predictions about the likely impact of public opinions on particular foreign policies. That usefulness will be enhanced if other scholars follow the patterns set out in these two books of examining multiple cases in a cross section of countries, using the same analysis and comparison criteria. Cross-cultural generalizations based on a single case, like Bosnia, regardless of the number of iterations, are bound to be shaky. Using the edited book as an analysis tool, policymakers need not even wait to see whether their predictions turn out to be accurate. They can use the criteria laid out by Sobel and Shiraev to guide the policy context in desired directions. Whether such public-opinion manipulations are a good or bad thing in specific cases is controversial, of course. The tools that scholars provide to policymakers are always double-edged, capable of doing good as well as producing harm.

Trade Threats, Trade Wars: Bargaining, Retaliation, and American Coercive Diplomacy. By Ka Zeng. Ann Arbor: University of Michigan Press, 2004. 324p. $57.50.

— I. M. (Mac) Destler, *Maryland School of Public Affairs*

This book examines bilateral U.S. trade diplomacy during the 1980s and early 1990s. Ka Zeng advances (and generally sustains) an important thesis: that in trade disputes, U.S. policy has been tougher and more effective toward advanced industrial democracies (Europe, Japan) than nondemocratic nations (esp. China). The reason, she argues, lies in the structure of the economic relationships. Trade between the United States and the democracies is largely "competitive," with industries and sectors going head-to-head for markets. The trade pattern with autocracies (and less-advanced economies like India and Brazil) is more "complementary," with imports from these countries largely in product areas U.S. producers have abandoned.

To test this thesis, Zeng examines market access cases brought under Section 301 of the Trade Act of 1974, as amended. Drawing substantially on ratings provided by Thomas O. Bayard and Kimberly Ann Elliott (*Reciprocity and Retaliation in U.S. Trade Policy,* 1994), she finds that among nine major trading partners, U.S. negotiators had the greatest success with Japan and the least success with India. Overall, responsiveness to U.S. pressure was greater in the nations that have the more competitive trade

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structures—Canada, the European Union, Japan, Taiwan, and South Korea, in descending order of competitiveness.

This linkage of trade conflict with trade structure is an important finding, backed by a domestic political explanation. In cases where a trade relationship is competitive, U.S. domestic interests will reinforce one another: In the Japanese semiconductor case, for example, companies threatened by Japanese competition at home backed the threat of sanctions aimed at securing greater access to Tokyo's market. If trade relations are mainly complementary, as with China, Americans benefiting from imports will be a more important relative force and will oppose actual implementation of sanctions, thus undercutting U.S. negotiators.

Zeng supplements her statistical analysis with a "structured, focused comparison" (per Alexander George) of U.S. trade bargaining with Japan and China. On the former, she examines the bitter semiconductor conflict of the mid-1980s (involving U.S. sanctions) and three so-called Super-301 cases initiated by the George H. W. Bush administration. In all four, she finds that U.S. negotiators achieved much of what they sought. She applies her framework effectively and tells the conflict stories credibly—though without the deep understanding of U.S. politics exhibited by John Kunkel in *America's Trade Policy Towards Japan* (2003).

But staying within the boundaries of the cases has costs. The reader will *not* learn here that United States Trade Representative Carla Hills's implementation of the "Super 301" law in 1989–90 was much softer than the language Congress enacted in 1988 seemed to demand. She named Japan as a "priority foreign country," not for the "number and pervasiveness" of its "acts, policies, and practices" that impeded U.S. exports (Section 1302, Public Law 100-418), but for restrictive practices in just three product areas. Nor will the reader learn that the economic importance of Dynamic Random Access Memory (commodity) semiconductors was wildly exaggerated by both countries in the 1980s; in fact, their cheapness, wherever produced, fueled the U.S. ability, in the nineties, to exploit its competitive strengths in more sophisticated products and systems, and enhance productivity economy-wide.

The China chapters, by contrast, show sophistication in recognizing the broader context (geostrategic, in particular), together with the fact that a primary U.S. "threat"—withdrawing most favored nation trading status—was too great a sanction to be credible. Still, the author might have explored other explanations for differences in U.S. outcomes with China and Japan, such as Tokyo's peculiar dependence on external pressure, or "gaiatsu," to achieve internal policy change (Leonard J. Schoppa, *Bargaining With Japan,* 1997). This sometimes made reform-minded Japanese officials *want* to yield to U.S. pressure. (She recognizes, in her conclusions, that her framework focuses mainly on the politics of the "sender of threats," not the "target" [p. 241].)

The weakest element of the book is Zeng's effort to draw parallels between the "democratic peace" literature and so-called trade wars. The apparent paradox was obviously tempting (democracies fight *mainly* one another on trade), and one can only applaud, in principle, any attempt to build analytical bridges across the security–economic policy divide. Moreover, the author carefully limits her "trade wars" to a small number (10) of cases featuring trade intervention by both sides, and she shows good understanding of the democratic peace debate. But her claim that these "wars" are comparable to military conflict is simply not persuasive. For example, it strains credibility to argue that because an agricultural subsidy "war" cost "the United States and the European Community . . . approximately $2.5 billion over three years," the two "should have as strong an incentive . . . to avoid trade wars as to avoid military ones" (p. 10).

Not only were the stakes in late-twentieth-century trade wars minuscule compared to those in (now mercifully unthinkable) military conflicts between the same adversaries, but these trade fights also took place within a broad cooperative context—a global trade regime featuring low and declining import barriers—which these democracies were themselves constructing even as they bickered over specifics. In fact, the very creation of the GATT–World Trade Organization regime by countries with competitive trade structures seems an anomaly within Zeng's framework. She might well address it in future research.

The WTO has also changed the nature of trade conflicts, as the author recognizes in her concluding chapter. Before 1995, the United States was relatively free to impose trade sanctions; now, doing so invites the target nation to launch a challenge before that organization's Dispute Settlement Body. This process has taken the teeth out of Section 301, and as long as it remains in effect, unilateral American trade diplomacy will not be as aggressive as it was in the period she treats.

But bilateral trade conflicts remain with us, and so Zeng's analysis remains relevant. The central distinction between competitive and complementary trade relationships is an important contribution to our understanding of trade diplomacy. And if her reach occasionally exceeds her grasp, Zeng's overall analytic sophistication makes *Trade Threats, Trade Wars* an important contribution to the literature of trade policy.

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